

NOTICES
OF THE
PROCEEDINGS
AT THE
MEETINGS OF THE MEMBERS
OF THE
Royal Institution of Great Britain
WITH
ABSTRACTS OF THE DISCOURSES
DELIVERED AT
THE EVENING MEETINGS

VOLUME XIX
1908—1910



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Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 17, 1908.

GEORGE MATTHEY, Esq., F.R.S., Manager, in the Chair.

Professor T. E. THORPE, C.B. PH.D. LL.D. D.Sc. F.R.S. *M.R.I.*

The Centenary of Davy's Discovery of the Metals of the Alkalies.

A HUNDRED years ago last October, there happened one of those events to which the term epoch-making may, without cavil or question, be fittingly applied.

As it was an occurrence with which the name and fame of the Royal Institution are inseparably bound up, the Managers have thought it only proper that its centenary should not pass unnoticed here, and it is by their wish, therefore, that I appear on this the first possible opportunity after the actual date of its hundredth anniversary to give you some account of it, and to state, so far as I am able and within the limits of an hour, the fruitful consequences that have flowed from it.

Let me, in the first place, attempt to recall the circumstances which led up to that cardinal discovery of which to-night we celebrate the centenary. These are connected partly with the Institution itself, and partly with the state of science in the early years of the 19th century.

In the year 1807 this Institution was entering upon the eighth year of its existence. As you doubtless know, the Royal Institution grew out of a proposal to deal with the question of the unemployed—namely, by forming in London by private subscription an establishment for feeding the poor and giving them useful employment, and also for furnishing food at a cheap rate to others who may stand in need of such assistance, connected with an institution for introducing and bringing forward into general use new inventions and improvements, particularly such as relate to the management of heat and the saving of fuel, and to various other mechanical contrivances by which domestic comfort and economy may be promoted. Such was the original prospectus, but, like many other prospectuses, it failed to equal the promise its projectors held out.

Eventually the promoters decided, on the initiation of Count Rumford, that the Associated Institution would, as they expressed it, be “too conspicuous and too interesting and important, to be made an *appendix* to any other existing establishment,” and therefore it ought to stand alone, on its own proper basis.



Accordingly the problem of the unemployed still remains with us, whilst the new institution took the form of converting Mr. Mellish's house in Albemarle Street into a place where, by regular courses of philosophical lectures and experiments, the applications of the new discoveries in science to the improvement of the arts and manufactures might be taught, so as to facilitate the means of procuring the comforts and conveniences of life.

The Royal Institution had a troubled infancy. Like the poor it was originally designed to succour, it suffered much in the outset from lack of nourishment. To add to its miseries, the little starveling was caricatured by Gillray, lampooned by Peter Pindar, and ridiculed by Lord Brougham; and it was literally in the throes of dissolution when new life was breathed into it by the opportune arrival, in 1801, of a small spare youth of 22, from Bristol, whom the Managers had engaged at a salary of 100 guineas a year. The youth was Humphry Davy, who had acted as assistant to Dr. Beddoes, of the Pneumatic Institution, and who had already made some slight stir in scientific circles by his discovery of a characteristic property of nitrous oxide. In announcing his arrival to the Managers, Count Rumford reported that he had purchased a cheap second-hand carpet for Mr. Davy's room, together with such other articles as appeared to him necessary to make the room habitable, and among the rest a new sofa-bed, which, in order that it may serve as a model for imitation, had been made complete in all its parts. Six weeks after his arrival Davy was called upon to lecture, and a descriptive paragraph of the period thus chronicles his success in the *Philosophical Magazine* for 1801:—

“It must give pleasure to our readers to learn that this new and useful institution, the object of which is the application of Science to the common purposes of life, may be now considered as settled on a firm basis. . . .

“We have also to notice a course of lectures, just commenced at the institution, on a new branch of philosophy—we mean the Galvanic Phenomena. On this interesting branch, Mr. Davy (late of Bristol) gave the first lecture on the 25th of April. He began with the history of Galvanism, detailed the successive discoveries, and described the different methods of accumulating galvanic influence. . . . He showed the effect of galvanism on the legs of frogs, and exhibited some interesting experiments on the galvanic effects on the solution of metals in acids. Sir Joseph Banks, Count Rumford, and other distinguished philosophers were present. The audience were highly gratified, and testified their satisfaction by general applause. Mr. Davy, who appears to be very young, acquitted himself admirably well; from the sparkling intelligence of his eye, his animated manner, and the *tout ensemble*, we have no doubt of his attaining a distinguished eminence.”

And what was of more immediate consequence, this confident

assurance was shared also by the Managers, for at a subsequent meeting they unanimously resolved "that Mr. Humphry Davy, Director of the Chemical Laboratory, having given satisfactory proofs of his talents as a lecturer, should be appointed, and in future denominated, Lecturer in Chemistry at the Royal Institution, instead of continuing to occupy the place of Assistant Lecturer, which he has hitherto filled."

That such shrewd experienced men of the world as Sir Joseph Banks and Rumford, who were the moving spirits in the management of the Institution and genuinely solicitous for its welfare, should thus entrust its fortunes, then at their lowest ebb, to the power and ability of a young and comparatively unknown man, barely out of his teens, seems, even in an age which was familiar with the spectacle of "a proud boy" as a Prime Minister, like the desperate throw of a gambler.

But Banks and Rumford had, doubtless, good reason for the faith that was in them. For a happy combination of circumstances had served to bring the Cornish youth within the range of many who could be of service to him in that search for the fame for which he hungered. His connection with the Beddoes brought him the friendship of the Edgeworths, and it is amusing to trace how the good-humoured patronage of the gifted Maria quickly passed into amazement and ended in awe as her acquaintance with him ripened. Living in Bristol, he was at once brought into that remarkable literary coterie which distinguished that city at the close of the eighteenth century. Southey spoke of him as a miraculous young man, whose talents he could only wonder at. Cottle, the publisher, on one occasion said to Coleridge, "You have doubtless seen a great many of what are called the cleverest men—how do you estimate Davy in comparison with these?" Mr. Coleridge's reply was strong and expressive. "Why, Davy can eat them all! There is an energy, an elasticity, in his mind which enables him to seize on and analyse all questions, pushing them to their legitimate consequences. Every subject in Davy's mind has the principle of vitality. Living thoughts spring up like turf under his feet."

Davy's experimental work on "the pleasure-giving air" had made him known to the Watts and the Wedgewoods. Priestley, then in exile, and Hope of Edinburgh were greatly impressed with the philosophical acumen of the author of phosoxxygen, and he had a powerful friend in his own countyman Davies Gilbert, who succeeded him in the Presidential Chair of the Royal Society. We need be in no doubt, therefore, as to the influences which conspired to bring Davy into what he termed "the great hot-bed of human power called London."

The mention of Davy's first course of lectures in this Institution brings me at once to the proper subject of this discourse.

The first year of the last century is memorable for the invention

of the voltaic battery and for its immediate application by Nicholson and Carlisle in this country to the electrolytic decomposition of water.

Davy himself has said, "The voltaic battery was an alarm bell to experimenters in every part of Europe; and it served no less for demonstrating new properties in electricity, and for establishing the laws of this science, than as an instrument of discovery in other branches of knowledge; exhibiting relations between subjects before apparently without connection, and serving as a bond of unity between chemical and physical philosophy."

We owe it to Sir Joseph Banks that Volta's great discovery was first made known to English men of science, and the study of the phenomena of Galvanic Electricity was at once entered upon by a score of experimenters in this country. Among them was Davy. Even before he left Bristol he was hard at work on the subject, sending the results of his observations to Nicholson's *Journal* in a series of short papers. He resumed his inquiries immediately on his arrival in London, and was doubtless well prepared, therefore, for his opening course of lectures.

In 1801 he sent his first communication to the Royal Society on "An Account of Some Galvanic Combinations Formed by the Arrangement of Single Metallic Plates and Fluids, Analogous to the New Galvanic Apparatus of Mr. Volta." Although the work was continually interrupted by requests made to him by the Managers to carry out their own ideas of facilitating the means of procuring the comforts and conveniences of life, he never lost sight of the subject of voltaic electricity, and in spite of innumerable distractions due to the precarious position of the Institution, he gradually accumulated the material, out of which grew his first Bakerian Lecture "On Some Chemical Agencies of Electricity," read before the Royal Society on November 20th, 1806. I have ventured elsewhere to express my opinion of this paper. In my judgment it constitutes, in reality, Davy's greatest claim as a philosopher to our admiration and gratitude, for in it he, for the first time, succeeded in unravelling the fundamental laws of electro-chemistry, and thereby imported a new order of conceptions, altogether unlooked for and undreamt of, into science.

I am only at the moment concerned with this memoir in its relation to the discovery of which to-night we celebrate the centenary. The isolation of the metals of the alkalis was unquestionably an achievement of the highest brilliancy, and as such appeals strongly to the popular imagination. But it was only the necessary and consequential link in a chain of discovery which, had Davy neglected to make it, would have been immediately forged by another.

The publication of Davy's first Bakerian Lecture produced a great sensation, both at home and abroad. Berzelius, years afterwards, spoke of it as one of the most remarkable memoirs that had ever enriched the theory of chemistry. Very significant, too, of the

impression it made on the world of science was the action of the French Institute. Bonaparte, then First Consul, had announced his intention of founding a medal "for the best experiment which should be made in the course of each year on the galvanic fluid," and a committee of the Institute, consisting of Laplace, Halle, Coulomb, Haüy, and Biot, was appointed to consider the best means of giving effect to the wishes of the First Consul. To the young man, with the little brown head, like a boy (as Lady Brownrigg described him), now 28 years of age, was awarded the medal. All the Institute got from the founder of the medal was, what Maria Edgeworth termed, "a rating all round in imperial Billingsgate." There was no *entente cordiale* in those days; indeed, the feeling of animosity was intense. Of course, there were persons who said that patriotism should forbid the acceptance of the award. Davy's own view was more sensible and politic: "Some people," he said to his friend Poole, "say I ought not to accept this prize; and there have been foolish paragraphs in the papers to that effect; but if the two countries or governments are at war, the men of science are not. That would, indeed, be a civil war of the worst description; we should rather, through the instrumentality of men of science, soften the asperities of national hostility."

Thanks to the kindness of Dr. Humphry Davy Rolleston, the grandson of Dr. John Davy, the brother of Sir Humphry, who has also been so good as to lend me this admirable bust of the great chemist by Chantrey, and this charming portrait by Jackson, I am able to show you this evening this historically interesting medal.

What Davy looked like at this period of his life may be seen from the picture I now project upon the screen. It is a reproduction of the large portrait which hangs in the vestibule, and which the Institution owes to the thoughtful kindness of the late Mr. Graham Young.

As the applications of voltaic electricity seemed in 1806 to have no immediate bearing on the comforts and conveniences of life, Davy, during the greater part of the following year, was required to direct his attention to other matters. But in the late summer of 1807 he was able to resume his work with the voltaic battery, and he commenced to study its action on the alkalis.

That the alkalis—potash and soda—would turn out to be compound substances was not an unfamiliar idea at the time, and it is significant that almost immediately after Nicholson and Carlisle had resolved water into its elements by the action of voltaic electricity, Henry, of Manchester, the friend and collaborator of Dalton, should have made the attempt to apply the same agency to the separation of the presumed metallic principle of potash. The conception that what the older chemists called "earths" might be made to yield metals was at least as old as the time of Boyle, and probably dates back from the earliest days of alchemy. The relation of the earths to the metals was part of the doctrine of Becher and Stahl: it was no less a

part of the antiphlogistic doctrine of Lavoisier, although the points of view were diametrically opposed. Neumann attempted to obtain a metal from lime, Bergman considered that baryta was, like lime, a metallic calx, and Baron that alumina contained a metal. From their many analogies to these substances it was not unreasonable, therefore, to surmise that potash and soda might also contain metallic principles.

I have elsewhere pointed out that there is some evidence that whilst at Bristol Davy had already attacked the problem of the resolution of the alkalis by means of voltaic electricity. What precise idea he had in again attacking it, or what expectation he had of a definite result, is difficult to determine. In one of his lectures on Electro-chemical Science, delivered some time subsequently, he said he had a suspicion at the time that potash might turn out to be "phosphorus or sulphur united to nitrogen," conceiving, that as the volatile alkali was composed of the light inflammable hydrogen united to nitrogen, so the fixed and denser alkalis might be composed of the denser inflammable bodies—phosphorus and sulphur—also united to nitrogen.

Davy once said that "analogy was the fruitful parent of error," and few more striking instances of perverted analogy are to be met with in science than this. In another of his lectures he said of the alchemists that "even their *failures* developed some unsought-for object partaking of the marvellous"; and if such had been his reasoning, the statement is no less true of himself.

So far as can be ascertained, it was on October 19, 1807, that he obtained his first decisive result. This is thus described in Davy's own handwriting in the Laboratory Journal, which has been preserved for us by the pious care of Faraday, and which is one of the most precious of the historical possessions of the Royal Institution: "When potash was introduced into a tube having a platina wire attached to it, so [fig.], and fused into the tube so as to be a conductor—i.e. so as to contain just water enough, though solid—and inserted over mercury, when the platina was made negative, no gas was formed and the mercury became oxydated, and a small quantity of the alkaligen was produced round the platina wire, as was evident from its quick inflammation by the action of water. When the mercury was made the negative, gas was developed in great quantities from the positive wire, and none from the negative mercury, and this gas proved to be pure oxygen—a capital experiment, proving the decomposition of potash."

On the 19th of the following month he delivered what is generally regarded as the most memorable of all his Bakerian Lectures. It is entitled "On some New Phenomena of Chemical Changes produced by Electricity, particularly the Decomposition of the fixed Alkalies; and the Exhibition of the new substances which constitute their bases; and on the general Nature of Alkaline Bodies."

Few discoveries of like magnitude have been made and perfected in so short a time, and few memoirs have been more momentous in result than that which, put together in a few hours, gave the results of that discovery to the world.

The whole work was done under conditions of great mental excitement. His cousin, Edmund Davy, who at the time acted as his assistant, relates that when he saw the minute globules of the quicksilver-like metal burst through the crust of potash and take fire, his joy knew no bounds; he actually danced about the room in ecstasy, and it was some time before he was sufficiently composed to continue his experiments.

The rapidity with which he accumulated results, after this first feeling of delirious delight had passed, was extraordinary, and he had obtained most of the leading facts concerning the physics and chemistry of the new substances before the middle of November.

He began his lecture with a felicitous reference to the concluding remarks of one of the previous year, namely: "That the new methods of investigation promised to lead to a more intimate knowledge than had hitherto been obtained concerning the true elements of bodies. This conjecture, then sanctioned only by strong analogies, I am now happy to be able to support by some conclusive facts."

In the first attempts he made to decompose the fixed alkalis he acted upon concentrated aqueous solutions of potash and soda with the highest electrical power he could then command at the Royal Institution, viz., from voltaic batteries containing 23 plates of copper and zinc of 12 inches square, 100 plates of 6 inches, and 150 of 4 inches, charged with solutions of alum and nitric acid; but although there was high intensity of action, nothing but hydrogen and oxygen was disengaged. He next tried potash in igneous fusion, and here the results were more encouraging: there were obvious and striking signs of decomposition; combustible matter was produced, accompanied with flame and a most intense light. He had observed that, although potash, when dry, is a non-conductor, it readily conducts when it becomes damp by exposure to air, and in this state "fuses and decomposes by strong electrical powers."

Let me state in his own words, for the words are classical, what followed:—

"A small piece of pure potash, which had been exposed for a few seconds to the atmosphere, so as to give conductive power to the surface, was placed upon an insulated disc of platina, connected with the negative side of the battery of the power of 250 of 6 and 4 [that is 100 plates of 6 inches square and 150 plates of 4 inches square] in a state of intense activity: and a platina wire communicating with the positive side was brought in contact with the upper surface of the alkali Under these circumstances a vivid action was soon observed to take place. The potash began to fuse at both its points of electrization. There was a violent effervescence at the upper surface; at the

lower, or negative surface, there was no liberation of elastic fluid ; but small globules, having a high metallic lustre, and being precisely similar in visible characters to quicksilver, appeared, some of which burnt with explosion and bright flame, as soon as they were formed, and others remained, and were merely tarnished, and finally covered by a white film which formed on their surfaces."

He goes on to say :—

"Soda, when acted upon in the same manner as potash, exhibited an analogous result ; but the decomposition demanded greater intensity of action in the batteries, or the alkali was required to be in much thinner and smaller pieces.

"The substance produced from potash remained fluid at the temperature of the atmosphere at the time of its production ; that from soda, which was fluid in the degree of heat of the alkali during its formation, became solid on cooling, and appeared having the lustre of silver."

It would seem from this description of its properties that the potassium Davy first obtained was alloyed with sodium owing to the fact that the potash contained soda. Potassium is solid up to 143° F., whereas, as Davy was the first to show, an alloy of potassium and sodium is fluid at ordinary temperatures.

On account of their alterability in contact with air, Davy had considerable difficulty in preserving and confining the new substances so as to examine their properties. As he says, like the Alkahests imagined by the Alchemists, they acted more or less upon almost every body to which they were exposed. Eventually, he found they might be preserved in mineral naphtha.

The "basis" of potash was described by him as a soft malleable solid with the lustre of polished silver.

"At about the freezing point of water it becomes harder and brittle, and when broken in fragments, exhibits a crystallised texture which in the microscope seems composed of beautiful facets of a perfect whiteness and high metallic splendour. It may be converted into vapour below a red heat, and may be distilled unchanged, and is a perfect conductor of heat and electricity. Its most marked difference from the common run of metals is its extraordinary low specific gravity." At the time of its discovery, it was the lightest solid known.

The "basis" of soda was found to have somewhat similar properties. It was slightly heavier than the "basis" of potash, and fused at a higher temperature.

Davy next examined the behaviour of the new substances towards a large number of reagents, but as his observations are now the common property of the text-books, it is unnecessary here to dwell upon them.

He then enters upon some general observations on the relations of the "bases" of potash and soda to other bodies :

"Should the bases of potash and soda be called metals? The greater number of philosophical persons," he says, "to whom this question has been put, have answered in the affirmative. They agree with metals in opacity, lustre, malleability, conducting powers as to heat and electricity, and in their qualities of chemical combination.

"Their low specific gravity does not appear a sufficient reason for making them a new class; for amongst the metals themselves there are remarkable differences in this respect. . . . In the philosophical division of the classes of bodies, the analogy between the greater number of properties must always be the foundation of arrangement.

"On this idea, in naming the bases of potash and soda, it will be proper to adopt the termination which by common consent has been applied to other newly discovered metals, and which, though originally Latin, is now naturalised in our language.

"Potassium (*sic*) and sodium are the names by which I have ventured to call the new substances; and whatever changes of theory, with regard to the composition of bodies, may hereafter take place, these terms can scarcely express an error; for they may be considered as implying simply the metals produced from potash and soda. I have consulted with many of the most eminent scientific persons in this country upon the methods of derivation, and the one I have adopted has been the one most generally approved. It is perhaps more significant than elegant. But it was not possible to found names upon specific properties not common to both; and though a name for the basis of soda might have been borrowed from the Greek, yet an analogous one could not have been applied to that of potash, for the ancients do not seem to have distinguished between the two alkalies."

Such, then, are the more significant features of one of the greatest discoveries ever made by a British chemist, as these are set forth in one of the most remarkable papers in the Philosophical Transactions of the Royal Society.

Sir James Dewar has been so good as to have prepared for me a photographic reproduction of a water-colour drawing of the laboratory of the Royal Institution as it existed in Davy's time, showing the actual spot where the isolation of the metals of the alkalies was first effected.

The publication of Davy's discovery created an extraordinary sensation throughout the civilised world, a sensation not less profound, and certainly more general from its very nature, than that which attended his lecture of the previous year. But at the very moment of his triumph, it seemed that the noise of the universal acclaim with which it was received was not to reach him. I have already made reference to the condition of mental excitement under which the discovery was made and prosecuted. Almost immediately after the delivery of his lecture he collapsed, struck down by an illness which nearly proved fatal, and for weeks his life hung on a

thread. He had been in a low feverish condition for some time previously, and a great dread had fallen upon him that he should die before he had completed his discoveries. It was in this condition of body and mind that he had applied himself to the task of putting together an account of his results. Four days after this was given to the world he took to his bed, and he remained there for nine weeks. Such a blow following hard on the heels of such a triumph aroused the liveliest sympathy. The doors of the Royal Institution were beset by anxious inquirers, and written reports of his condition at various periods of the day had to be posted in the hall. The strength of the feeling may be gleaned, too, from the sentences with which the Rev. Dr. Dibdin, who had been hurriedly engaged to take his place in the theatre, began the lecture introductory to the Session of 1808.

"The Managers of this Institution have requested me to impart to you that intelligence, which no one who is alive to the best feelings of human nature can hear without the mixed emotion of sorrow and delight.

"Mr. Davy, whose frequent and powerful addresses from this place, supported by his ingenious experiments, have been so long and so well known to you, has, for the last five weeks, been struggling between life and death. The effects of these experiments recently made in illustration of his late splendid discovery, added to consequent bodily weakness, brought on a fever so violent as to threaten the extinction of life. Over him it might emphatically be said in the language of our immortal Milton, that

' Death his dart
Shook, but delayed to strike.'

"If it had pleased Providence to deprive the world of all further benefit from his original talents and intense application, there has certainly been sufficient already effected by him to entitle him to be classed among the brightest scientific luminaries of his country."

After having, "at the particular request of the Managers," given an outline of Davy's investigations, Dr. Dibdin proceeded to say:—

"These may justly be placed among the most brilliant and valuable discoveries which have ever been made in chemistry, for a great chasm in the chemical system has been filled up; a blaze of light has been diffused over that part which before was utterly dark; and new views have been opened, so numerous and interesting, that the more any man who is versed in chemistry reflects on them, the more he finds to admire and heighten his expectation of future important results.

"Mr. Davy's name, in consequence of these discoveries, will be always recorded in the annals of science amongst those of the most illustrious philosophers of his time. His country, with reason, will be proud of him, and it is no small honour to the Royal Institu-

tion that these great discoveries have been made within its walls—in that laboratory, and by those instruments which, from the zeal of promoting useful knowledge, have, with so much propriety, been placed at the disposal and for the use of its most excellent professor of chemistry.”

And now, in the few minutes that remain to me, let me indicate what has been the outcome of this great and fundamental discovery. How far has the expectation of future important results been realised? Have sodium and potassium at all justified the hope that they would facilitate the means of procuring the comforts and conveniences of life?

I have not the time, even if I had the intention, to attempt to follow the many changes in the metallurgy of the metals of the alkalis of the past century. Let me at once proceed to show how the matter stands at the end of a hundred years.

The general properties and chemical activities of potassium and sodium are so very similar that as a matter of commercial production that metal which can be most economically obtained is necessarily the one most largely manufactured, and of the two that metal is sodium. To-day, sodium is made by thousands of tons, and by a process which in principle is identical with that by which it was first made by Davy, i.e. by the electrolysis of fused caustic soda. It is very significant that after a series of revolutions in its manufacture, sodium, having been produced from time to time on a manufacturing scale by a variety of metallurgical methods involving purely thermal processes of reduction and distillation, entirely dissociated from electricity, we should have now got back to the very principle of the process which first brought the metal to light. And that this has been industrially possible is entirely owing to another of Davy's discoveries—possibly indeed the greatest of them all—Michael Faraday. As we all gratefully acknowledge, it is to the genius and labours of Faraday—Davy's successor in this place—that the astonishing development of the application of electrical energy which characterises this age has taken its rise.

The modern method of production of sodium is based, therefore, as regards principles upon the conjoint labours of Davy and Faraday.

These principles took their present form of application at the hands of a remarkably talented American—Mr. Hamilton Y. Castner—whose too early death, in the full vigour of his intellectual powers, was an incalculable loss to metallurgical chemistry. It is by Castner's process that all the sodium of to-day is manufactured.

In the Castner process melted caustic soda produced by the electrolysis of a solution of common salt by a method also devised by Castner, is brought into an iron vessel shaped like a large cauldron, mounted in brickwork, and provided with an extension adapted to receive the negative electrode. Suspended directly above the cathode is an iron vessel attached to a lid; to its lower edge is secured iron

wire gauze, which, when the receptacle is in position, completely surrounds the cathode. The positive electrode is connected with the lid of the vessel, which is provided with openings for the escape of the gases resulting from the electrolysis, and is suitably insulated.

As the electrolysis proceeds the alkali metal, being much lighter than the molten caustic, rises from the negative electrode and passes into the receiver, the gases escaping around the edges of the cover. The molten metal collects on the surface of the caustic, and is removed by means of a large perforated spoon, the perforations enabling the melted caustic to flow out, while the metal remains in the spoon. As the several vessels are thus skimmed in succession the fused sodium is collected into an iron vessel, whence it is poured into moulds in which it congeals, forming blocks of the size and shape of an ordinary building brick. These, after being trimmed to remove adherent oxide, are immersed in paraffin oil, and are then packed into large iron drums holding about 6 or 7 cwt., capable of being closed air-tight, and protected in transit by an outer casing of wood.

The due regulation of the volume and intensity of the current is a matter of the greatest importance in order to obtain the most economical yield of the metal. No very high temperature is needed; indeed, the temperature of the fused caustic soda should not be much higher than that of its melting point. By suitably regulating the current, the soda, in fact, may be maintained at the proper temperature and in the proper degree of fluidity without extraneous heat. Fresh melted caustic soda is added to the vessel from time to time to replace the metal removed, and in this manner the process is made continuous.

The Castner process is now worked in England at Wallsend-on-Tyne, and at Weston Point, in Cheshire; at Rheinfelden, in Germany; at Clavaux, in France; also in Switzerland, and at Niagara, in America. The present yearly output amounts to about 5000 tons, but the plant already laid down is capable of producing at least twice this quantity.

The greater quantity of the sodium made in England is sent to Glasgow, where it is converted into sodium cyanide by the Cassel Cyanide Company for use in the extraction of gold. As gold is, I suppose, generally considered the principal material factor in procuring the comforts and conveniences of life, Davy's great discovery may be thus said to have secured the primary object which the projectors of the Royal Institution had in view. Other important uses of sodium are in the manufacture of peroxide for bleaching purposes, of artificial indigo, and of a number of other synthetic dye stuffs and of drugs like antipyrin.

It need hardly be said that this extraordinary development of the manufacture has not been without its influence on the price of sodium. A quarter of a century ago it was a comparatively rare metal, and a stick of it was regarded as a chemical curiosity, to be

handled with circumspection and care. Even as late as 1890 its selling price was as high as 8s. per lb. To-day it is 8*d.* Sodium now takes rank, therefore, with zinc, tin, copper, or aluminium as a common, ordinary metal of commerce.

I am indebted to the directors of the Castner-Kellner Company, and in particular to my friends Sir Henry Roscoe and Mr. Beilby, for affording me the opportunity, in connection with this lecture, of actually witnessing the modern process of manufacturing sodium as it is carried out at Wallsend; and I am further indebted to Mr. Beilby for the loan of the lantern slides and specimens with which I have sought to illustrate that process.

And in concluding may I be permitted to recall here the feelings to which that visit to Wallsend gave rise. There, grouped together on the very spot where ended the old wall—the visible symbol of the power and might of a civilisation long since passed away—were some of the characteristic signs of another civilisation ampler and more beneficent. Before me, stretching down to the river, was the factory where a score of workers, clad in helmets and gauntlets and swathed like so many Knights Templar, travel-stained and war-worn, their visages lit up by the yellow soda flames, and their ears half-deafened with the sound of exploding hydrogen—a veritable inferno—were repeating on a Gargantuan scale the little experiment first made a century ago in the cellars of this building; turning out, day and night, hundredweights of the plastic metal in place of the little pin-heads which then burst upon the astonished and delighted gaze of Davy. Behind me was the magnificent power-house—one of the most magnificent of its kind in the world—furnishing not only the electrical energy which transformed the soda into sodium, but diffusing this energy for a multitude of other purposes over an entire district—a noble temple to the genius and prescience of Faraday. Surely one might here say, if you desire to see the monuments of these men, look around! And to my right, and close at hand, was the huge building slip just vacated by the *Mauretania*, herself a symbol of the supremacy of an empire, far mightier, more world-wide, and more potent for good than that which massed its legions behind the old wall.

[T. E. T.]



WEEKLY EVENING MEETING,

Friday, January 24, 1908.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer
and Vice-President, in the Chair.

COLONEL DAVID BRUCE, R.A.M.C. C.B. D.Sc. F.R.S.

The Extinction of Malta Fever.

THE subject of this evening's discourse is the Extinction of Malta Fever, and I propose to bring before you in this paper the various steps in the investigation of this disease which led up to the discovery of its mode of spread, and so to its prevention and extinction.

HISTORICAL.

This fever has been studied in various ways for the last quarter of a century, but it was not until 1904 that the Government, alarmed by the great wastage in man caused by it, took the matter up seriously, and asked the Royal Society to undertake a thorough investigation of the disease. This the Royal Society agreed to do, and early in the summer of the same year sent out to Malta a small Commission for this purpose ; and it is principally the result of the work of the Commission which I have the honour of bringing before you this evening.

It seems a pity that this research was not undertaken twenty years earlier, as during this time some 14,000 or 15,000 soldiers and sailors have suffered from the disease. It is to be hoped that the result of this work will bring home to the Government the great good to be gained by introducing scientific methods of research into the study of disease in the Army. This, strange as it may seem, has not yet come home to Government departments. If an application was made to the Treasury to-morrow for, say, 100/. for such scientific purposes, the answer would be that it was not the function of the Royal Army Medical Corps to engage in scientific research, but that their duty was to attend to the sick soldiers. This waiting till a man is sick is fatal. It ought to be our chief duty to anticipate and prevent sickness.

Before I leave the subject of the Commission, I may remark that its work went on for three years before the successful result was attained.

But now to return to Malta fever.

DESCRIPTION OF MALTA FEVER.

At the outset it will be necessary to give a short description of this fever, in order that you may know what we are dealing with.

Malta fever is no trivial complaint, but is a severe and dangerous disease, which lasts a long time, and is accompanied by a good deal of pain. To give you an idea of the long duration of this fever, I may tell you that our soldiers remain under treatment in hospital with it on an average for 120 days, and it is by no means uncommon for a patient to suffer almost continually from it for two or even more years.

During the whole course of his illness the patient is apt to suffer from severe rheumatic pains in the joints, and neuralgia in various nerves, and this combined with the long-continued fever, brings about a condition of extreme emaciation and weakness, from which recovery is slow.

In order to show you to what a degree of emaciation a few weeks of this fever may bring a man, I will take the liberty of throwing on the screen a photograph of a soldier who has been suffering from it for a few weeks. [Here a picture of a man extremely thin and evidently very ill was thrown on the screen.]

On admission to hospital this man was a robust and muscular soldier, and now see what a few weeks have brought him to.

INCIDENCE OF MALTA FEVER IN THE
GARRISON.

Next I would draw your attention to the number of cases of this fever which occur among our sailors and soldiers in Malta, in order to impress upon you the importance of this disease to the State. Among our soldiers, who number about 7000, there have been on an average 312 admissions to hospital every year from Malta fever alone, and among the sailors about the same number. This means that 624 soldiers and sailors have been treated in hospital 120 days each, which makes about 75,000 days of illness per annum.

To illustrate this I throw on the screen a diagram (Fig. 1).

Now I have said enough to show you that we are dealing with a severe and important form of disease.

STUDY OF MALTA FEVER FROM THE EPIDEMIOLOGICAL
POINT OF VIEW.

Before we begin the experimental investigation of this fever, it is well that we should know as much as possible about it from a general point of view. For example: In what parts of the world is it found; under what conditions of climate; whether any connection can be

made out between it and the temperature or rainfall; whether age or sex render a person more liable; whether occupation or social position has any bearing on it; whether a difference in sanitary conditions has any effect, as, for example, do people living in small villages without any proper system of water supply suffer more than those living in towns supplied with pure water and a modern drainage system?

Now it is clearly impossible for me to go into all these points with the time at my disposal, but I would like to bring before you a few facts which bear on the problem we have before us.

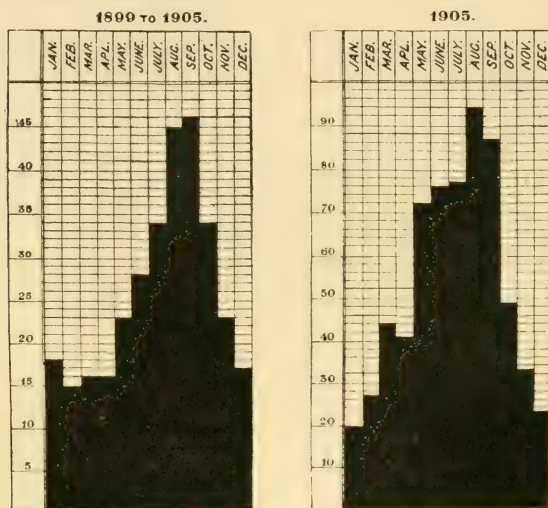


FIG. 1.—CHART OF INCIDENCE IN 1899-1905, AND 1905.

Geographical Distribution.—For example, it is interesting to know that Malta fever is not confined to Malta, but occurs in most parts of the world.

Climatic Conditions.—Then again in regard to the effect of climate. Malta is extremely hot and dusty in the summer, and correspondingly cold and wet in winter. But, although the number of cases of Malta fever do show an increase in summer, yet it is a disease which is prevalent all the year round, one-third as many cases occurring in the coldest and rainiest months, as in the hottest and dustiest.

Another fact of importance, is that if we study the occurrence of Malta fever in individual years, we are struck by its irregularity, a number of cases appearing in December or February or other of the cold and rainy months.

Social Position.—Another curious fact in regard to this disease is, that the better the social position of a person the more risk is there of catching this fever. Officers and their wives and children, living in large, airy and clean houses, suffer more frequently than the men in their more crowded barrack-rooms. In fact the chance of a naval or military officer taking this fever was more than three times as great as in the case of the men.

This is shown on this diagram†(Fig. 2).

MALTA FEVER IN THE CARRISON
RATIO per 1000.
1897 to 1905

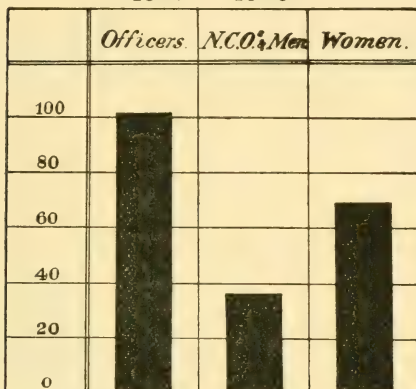


FIG. 2.—INCIDENCE IN OFFICERS, MEN AND WOMEN,
FOR 1887-1905.

Distribution of Malta Fever among the Civil Population.—Another important fact is the distribution of Malta fever among the civil population. Until recently it was supposed by many of us that it was restricted to the inhabitants of the cities surrounding the Grand Harbour. This was in the days when the theory was held that the poison which causes this fever was found in the air. As the Grand Harbour at that time was in a very dirty condition, the drainage of Valetta and the three cities falling into it, there was some excuse for this belief.

Malta fever is now known to occur in every part of Malta, and, in fact, the general distribution of this disease is very striking. It is not the cities round the harbours which are struck most heavily, some of the inland towns and villages showing a much higher fever-rate.

This is illustrated by the following diagram (Fig. 3).

STUDY OF MALTA FEVER BY THE EXPERIMENTAL
METHOD.

Discovery of the Parasite.—Let us approach this problem from the experimental side. The first step to be taken is to discover if any parasite or micro-organism is associated with this fever. To do this we examine the blood and the tissues of the various organs, both microscopically and by means of cultivation, on suitable media, to find out if anything can be seen or grown. In this way, as long ago as 1887, it was discovered that a minute organism to which the name of *Micrococcus melitensis* was given, is the cause of this disease.

Description of the Micrococcus Melitensis.—There is not much to be said about this micro-organism, except that it is very minute, only becoming visible under a magnification of 1000 diameters. It is round or oval in shape, and non-motile. It is found in every case of Malta fever, and if injected under the skin of monkeys gives rise in them to a fever similar to that in man.

CHARACTERISTICS OF THE MICROCOCCUS MELITENSIS.

Behaviour outside the Body.—Now, having found the micro-organism, it is necessary to study its characteristics.

It is found to survive outside the body for some time. For example, it can retain its vitality and virulence in a dry condition in dust or on clothing for at least two or three months. It can also live in a moist condition; in water—tap-water or sea-water—for a somewhat shorter period.

The important thing to be noted is, that it does not increase outside the body; it merely survives for some time, and then dies off; and that, if exposed to direct sunlight, it disappears in a few hours.

Many attempts were made to discover it outside the body, under natural conditions. As the generally accepted theory was that it was conveyed in air, naturally the air of fever wards or of places where cases had occurred was examined with great care. It was also looked for in the dust of suspected places and in the water of the harbour; but with no success. It is evidently what is known as a facultative parasite, or one which depends on a host for its existence.

Thus, then, the first important step in our discovery of a means of preventing Malta fever has been taken. We now know the cause of the disease, and can look with some chance of success for the source whence man obtains it.

The next steps are to find out how this micrococcus leaves, and how it gains entrance to the body.

HOW DOES THE MICROCOCCUS MELITENSIS LEAVE THE BODY ?

In regard to the first of these, it is conceivable that it might leave the body by way of the expired air, in the saliva, in mucus from the lungs, as in consumption, in the secretion of the skin, as in scarlet fever, in the renal secretion, or by way of the intestinal tract. Or it might leave the body by way of the blood, by the agency of mosquitoes or other biting flies.

Many experiments were made along all these lines, and finally it was decided that this micro-organism leaves the body principally in the renal secretion, and in the blood taken out of the body by blood-sucking insects.

The result, therefore, of this experimental work was to give rise to the belief that the disease was either conveyed from the sick to the healthy by contact, or by inhalation of infected dust, or, lastly by the agency of mosquitoes.

HOW DOES THE MICROCOCCUS MELITENSIS GAIN ENTRANCE TO THE BODY ?

The investigation of these various modes of infection was therefore undertaken.

By Contact.—Let me first consider infection by contact. Experiments were made by placing monkeys, one affected by Malta fever, the other healthy, in more or less intimate contact, and it was found that if the monkeys lived together in the same cage infection did take place. If, on the other hand, the monkeys were kept in the same cage, but separated by a wire screen, so that, although they could touch each other, contamination of the healthy monkey's food by the sick monkey could not take place, then infection did not take place.

In regard to this question of conveyance by contact, there is one argument against it which has always seemed to me unanswerable, and that is, that thousands of cases of Malta fever have been invalided home to England, and treated in our naval and military hospitals, without, as far as I am aware, a single case of the fever arising among the patients, orderlies, or nursing sisters.

It was, therefore, concluded that contact with Malta fever patients, or the handling of infected clothing or discharges, is not the mode of infection.

Then the question of infection by contaminated dust was taken up.

By Dust Contaminated by the Micrococcus Melitensis.—For some time it was considered probable that this would prove to be the common method of infection. The fact that the micrococcus withstands drying for a long time, the dusty nature of Malta, and the

probability that gross contamination of the surface of the soil takes place by infective discharges, rendered this view likely.

Experiments were made to put the theory to the test. Dust was artificially contaminated with micrococci and blown about a room in which monkeys were confined, or blown into their nostrils or throat. Several of these experiments were successful. It was therefore proved that dust *artificially* contaminated with *micrococcus melitensis* could give rise to the disease.

This, however, was no proof that this mode of infection occurs in nature. The artificially contaminated dust contained myriads of micrococci. Under natural conditions, they could seldom be numerous, and the powerful Maltese sunlight would tend to kill them off rapidly. The dust blown about by the wind must also dilute the micrococci to an enormous extent, so that it is only possible to conceive of a micrococcus here and there in a vast quantity of dust. Experiments were therefore made with dust naturally contaminated, in order more closely to resemble natural conditions. Dust contaminated in this way, and also that collected from suspicious places and blown about the cages, sprinkled on food, or injected under the skin, always gave negative results.

The conclusion was therefore again come to that conveyance of the infective germ by means of contaminated dust could only rarely, if ever, give rise to the disease.

By Mosquitoes or other Biting Insects.—As already mentioned, the theory had been strongly advanced that Malta fever, like yellow fever or plague, might be conveyed by blood-sucking insects. The fact that the micrococci are frequently found in the peripheral blood, gave some colour to the belief. This point was therefore fully investigated and numerous experiments made with the different species of mosquitoes found in Malta, and also with other blood-sucking insects.

The results, again, were all negative, and it was therefore decided that Malta fever is not conveyed by contact, by contaminated dust, or by mosquitoes.

What, then, could be the mode of spread?

By Way of the Alimentary Canal.—It had long been known that the smallest quantity of the micrococci introduced under the skin or applied to a scratch would give rise to the disease in man or monkeys, but some work by previous observers had led us to believe that infection did not take place by way of the mouth in food or drink. They had fed monkeys on milk contaminated by the micrococci, and stated that in no case had infection taken place. This observation kept the Commission at first from making feeding experiments. As infection, however, did not appear to take place by contact, by the inhalation of infected dust, or by mosquitoes, it was clearly necessary to repeat these feeding experiments.

FEEDING EXPERIMENTS.

Here is a table showing the result of some of these feeding experiments, and you see it is abundantly proved that Malta fever can be conveyed to healthy animals by way of the alimentary canal. Even a single drink of a fluid containing few micrococci almost certainly gives rise to the disease (Fig. 4).

Malta Fever

Species of animal	Mode of infection. M. = <i>M. malleus</i>	Probable time which elapsed before infection took place in days	Result + Infection - No infection
Monkey 39	Feeding on potato containing M.	30	+
" 40	Do. do.	31	+
" 66	Accidental feeding	+
" 72	Milk + M.; stomach tube	..	+
" 113	Dust + Mediterranean fever urine. Dried	..	-
" 114	Do. do.	..	-
" 119	Dust + Mediterranean fever urine. Moist	..	+
" 124	Potato + M. from spleen	..	+
" 125	Do. do.	..	+
" 126	Potato + M. from urine	..	+
" 127	Do. do.	..	+
" 2	Milk + M	..	+
" 4	Do.	..	+
" 5	Do.	..	+
" 99	Do.	..	+
" 6	Culture	..	+
" 7	Do.	..	+
" 8	Do.	..	+
" 9	Do.	..	+
" 19	Do.	18	+
" 19a	Do.	82	+
Kid 9	Milk	..	-
" 19a	Mother's milk	-
Goat 12	Culture from milk	+
" 13	Mediterranean fever urine and dust	..	+
" 14	Do. do.	..	+
" 4	Milk + culture	+

FIG. 4.
FEEDING EXPERIMENTS.

From the results, then, of all these experiments it seemed most probable that the micrococcus gained an entrance to the body by way of the alimentary canal, and therefore by some infected food or drink.

This led to an examination of food stuffs, and among these the milk of the goat is one of the most important.

INFECTION BY MEANS OF GOAT'S MILK.

The goat is very much in evidence in Malta, and supplies practically all the milk used. There is, I believe, one goat to every ten of the population, so that, as there are 200,000 inhabitants there must be 20,000 goats. Flocks of them wander about the streets from morning till night, and are milked as required at the customers' doors (Fig. 5).

It must be confessed there seemed little hope that an examination of these animals would yield any result. The goats appeared perfectly healthy, and they have the reputation of being little susceptible to disease of any kind.



FIG. 5.—MILKING GOATS.

To put the matter to the test several goats were inoculated with the micrococcus, and the result watched. There was no rise of temperature, no sign of ill-health in any way, but in a week or two the blood was found to be capable of agglutinating the specific micro-organism.

This raised our suspicions, and a small herd of apparently healthy goats was then procured and their blood examined to see if they were all healthy. Several of them were found to react naturally to the agglutination test, and this led to the examination and the discovery of the *Micrococcus melitensis* in their blood, urine and milk. Fig. 6 shows the enormous number of these microbes found in goat milk. Each of the tiny dots represents a colony of micrococcus.

MICROCOCCHI IN GOAT'S MILK.

Some thousands of goats in Malta were then examined, and the astounding discovery was made that 50 per cent. of the goats responded to the agglutination test, and that actually 10 per cent. of them were secreting the micrococci in their milk.

Monkeys fed on milk from an affected goat, even for one day, almost invariably took the disease.

S.S. "JOSHUA NICHOLSON."

At this time, curiously enough, an important experiment on the drinking of goat's milk by man took place accidentally. Shortly, the story is as follows: In 1905 the s.s. 'Joshua Nicholson,' shipped

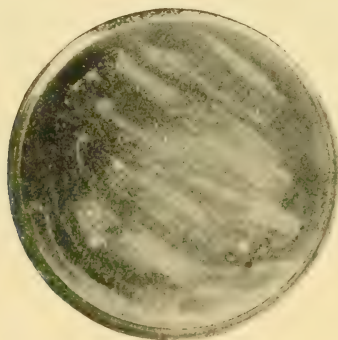


FIG. 6.—GROWTH OF MILK ON AGAR PLATE.

sixty-five goats at Malta for export to America. The milk was drunk in large quantities by the captain and the crew, with the result that practically everyone who drank the milk was struck down by Malta fever.

Sixty of the goats (five having died) on arrival in America were examined, and thirty-two found to give the agglutination reaction, while the *Micrococcus melitensis* was isolated from the milk of several of them. This epidemic of Malta fever on board the s.s. 'Joshua Nicholson' therefore clinched the fact, that the goats of Malta act as a reservoir of the virus of Malta fever, and that man is infected by drinking the milk of these animals.

EPIDEMIOLOGICAL FEATURES.

Here, then, at last was discovered a mode of infection which explains the curious features of Malta fever—the irregular seasonal prevalence, the number of cases which occur during the winter months, when there are no mosquitoes and little dust. It is true there are more cases in summer than in winter, but this may be explained by the fact that more milk is used at that time of the year for fruit, in ice-creams, etc. It also explains the fact that officers are more liable than the men, as the former consume more milk than the latter. It also explains the liability of hospital patients, milk entering so largely into a hospital dietary.

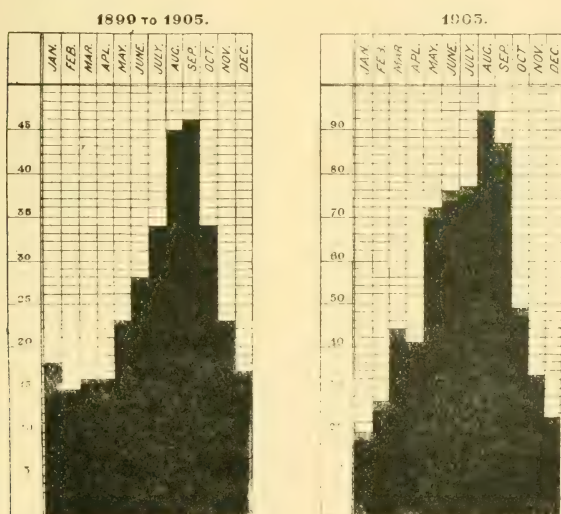


FIG. 7.—CHART OF INCIDENCE IN 1899-1905, AND 1905.

RESULT OF MEASURES DIRECTED AGAINST THE USE OF GOAT'S MILK.

As soon as goat's milk was discovered to be the source of infection, preventive measures were begun. The result is very striking, as is shown in the charts thrown on the screen, which give the number

of cases of Malta fever among the soldiers in the garrison before and after the preventive measures came into action.

Here is a chart of the incidence of Malta fever among the soldiers each month before the preventive measures were put into force (Fig. 7).

And here is another (Fig. 8) showing the incidence of this fever

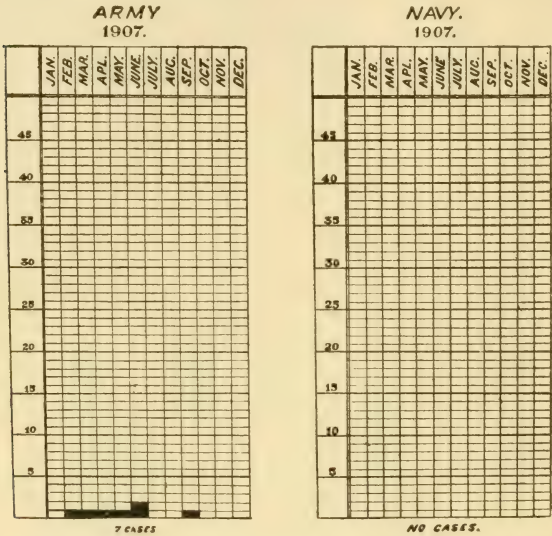


FIG. 8.—CHART OF SOLDIERS AND SAILORS, 1907.

among the soldiers and sailors in Malta since goat's milk has been banished from their dietary. With this chart, which shows the practical extinction of Malta fever, my discourse comes to a close.

[D. B.]

WEEKLY EVENING MEETING,

Friday, January 31, 1908.

THE RIGHT HON. LORD RAYLEIGH, O.M. P.C. M.A. D.C.L.
LL.D. Sc.D. Pres.R.S., in the Chair.

PROFESSOR ERNEST RUTHERFORD, M.A. LL.D. D.Sc. F.R.S.

Recent Researches in Radio-activity.

IN 1904 I had the honour of giving an address at the Royal Institution on the subject of Radio-activity. In the interval steady and rapid progress has been made in unravelling the tangled skein of radio-active phenomena. In the present lecture I shall endeavour to review very shortly some of the more important advances made in the last few years, but as I cannot hope to mention, even briefly, the whole additions to our knowledge in the various branches of the subject, I shall confine my attention to a few of the more salient facts in the development of which I have taken some small share.

In my previous lecture I based the explanation of radio-active phenomena on the disintegration theory put forward in 1903 by Rutherford and Soddy, which supposes that the atoms of the radio-active bodies are unstable systems which break up with explosive violence. This theory has stood the test of time, and has been invaluable in guiding the experimenter through the maze of radio-active complications. In its simplest form, the theory supposes that every second a certain fraction (usually very small) of the atoms present become unstable and explode with great violence, expelling in many cases a small portion of the disrupted atom at a high speed. The residue of the atom forms a new atomic system of less atomic weight, and possessing physical and chemical properties which markedly distinguish it from the parent atom. The atoms composing the new substance formed by the disintegration of the parent matter are also unstable, and break up in turn. The process of degradation of the atom, once started, proceeds through a number of distinct stages. These new products formed by the successive disintegrations of the parent matter are in most cases present in such extremely minute quantity that they cannot be investigated by ordinary chemical methods. The radiations from these substances, however, afford a very delicate method of qualitative and quantitative analysis, so that we can obtain some idea of the physical and chemical properties of substances existing in an amount which is far below the limit of detection of the balance or spectroscope.

The law that governs the breaking up of atoms is very simple and universal in its application. For any simple substance, the average number of atoms breaking up per second is proportional at any time to the number present. In consequence, the amount of radio-active matter decreases in a geometrical progression with the time. The "period" of any radio-active product, i.e. the time for half the matter to be transformed, is a definite and characteristic property of the product which is uninfluenced by any of the laboratory agents at our command. In fact, the period of any radio-active product, for example, the radium emanation, if determined with sufficient accuracy, might well be taken as a definite standard of time, independent of all terrestrial influences.

The law of radio-active transformation can be very simply and aptly illustrated by an hydraulic analogy. Suppose we take a vertical cylinder filled with water, with an opening near the base through which the water escapes through a high resistance.* When the discharge is started, the amount of water escaping per second is proportional to the height of water above the zero level of the cylinder. The height of water decreases in a geometrical progression with the time in exactly the same way as the amount of radio-active matter decreases. We can consequently take the height of the column of water as representing the amount of radio-active matter A present at any time. The quantity of water escaping per second is a measure of the rate of disintegration of A and also of the amount of the new substance B formed per second by the disintegration of A. The "period" of the substance is controlled by the amount of resistance in the discharge circuit. A high resistance gives a small flow of water and a long period of transformation, and *vice versa*. By a suitable arrangement we can readily trace out the decay curve for such a case. A cork carrying a light vertical glass rod is floated on the water in the cylinder. A light camel's hair brush is attached at right angles, and moves over the surface of a smoked-glass plate. A vertical line drawn on the glass through the point of contact of the brush gives the axis of ordinates, while a horizontal line drawn through the brush when the water has reached its lowest level gives the axis of abscissæ. If the glass plate is moved with uniform velocity from the moment of starting the discharge, a curve is traced on the glass which is identical in shape with the curve of decay of a radio-active product, where the ordinates at any time represent the relative amount of active matter present, and the abscissæ time. With such an apparatus we can illustrate in a simple way the increase with time of radio-active matter B, which is supplied by the transformation of a substance A. This will correspond, for example, to the growth of the radium emanation with time in a quantity of radium initially freed from emanation. Let us for convenience

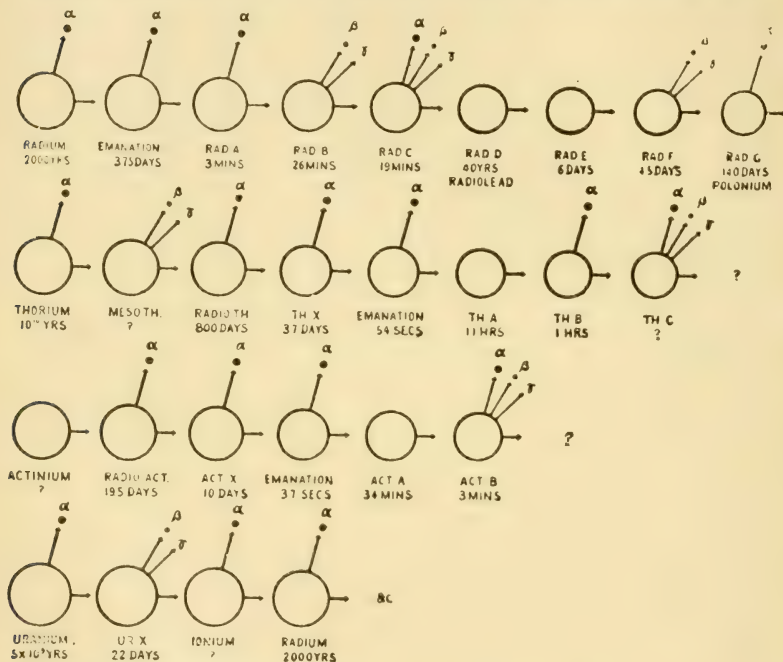
* A short glass tube in which is placed a plug of glass wool is very suitable.

suppose that A has a much longer period than B. In the hydraulic analogy A is represented by a high head of water discharging at its base through a circuit of high resistance into the top of another cylinder representing the matter B. The water from the cylinder B escapes at its base through a lower resistance. Suppose that initially only A is present. In this case the water in the cylinder B stands at a zero level. On opening the stop-cock connecting with A, water flows into B. The rise of water with time in the cylinder B is traced out in the same way as before by moving the glass plate at a constant rate across the tracing brush. If the period of A is very long compared with that of B the water is supplied to B at a constant rate, and the water in B reaches a constant maximum height when the rate of supply to B equals the rate of escape from the latter. The curve traced out in that case is identical in shape with the "recovery curve" of a radio-active product supplied at a nearly constant rate. The quantity of matter reaches a maximum when the rate of supply equals its own rate of transformation. The relative height of the columns of water in A and B represents at any time the relative amounts of these substances present.

If the period is comparable with that of B, the height of water in B after reaching a maximum falls again, since as the height of A diminishes, the supply to B decreases. Ultimately, the height of B will decrease in a geometrical progression with the time at a rate corresponding to the longer period of the two. This is an exact illustration of the way the amount of a radio-active substance B varies when initially only the parent substance A is present. By using a number of cylinders in series, each with a suitable resistance, we can in a similar way illustrate in a quantitative manner the variation in amount with time of a number of products arising from successive disintegrations of a primary substance. By suitably adjusting the amount of resistance in the discharge circuits of the various cylinders, the curves could be drawn to scale to imitate approximately the variation in amount of the various products with time when the initial conditions are given.

During the last few years a very large amount of work has been done in tracing the remarkable succession of transformations that occur in the various radio-active substances. The known products of radium, thorium, actinium, and uranium are shown graphically below, together with the periods of the products and the character of the radiations they emit. It will be seen that a large list of these unstable bodies are now known. It is probable, however, that not many more remain to be discovered. The main uncertainty lies in the possibility of overlooking a product of rapid transformation following or succeeding one with a very slow period. In tracing out the succession of changes, the emanations or radio-active gases continuously evolved by radium, thorium, and actinium have marked a very definite and important stage, for these emanations can be easily

removed from the radio-active body and their further transformations studied quite apart from the parent element. The analysis of the transformation of the radium emanation has yielded results of great importance and interest. After passing through three stages, radium A, B, and C, of short period, a substance, radium D, of long period, makes its appearance. This is transformed through two stages E and F of short period into radium G, of period 140 days. Meyer and Schweidler have conclusively shown that radium D is the primary constituent of the radio-active substance separated by Hofmann, and called by him radio-lead. Radium G is identical with the first radio-



active substance separated from pitch-blende by Madame Curie, viz. polonium. We are thus sure that these bodies are transformation products of radium. It will be seen that I have added another product of period 4.5 days between radium D and polonium. The presence of such a product has been shown by Meyer and Schweidler.

In the case of thorium, a very long list of products is now known. For several years thorium X was thought to be the first product of thorium, but Hahn has recently shown that at least two other products of slow transformation intervene, which he has called mesothorium and radiothorium. The radiothorium emits α rays, and

has a period of more than 800 days. Mesothorium apparently emits β rays, and has a still longer period of transformation, the exact value of which has not yet been accurately determined. Since thorium is used commercially on a large scale, there is every prospect that we shall soon be able to obtain considerable quantities of very active preparations of mesothorium and radiothorium. The separation of these bodies from thorium does not in any way alter its commercial value. It is to be hoped that if these active preparations are separated in quantity, the physicist and chemist may be able to obtain a supply of very active material at a reasonable cost, and that there will not be an attempt to compete with the ridiculously high prices charged for radium.

From the radio-active point of view, the radio-elements are only distinguished from their families of products by their comparatively long period of transformation. Now we have reason to believe that radium itself is transformed according to the laws of other radio-active products with a period of about 2000 years. If this be the case, in order to keep up its supply in a mineral, radium must be produced from another substance of relatively long period of transformation. The search for this elusive parent of radium has been one of almost dramatic interest, and illustrates the great importance of the theory as a guide to the experimenter. The view that radium was a substance in continuous transformation was put forward by Rutherford and Soddy in 1903. The most probable parent of radium appeared to be uranium, which has a period of transformation of the order of 1000 million years. If this were the case, uranium, initially freed from radium, should in the course of time grow radium, i.e. radium should again appear in the uranium. This has been tested independently by Soddy and Boltwood, and both have shown that in carefully prepared uranium solutions there is no appreciable growth of radium in the course of several years. The rate of production of radium, if it occurs at all, is certainly less than $\frac{1}{10000}$ of the amount to be expected from theory. This would appear at first sight to put out of count the view that uranium is the parent of radium. This, however, is by no means the case, for such a result could be very easily explained if one or more substances of very slow period of transformation appeared between uranium and radium. It is obvious that the necessity of forming such an intermediate product would greatly lengthen the time required before an appreciable amount of radium appeared.

There is, however, another indirect but very simple method of attack to settle the parentage of radium. If radium is derived from the transformation of uranium, however many unknown products intervene, the ratio between the amount of radium and uranium in old minerals should be a definite constant. This is obviously the case, provided sufficient time has elapsed for the amount of radium to have reached its equilibrium value. The constancy of this relation

has been completely substantiated by the independent work of Boltwood, Strutt, and McCoy. It has been shown that the quantity of uranium corresponding to 1 gram of radium is 3.8×10^{-7} gram, and is the same for minerals obtained from all parts of the world. Since the radium is always distributed throughout the mass of uranium, we cannot expect to find nuggets of radium like nuggets of gold, unless by some chance the radium has been dissolved out of radio-active minerals and redeposited within the last few thousands of years. To those who had faith in the disintegration theory, this unique constant relation between the amounts of two elements was a satisfactory proof that radium stood in a genetic relation with uranium. A search was then made for the unknown intervening product which, if isolated, must grow radium at a rapid rate. A year or so ago Boltwood observed that a preparation of actinium separated from a uranium mineral did grow radium at a constant but rapid rate. It thus appeared as if actinium were the long-looked-for parent of radium, and that actinium and its long family of products intervened between uranium X and radium. I was, however, able to show that actinium itself was not responsible for the growth of radium, but another unknown substance separated with it. These results were confirmed by Boltwood, who finally succeeded in isolating a new substance from uranium minerals, which was slowly transformed into radium. This substance, which he termed "ionium," has apparently chemical properties similar to those of thorium, and emits α rays of penetrating power less than those of uranium.

The main provisions of the theory have thus been experimentally verified. Radium is a changing substance the amount of which is kept up by the disintegration of another element, ionium. In order to complete the chain of evidence, we require to show that uranium grows ionium, and it is probable that evidence in this direction will soon be forthcoming. We thus see that we are able to link uranium, ionium, radium, and its long line of descendants, into one family, with uranium as its first parent. As uranium has a period of transformation of more than one thousand million years, it will not be profitable at the moment to try and trace back the family further.

It appears almost certain that, from the radio-active point of view, uranium and thorium must be considered as two independent elements. The case of actinium is different, for Boltwood has shown that the amount of actinium in minerals, like the amount of radium, is proportional to the amount of uranium. This indicates that actinium stands in a genetic relation with uranium. Unless our experimental evidence is at fault, it does not appear probable that actinium belongs to the main line of descent of uranium, for the activity of actinium separated from a mineral compared with radium is only about one-quarter of what we should expect under such conditions. I think that a suggestion which I put forward some time ago may account for the obvious connection of actinium with

uranium, and at the same time for the anomaly observed. This supposes that actinium is a branch descent from some member of the uranium family. It does not appear improbable that at one stage of the disintegration two distinct substances may be produced, one in greater quantity than the other. After the expulsion of an α particle, it may happen that there are two possible arrangements of temporary stability of the residual atom. The great majority of the atoms may fall into one arrangement, and the remainder into the other. Actinium in this case would correspond to the substance in lesser quantity. It would act as a distinct element, and would break up in a different way from the main amount. It is probable that a large amount of accurate work will be required before the position of actinium in the scheme of changes can be fixed with certainty. It is a matter of remark how closely actinium resembles thorium in its series of transformations. It would appear that the atom of actinium has many points in common with thorium, or rather with its product, mesothorium.

The recent observations on the growth of radium offer a very simple and straightforward method of determining experimentally the period of radium. Suppose that we take a uranium mineral and determine by the emanation method the quantity of radium contained in it. If the immediate parent of radium (i.e. ionium) is next completely separated from the uranium and radium, it will begin to grow radium at a constant rate. Now the rate of growth of radium observed is a measure of the rate of breaking up of the radium parent in the mineral, since before separation the rate of production was equal to the rate of breaking up. Now the growth of radium observed for a short interval, for example, a year, divided by the quantity present in the mineral, gives the fraction of the radium breaking up per year. Proceeding in this way, Boltwood found that the fraction breaking up per year is about $\frac{1}{3000}$, and that the period of radium is about 2000 years—a value which lies between the most probable values deduced from quite distinct data.

From an inspection of the radio-active families, it will be seen that out of twenty-six radio-active substances that have been identified, seventeen give out α rays or α and β rays, four give out only β rays, and five emit no rays at all. The rayless and β -ray products are transformed according to the same law as the α -ray products, and there is the same sudden change of physical and chemical properties as the result of the transformation. In the case of the substances which throw off atoms of matter in the form of α particles, there are obvious reasons for anticipating a change in properties of the substance, but this is not the case for the ray-less or β -ray products. We must either suppose that the mass of the atom is not appreciably changed by the transformation, which consists in an internal rearrangement of the parts of the atom, or that the atom expels a particle at too low a velocity to be appreciated by the

electrical methods. Unfortunately, it is very difficult to study the rayless products with care, as in practically every case they are succeeded by a ray product of comparatively rapid transformation. The rayless products are of great interest as indicating the possibility of transformations which can occur without any detectable radiation.

In the course of the analysis of radio-active changes, special methods have been developed for the separation of the various products from each other. It is only in a few cases, however, that we can hope to obtain a sufficient quantity of the substance to examine by means of the balance. It should be possible to obtain workable quantities of actinium, radium D (radio-lead), and radium G (polonium), but the isolation of these substances in any quantity has not yet been effected. Sir William Ramsay and Mr. Cameron have made a number of important investigations of the properties and volume of the radium emanation, freed so far as possible from any traces of known gases. The remarkable initial contraction of the volume due to the emanation shows that there is still much to be done to obtain a clear understanding of the behaviour of this intensely radio-active gas when obtained in a pure state.

Simultaneously with the work on the analysis of radio-active changes, a large number of investigations have been made on the laws of absorption by matter of the three primary types of radiation from active matter, viz. the α , β , and γ rays, and the secondary radiations to which they give rise. It has generally been accepted for some years that the γ rays are a type of penetrating X-rays. The latter are supposed to consist of electro-magnetic pulses in the ether, set up by the impact or escape of electrons from matter, and akin in many respects to very short waves of ultra-violet light. Recently, however, Bragg has challenged this view, and has suggested that the γ rays (and probably also the X-rays) are mainly corpuscular in character, and consist of uncharged particles, or "neutral pairs," as he terms them, projected at a high velocity. Such a view serves to explain most of the experimental observations equally well as the pulse theory; Bragg has recently brought forward additional evidence, based on the direction of the secondary radiation from the γ rays, which he considers to be inexplicable by the pulse theory. We must await further data before this important question can be settled definitely, but the theory of Bragg, which carries many important consequences in its train, certainly deserves very careful examination.

From the radio-active point of view, the α rays are by far the most important type of radiation emitted by active matter, although their power of penetration is insignificant compared with the β or γ rays. They consist of veritable atoms of matter projected at a speed, on an average, of 6000 miles per second. It is the great energy of motion of these swiftly expelled masses that gives rise to the heating effect of radium. In addition, they are responsible for the greater part of the ionisation observed near an uncovered radio-active sub-

stance. On account of their importance in radio-active phenomena, I shall devote some little attention to the behaviour of these rays. The work of Bragg and Kleeman, of Adelaide, first gave us a clear idea of the nature of the absorption of these rays by matter. The α particles from a very thin film of any simple kind of radio-active matter are all projected at an identical speed, and lose their power of ionising the gas or of producing phosphorescence or photographic action after they have traversed exactly the same distance, which may conveniently be called the "range" of the α particle. Now every product emits α particles at an identical speed among themselves, but different from every other product. For example, the swiftest α particles from the radium family, viz. that from radium C, travels 7 cm. in air under ordinary conditions before it is stopped, while that from radium itself is projected at a slower speed, travelling only 3.5 cm. We may regard the α particle as a projectile travelling so swiftly that it plunges through every molecule in its path, producing positively and negatively charged ions in the process. On an average, an α particle before its career of violence is stopped breaks up about 100,000 molecules. So great is the kinetic energy of the α projectile that its collisions with matter do not sensibly deflect it, and in this respect it differs markedly from the β particle, which is apparently easily deflected by its passage through matter. At the same time, there is undoubted evidence that the direction of motion of some of the α particles is slightly changed by their passage through matter.

The sudden cessation of the ionising power produced by the α particle after traversing a definite distance of air has been shown by Bragg to be a powerful method of analysis of the number of α -ray products present in a substance. For example, suppose the amount of ionisation in the gas produced by a narrow pencil of α rays is examined at varying distances from the radium. At a distance of 7 cm. there is a sudden increase in the amount of ionisation, for at this distance the α particles from radium C enter the testing vessel. There are again sudden changes in the ionisation at distances of 4.8 cm., 4.3 cm., and 3.5 cm. These are due to the rays from the radium A, the emanation and radium itself respectively entering the testing vessel. The α -ray analysis thus discloses four types of α rays present in radium in equilibrium—a result in conformity with the more direct analysis. This method allows us to settle at once whether more than one α -ray product is present in a given radio-active material. For example, an analysis by Hahn by this method of the radiation from the active deposit of thorium has disclosed the existence of two α -ray products instead of one as previously supposed. We can consequently gain information on the complexity of radio-active material, even though no chemical methods have been found to separate the products concerned. The range of the α particle from each product is a definite constant which is characteristic of each product.

The α particle decreases in velocity as it passes through matter. This result is clearly brought out by photographs showing the deflection of a homogeneous pencil of α rays in a magnetic field before and after passing through an absorbing screen. The greater divergence of the trace of the α rays on the plate, after passing through the screen, shows that their velocity is reduced, while the sharpness of the band shows that the α particles still move at an identical speed.

In order to make an accurate determination of the constants of the α particles, it is necessary to work with homogeneous rays, and we consequently require to use a thin layer of matter of one kind. For experiments of this character, a wire coated with a thin film of radium C by exposure to the radium emanation is very suitable. The velocity of the α particle and the value e/m , the ratio of the charge carried by the α particle to its mass, can be deduced by observing the deflections of a pencil of α rays exposed in a magnetic and in an electric field of known strengths. The deflection of a pencil of α rays in an electric field is small under normal conditions, and special care is needed to determine it with accuracy.

In this way I have calculated the velocity and value of e/m for a number of α -ray products. The velocity of expulsion varies for different products, but is connected by a simple relation with the range of the α particle in air. The value of e/m has been determined for selected products of radium, thorium, and actinium, and in each case the same value has been found. This shows that the α particles expelled from radio-active substances in general are identical in constitution. They have all the same mass, but differ from one another in the initial velocity of their projection. Although we are sure that the α particles, from whatever source, are identical atoms of matter, we are still unable to settle definitely the true nature of the α particle. The value of e/m found by experiment is nearly 5×10^3 . Now the value of e/m for the hydrogen atom in the electrolysis of water is 10^4 . If the charge carried by the α particle and the hydrogen atom is the same, the mass of the α particle is twice that of the hydrogen atom, i.e. a mass equal to the hydrogen molecule. But we are not certain that they do carry the same charge. Here we are, unfortunately, confronted by a number of possibilities, for the magnitude of m for the α particle is conditioned by the value assumed for e . If the charge of the α particle is assumed to be twice the value of the hydrogen atom, the mass comes out four times the hydrogen atom -- the value found for the helium atom. The weight of evidence still supports the view that the α particle is in some way connected with the helium atom. If the α particle is a helium atom with twice the ionic charge, we must regard the helium produced by radio-active bodies as actually the collected α particles the charges of which have been neutralised. This at once offers a reasonable explanation of the production of helium by actinium as well as by radium. In addition, Strutt has recently contributed strong evidence that helium is a

product of thorium. Such results are only to be expected on the above view, since the α particle is the only common product of these elements.

The determination of the true character of the α particle is one of the most pressing unsolved problems in radio-activity, for a number of important consequences follow from its solution. Unfortunately, a direct experimental proof of its true character appears to be very difficult unless a new method of attack is found. We have seen that if the charge carried by the α particle could be experimentally determined, the actual value of m could be determined in terms of the hydrogen atom, since the value of the charge carried by the latter is known. This could be done if we could devise a method of detecting the emission of a single α particle, and thus counting the number of particles expelled from a known quantity of a radio-active substance, for example, from radium. In considering a possible method of attack of this question, the remarkable property of the α particles of producing scintillations in zinc sulphide at once suggests itself. Apart from the difficulty of counting the scintillations, it is very doubtful whether more than a small fraction of the α particles which strike the screen produce the scintillations. Viewed from the electrical side, a simple calculation from the data at our disposal shows that the ionisation produced in a gas by a single α particle should be detectable. The electrometer or electroscope used for measurement would, however, require to be extremely sensitive, and under such conditions it is known that small electrical disturbances are very difficult to avoid.

In order to obtain a reasonably large effect, we require some method of magnifying the ionisation produced by the α particle. In conjunction with Dr. Hans Geiger, I have recently developed a method whereby the electrical effect produced by the α particle can be magnified several thousand times. From the work of Townsend it is known that if a strong electric field acts on gas at low pressure, any ions generated in the gas by an external agency are set in motion by the electric field, and under the proper conditions produce fresh ions by collision with the gas molecules. The negative ion is the most effective ioniser in weak fields, but when the voltage is increased near the point at which a discharge passes, the positive ion also produces fresh ions by collision. In the experimental arrangement the α particle from the active matter is fired through a small opening about 2 mm. in diameter, covered with a thin layer of mica, into a cylinder 60 cm. long and 2.5 cm. in diameter, in which the gas pressure is about 3 cm. of mercury. A thin insulated wire connected to the electrometer is fixed centrally in the cylinder. If the outside cylinder is charged negatively, for a difference of potential of about 1000 volts any ionisation produced in the cylinder is increased about 2000 times by collision. This can be simply illustrated by using the γ rays of radium as a source of ionisation. When a difference of potential is applied to the cylinder, the ionisation produced by the γ

rays only causes a slight movement of the electrometer needle. By applying, however, a voltage nearly equal to that required for a discharge through the gas there is a very rapid movement of the needle. On removing the radium there is no appreciable current through the gas. On placing a source of α rays near the small opening in the cylinder so that some of the α particles can be fired along the axis of the cylinder, the electrometer needle does not move uniformly, but with a succession of rapid throws with a considerable interval in between. Each of these throws is due to the discharge produced by a single α particle entering the cylinder, increased several thousand times by the intermediary of the strong electric field. If a sheet of paper which stops the α rays is placed before the opening, the electrometer needle at once comes to rest. The interval of time between the throws is not uniform. This is exactly what we should expect if the number of α particles entering such a small opening is governed by the law of probability. On the average, a certain number of α particles are fired through the opening per minute, but in some cases the interval is less than the average, in others much greater. In fact, by observing the intervals between the entrance of a large number of α particles, we should be able to determine accurately the "probability" curve of distribution of the α particles with time. For purposes of measurements, the active material, in the form of a thin film covering a small area, is placed in an exhausted tube connected in series with the ionisation cylinder, and at a considerable distance from the hole. The number of α particles entering the opening per minute is counted, and from this the total number expelled can be calculated. Preliminary measurements show that the number of α particles expelled from a known weight of radium is of the same order as the calculated value. When the measurements are completed it should be possible to determine the charge carried by each α particle, since the total charge carried by the α particles from 1 gram of radium is known. In this way it may be possible to settle whether the α particle is a helium atom or not. In any case, it is a matter of some interest to be able to detect by its electrical effect a single atom of matter, and so to determine directly with a minimum of assumption the magnitude of some of the most important quantities in radio-active phenomena.

[E. R.]

GENERAL MONTHLY MEETING,

Monday, February 3, 1908.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and
Vice-President, in the Chair.

The Rt. Hon. Lord Ellenborough,
Alfred Mosely, Esq., C.M.G.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to W. J. Russell, Esq., Ph.D. F.R.S., for his Donation of £100 to the General Fund; and to Charles Hawksley, Esq., M.Inst.C.E., for his Donation of £100 (in commemoration of the 100th anniversary of the birth, on July 12th, 1807, of Thomas Hawksley, F.R.S., Civil Engineer), to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Honorary Secretary reported the decease of the Right Hon. Lord Kelvin, O.M. G.C.V.O. P.C. D.C.L. LL.D. D.Sc. F.R.S., Grand Officer of the Legion of Honour, Chancellor of the University of Glasgow, on the 17th of December, 1907, and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted:—

Resolved, That the Managers of the Royal Institution of Great Britain desire to record at this, their first Meeting subsequent to his death, their sense of the great loss sustained by the Institution and by Science in the decease of Lord Kelvin.

Lord Kelvin became a Member of the Royal Institution in 1886. He gave his first lecture at the Royal Institution at the time when Faraday was engaged in his epoch-making researches on Electricity and Magnetism; and between the years 1856 and 1900 Lord Kelvin delivered a Course of Lectures on The Electric Telegraph in 1863, and no less than nine Friday Evening Discourses on the following subjects: "The Origin and Transformations of Motive Power" (1856), "Atmospheric Electricity" (1860), "Tides" (1875), "Effects of Stress on Magnetisation of Iron, Nickel and Cobalt" (1878), "The Sorting Demon of Maxwell" (1879), "Elasticity viewed as possibly a Mode of Motion" (1881), "Isoperimetrical Problems" (1893), "Contact Electricity of Metals" (1897), "Nineteenth Century Clouds over the Dynamical Theory of Heat and Light" (1900).

On the occasion of the celebration of the Jubilee of his appointment to the Chair of Natural Philosophy in the University of Glasgow, an address of congratulation was presented to Lord Kelvin on behalf of the Members of the Royal Institution, expressing their high appreciation of the conspicuous services rendered by him in the extension and diffusion of Scientific Knowledge.

When Lord Kelvin resigned his Professorship and came to reside in London

he took much interest in the Royal Institution, and became a Manager in 1892.

The Managers desire to offer on behalf of the Members of the Royal Institution the expression of the most sincere sympathy with Lady Kelvin and the family in their bereavement.

The Chairman announced that the Managers had appointed Kenneth Robert Hay, Esq., M.B. (Cambridge), Medical Officer to the Royal Institution in succession to the late Dr. Woodhouse Braine, who held the appointment for thirty-six years.

The Honorary Secretary read the following Letters received from the Honorary Members who were elected at the General Meeting on December 2, 1907 :—

Paris, *December 7, 1907.*

Sir, and honoured Colleague,

I am greatly touched by the high honour which the Royal Institution of Great Britain has just done me by enrolling me amongst its Honorary Members, and I beg that you will communicate my best feelings of gratitude to His Grace the Duke of Northumberland and to your fellow Members.

Accept, my dear Colleague, the expression of my very devoted sentiments.

A. HALLER.

Paris, *December 8, 1907.*

My dear Colleague,

Will you be good enough to express my best thanks for my election as Honorary Member of the Royal Institution of Great Britain, which you announce to me.

Accept, my dear Colleague, the expression of my most distinguished sentiments.

L. TROOST,

Member of the Institute.

Mr. William Crookes,

Secretary of the Royal Institution.

Liège, *December 9, 1907.*

Dear Sir, and most honoured Colleague,

I have received, after some delay, due to absence on my part, the letter dated the 2nd of this month in which you were good enough to inform me that the Members of the Royal Institution of Great Britain had done the marked honour of associating me with themselves in the capacity of Honorary Member.

This high distinction is inexpressibly valuable to me in view of the world-wide reputation with which the Royal Institution is invested. I am profoundly touched and sincerely grateful. I have always professed enthusiastic admiration for the admirable work done by English men of Science for so many generations, the importance of which has never diminished, so that Great Britain remains a shining star in the scientific world, as it is indeed in every domain of intellectual activity.

Will you, Mr. Secretary, be good enough to interpret my feelings to your illustrious colleagues, and thank them most cordially for the honour they have done me. I would be happy if the powers still at my disposal permit me to rise as I should like to do, to the distinction which has just been conferred on me, and to afford proof of the interest I bear towards the prosperity of the Royal Institution.

Accept, dear Sir, the assurance of my high consideration, and the expression of my best and most fraternal sentiments.

W. SPRING.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. : -

FROM

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- American Geographical Society*—Bulletin, Vol. XXXIX. Nos. 11-12. 8vo. 1907.
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- British Astronomical Association*—Journal, Vol. XVIII. Nos. 2-3. 8vo. 1907.
- Buenos Aires*—Monthly Bulletin of Statistics, Sept.-Oct. 1907. 4to.
- Burgoyne, A. H., Esq. (the Editor)*—The Navy League Annual, 1907-8. 8vo. 1907.
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- Carnegie Institution*—Contributions from Solar Observatory, No. 20. 8vo. 1907.
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- A Sequel to Christabel. A Review by L. 8vo. 1899.
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- Chemical Society*—Journal for Dec.-Jan. 1907-8. 8vo.
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- Clinical Society*—Transactions, Vol. XL. 8vo. 1907.
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 Dioptric Review for Dec.-Jan. 1907-8. 8vo.
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 Electrical Industries for Dec.-Jan. 1907-8. 4to.
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- Washington Academy of Sciences*—Proceedings, Vol. X. pp. 1-50. 8vo. 1907-8.
- Washington Philosophical Society*—Bulletin, Vol. XV. pp. 57-74. 8vo. 1907.
- Western Australia, Agent-General*—Supplement to Government Gazette for Nov.-Dec. 1907. 4to.
- Monthly Statistical Abstract for Sept.-Oct.-Nov. 1907. 4to.
- Western Society of Engineers*—Journal, Vol. XII. No. 5. 8vo. 1907.
- Yorkshire Archaeological Society*—Journal, Vol. XIX. Part 4; Part 76. 8vo. 1907.
- Report for 1907. 8vo. 1908.

WEEKLY EVENING MEETING.

Friday, February 7, 1908.

SIR JAMES CRICHTON-BROWNE, M.D., LL.D., F.R.S., Treasurer
and Vice-President, in the Chair.

HUMPHRY WARD, Esq., M.A.

Napoleon and the Louvre.

THE lecturer touched upon the early attempts, under Louis XV. and Louis XVI., to form a museum in the Salon Carré and the Long Gallery, and described how, on the motion of Barère, this was first carried out by the Convention in 1791. At first the museum contained only the works of art belonging to the Crown, mostly removed from Versailles; but three years later, during the campaigns in the Netherlands, the Republican Government formally adopted the policy of seizing and annexing any masterpieces in the invaded countries. The Representatives of the People with the armies of the north boldly declared that any masterpieces existing in the countries "where the victorious armies of the French Republic have just chased away hordes of slaves hired by tyrants," could find their only proper home "in the dwelling place and in the possession of free men," i.e. in Paris; and accordingly their Commissioner, Lieutenant Barbier, took all the Rubenses and Van Dycks he could find, and justified himself at the bar of the Convention by declaring that "these masterpieces had been too long sullied by beholding servitude." The same treatment was applied to the libraries and galleries of Aix-la-Chapelle, Louvain, and Cologne; and from the University of Louvain alone 5000 volumes were taken. So was the whole collection of the Statholder William V. when he left Holland on the approach of the French troops. In Italy, from the very beginning of the campaign, Bonaparte adopted the same policy, and carried it out with amazing thoroughness, although some protests were raised in Paris, and the well-known scholar, Quatremère de Quincy, got fifty artists to join him in a petition that works of art should not be taken.

The lecturer showed lantern slides from engravings of the time representing the stripping of the Parma Gallery, the taking away of the bronze horses from St. Mark's in Venice, and the journey of a convoy down the Tiber valley; and he proceeded to describe Bonaparte's Treaty of Tolentino, in 1797, by which he forced the Pope to surrender a hundred works of art at the conqueror's choice.

Another slide showed the famous Fête de la Liberté, in which the procession of cars bearing these trophies was received in Paris on its way to the Louvre. The lecturer then spoke of the stripping of the German and Austrian galleries after Austerlitz and Jena, when 299 pictures were taken from Cassel alone; and he briefly described the Musée when it was at last completed in 1810, and as it was when crowds of English visitors, including artists such as Lawrence and Chantrey, visited it in 1814. Up to this point he had drawn chiefly on the writings of the late Eugène Müntz, the late Frederic Villot, and M. Saunier, who has collected all the documents in his book, '*Les Conquêtes Artistiques de la Révolution et de l'Empire*'; and he also showed a fine copy of the splendid official publication, '*Le Musée Français*.' In his description of the breaking up of the museum by the victorious Allies, he partly followed Saunier and partly the '*Letters and Papers*' of the Scotch miniature painter, Andrew Robertson, who was an eye-witness. On the first Restoration, in 1814, the Allies agreed not to disturb the museum; but the Hundred Days changed the position, and after Waterloo they determined to claim their own, Blücher and his Prussians being particularly unbending, supported, in the interests of Belgium and Holland, by the Duke of Wellington. The dramatic interest of the story, said the lecturer, lay in the courage and ingenuity with which the interests of the Musée were defended by its celebrated administrator, Vivant Denon, who fought a losing battle with extraordinary skill, and succeeded in the end in saving a great many pictures and other works of art which ought by rights to have gone back. His resistance in Paris was at all events so obstinate that it probably prevented the Allies from seeking to break up the provincial museums, many of which still contain works taken by the French armies. The lecturer concluded by an account of that curious affair, the removal of the bronze horses of St. Mark's from their position on the top of the Triumphal Arch in the Place du Carrousel, which was carried out by the Austrians, as rulers of Venice, and excited the Parisian population almost to insurrection. In the end the horses were returned to their place, as were the Raphaels to Rome, the Correggios to Parma, and the Rembrandts to Cassel; and since that time, by a sort of tacit agreement among the nations, works of art have never been regarded as the proper spoils of war.

[H. W.]

WEEKLY EVENING MEETING,

Friday, February 14, 1908.

ALEXANDER SIEMENS, ESQ., M.Inst., C.E., Vice-President,
in the Chair.

CALEB WILLIAMS SALEEBY, ESQ., M.D., F.R.S.E.

Biology and History.

[ABSTRACT.]

YOU will not expect me to insult you this evening with any discussion of the garbage and gossip, records of scoundrels and courts and battles, murder and theft, which we were taught at school under the name of History. If history be, as nearly all historians have conceived it, and as Gibbon defined it, "little more than the register of the crimes, follies and misfortunes of mankind," it is an empty and contemptible study, save for the social pathologist. But if history, without by any means ignoring great men or underrating their influence, is, or should be, the record of the past life of mankind, of progress and decadence, the rise and fall of empires and civilisations, and their mutual reactions; if it be the record of the intermittent ascent of man, "sagging but pertinacious"; if this record be subject to the law of causation, and therefore susceptible, in theory at least, of explanation as well as description; if its factors are at work to-day, and will shape the destiny of all the to-morrows; if it be neither phantasmagoria, nor panorama, nor pageant, nor procession, but *process*—in short, an organic drama—then, indeed, it is a supreme study. Especially must it appeal to us, who boast a tradition greater than the world has ever yet seen, and kinship with men who represent the utmost of which the human spirit has yet shown itself capable—who speak the tongue that Shakespeare spake, but to whom the names of all our imperial predecessors, from Babylon to Spain, serve as a perpetual *memento mori*. My special question, this evening, is whether there are inherent and necessary reasons why our predecessors' fate must sooner or later be ours? Must races die?—or, if we are sceptical about races, and more especially about the so-called Anglo-Saxon race, must civilisations, states or nations die?

Nations, races, civilisations rise, we shall all agree, because to inherent virtue of breed they add sound customs and laws, acquirements of discipline and knowledge. But, these acquirements made, power

established, and crescent from year to year—why do they *then* fall? If they can *make* a place for themselves, how much easier should it not be to *maintain* it?

Two explanations, each falsely asserting itself to be rooted in biological fact, have long been cited, and are still cited, in order to account for these supreme tragedies of history.

The first—cited by no less a thinker than Mr. Balfour the other day—may claim Plato and Aristotle as its founders, and consists of an argument from analogy. Races may be conceived in similar terms to individuals. There are many resemblances between a society—a “social organism,” to use Herbert Spencer’s phrase—and an individual organism. Just, then, as the individual is mortal, so is the race. Each has its beginning, its period of youth and growth, its maturity, and finally, its decadence, senility and death. So runs the common argument.

Biology, however, so far from confirming it, declares as the capital fact which contrasts the individual and the race, that whilst the individual is doomed to die from inherent causes, the race is naturally immortal. The tendency of life is not to die, but to live. If individuals die, that is doubtless because more life and fuller is thus attained than if life bodied itself in immortal forms; but the germ-plasm *is* immortal; it has no inherent tendency either to degenerate or to die. Species exist and flourish now which are millions of years older than mankind. “The individual withers, the race is more and more.” The most conspicuously persistent of all races during the last two millenia, the Jews, have survived one empire after another of their oppressors, but have never had an empire of their own. Thus, so far as the historian is concerned, it is not races that die, but civilisations and empires. Plato’s analogy between the individual and the race is therefore irrelevant, as well as untrue. The fatalistic conception to which it tempts us, saying that races must die, just as individuals must, and that therefore it is idle to repine or oppose, is utterly unwarrantable, and extremely unhealthy. To take our own case, despite the talk about our own racial decadence, our babies still come into the world fit and strong and healthy. We kill them in scores of thousands every year, but this infant mortality is not a sign that the race is dying, but a sign that even the most splendid living material can be killed or damaged, if you try hard enough. The babies do not die because races are mortal, but because individuals are—and we kill them. The babies drink poison, eat poison, and breathe poison, and in due course die. The theory of racial senility, inapplicable everywhere because untrue, is most of all inapplicable here. If a race became infertile, Plato and Aristotle would be right. There is no such instance in history apart from well-defined external, *not inherent*, causes, as in the case of the Tasmanians. Dismissing this analogy, we may also dismiss, as based upon nothing better, the idea that the great tragedies of history

were necessary events at all. We must look elsewhere than amongst the inherent and necessary factors of racial life for the causes which determine these tragedies; and we shall be entitled to assume as conceivable the proposition that, notwithstanding the consistent fall of all our predecessors, these causes are not inevitable, but being external and environmental, may possibly be controlled.

The second of the two false interpretations of history in terms of biology is still, and always has been, widely credited. It is that, in consequence of success, a people becomes idle, thoughtless, unenterprising, luxurious; and that these *acquired characters are transmitted* to succeeding generations, so that finally there is produced a degenerate people unable to bear the burden of empire, and then the crash comes. The historian usually introduces the idea already dismissed, by saying that a "young and vigorous race" invaded the imperial territories, and so forth. The terms "young" and "old," applied to human races, usually mean nothing at all.

This doctrine of the transmission to children of characters acquired by their parents, is the explanation of organic evolution advanced by Lamarck rather more than a century ago. It is employed by historians for the explanation of both the processes they record, progress and retrogression. Thus they suppose that for many generations a race is disciplined, and so at last there is produced a race with discipline in its very bone; or for many generations a nation finds it necessary to make adventure upon the sea, and so at last there is produced a generation of predestined sailors with blue water in its blood. And, in similar terms, moral and physical retrogression or degeneration are explained.

Let us consider the contrast between the interpretation which accepts the Lamarckian theory of the transmission of acquired characters and that which does not. Consider the babies of a new generation. According to Lamarck, they have in their blood and brain the consequences of the habits of their ancestors. If these have been idle and luxurious, the new babies are predestined to be idle and luxurious too. This, in short, is a "dying nation." But, if acquired characters are not transmitted, the new generation is, on the whole, not much better, not much worse, than its predecessors, so far as this supposed factor of change is concerned. Each generation makes a fresh start, as we see in the babies of our slums to-day.

Lamarck's theory is discredited. The view of Mr. Francis Galton is accepted, that acquired characters are not transmitted, either for good or for evil. If there are no other factors of racial degeneration or racial advance, then races do not degenerate or advance, but make a fresh start every generation, and empires rise and fall without any relation to the breed of the imperial people—an incredible proposition.

Certain apparent though not real exceptions exist to the denial of the Lamarckian theory of the transmission of acquired characters.

These exceptions are furnished by what may be called the *racial poisons*. Alcohol, for instance, is a substance, certainly poisonous in all but very small doses, which is carried by the blood to every part of the body, and may and does injure *its racial elements*. Thus a true racial degeneration may be caused. Other poisons, such as those of certain *diseases*, act similarly.

We must therefore note, in passing, a biological factor of historical importance, hitherto scarcely recognised by historians. Certain of our diseases, and especially consumption or tuberculosis, are at present making history by their extermination of aboriginal races. Minute living creatures, which we call microbes, are introduced into the new and favourable environment constituted by the blood and tissues of human races hitherto unacquainted with them, and the consequences are known to all. But, further, it has lately been suggested as highly probable, by Professor Ronald Ross, that the fall of Greece, that incalculable disaster for mankind, was due to the invasion not of human foes, but of the humble living species which are responsible for the disease miscalled malaria. Malaria, like alcohol, produces true racial degeneration, its poisons affecting *those racial elements* of which the individual body, as biologically conceived by Weismann, is merely the ephemeral host—recalling the great line of Lucretius, "*Et quasi cursores, vitai lampada tradunt.*" To lame the runner is not to injure the torch he bears—acquired characters are not transmitted; but the racial poison makes dim the lamp ere he passes it on.

But, leaving poisons out of the question, races of men and animals *do* undergo change, progressive and retrogressive, in consequence of the action of another factor than that advanced by Lamarck; and this is the factor of "natural selection," so termed by Charles Darwin in 1858, exactly half a century ago; or "survival of the fittest," to use Herbert Spencer's phrase. If, of any generation, individuals of a certain kind are chosen by the environment for survival and parentage, the character of the species will change accordingly. If what we call the best are chosen, their goodness will be transmitted in some degree, and the race will advance; if what we call the worst are chosen, their badness will be transmitted in some degree, and the race will degenerate.

Now in the case of all species other than man the only possible progress is this *racial or inherent progress*, which is dependent upon a choice or selection of the best for parents, and is comparable in some measure, as Darwin showed, with the change similarly produced by the selective breeding, or "artificial selection," of the lower animals by man. But in the case of man himself, there is a wholly different kind of progress also attainable, which is not inherent or racial progress at all, but yet is real progress; and which has the most important relation to the inherent or racial progress that may be achieved by the process of natural selection, or the choice of parents. *The dis-*

inction between these two kinds of human progress is as cardinal as it is hitherto ignored.

It was said just now that acquired characters are not transmissible by heredity ; but man has learnt to circumvent the laws of heredity by transmitting his spiritual acquirements through language and art. Even before writing, there was tradition passed on from mouth to mouth. As long as man was speechless he advanced, I believe, no faster than other creatures—we know that he has an undistinguished past of some hundreds of thousands of years : but with speech and writing came the transmission of acquirements in this special sense. The past education of a mother will not enlarge her baby's brain, but she can teach her daughter what she has learnt, and so the child can, in a sense, begin where the parent left off—in analogy with what Lamarck wrongly imagined to be the case with the young giraffe, that was supposed to profit by the stretching of the parental necks. It is this transmission of spiritual acquirements—outside the germ-plasm, and notwithstanding its laws—that explains the amazing acquired progress of man in the last ten or twenty thousand years, as compared with three or perhaps five hundred thousand before them.

This kind of progress is peculiar to man ; it is the gift of intelligence, and it may be called *traditional or acquired progress*. It is an utterly different thing from *inherent or racial progress*, an improvement in the breed dependent upon the happy choice of parents. And it is surely evident that *acquired progress is compatible with inherent decadence*. To use Coleridge's image, a dwarf may see further than a giant if he sits on the giant's shoulders ; yet he is a dwarf, and the other a giant. Any schoolboy now knows more than Aristotle, and that is true progress of one kind, but the schoolboy may well be a dwarf compared with Aristotle, and may belong to a race degenerate when compared with his : and that would be *inherent or racial decadence subsisting with acquired or traditional progress*.

Now whilst the accumulation of knowledge and art and invention from age to age is real progress, it evidently depends for its security upon the quality of the race. If the race degenerates—whether through a racial poison, alcohol or malaria, or through, say, the selection of the worst for parentage—the time will come when its heritage is too much for it. The pearls of the ancestral art are now cast before swine, and are trampled on ; statues, temples, books, are destroyed, or burnt, or lost. If an empire has been built, the degenerate race cannot sustain it. *There is no wealth but life ; and if the inherent quality of the life fails, neither battle-ships, nor libraries, nor symphonies, nor Free Trade, nor Tariff Reform, nor anything else, will save a nation*. Empires and civilisations, then, may have fallen, despite the strength and magnitude of the superstructure, because their living foundations became weak ; and the bigger and heavier the superstructure, the less could it survive the failure of the foundations. If the Fiji islanders degenerate, there is little consequence ; if the breed of

Romans degenerate, all their vast mass of acquired progress and power crushes them into dramatic ruin. Acquired progress will not compensate for racial or inherent decadence. If the race is going down, it will not compensate to add another dependency to your empire; on the contrary, the bigger the empire, the stronger must be the race: the bigger the superstructure, the stronger the foundations. Acquired progress is real progress, but it is always dependent for its maintenance upon racial or inherent progress—or, at least, upon racial maintenance.

It is submitted that civilisations and empires have succumbed because they represented only acquired or traditional progress; and this availed not at all when, for instance, the races that built them up began to degenerate. And, apart from the action of racial poisons, the only explanation of racial degeneration yet considered by the historians is the Lamarckian one of the transmission of acquired habits of luxury and idleness from parent to child: an explanation which the modern study of heredity empowers us to repudiate. What theory of this alleged degeneration is there to offer in its place? and especially what theory which explains racial degeneration amongst not the conquered but the conquerors, amongst the successful, the imperial, the cultured, the leisured—the well-catered-for in all respects, bodily and mental? *Why is it that not enslaved, but imperial peoples degenerate? Why is it that nothing fails like success?*

The true and sufficient answer has been given by no academic historian: but the clue to it was given half a century ago by the greatest historian of all time, Charles Darwin. The reason is that *no race or species, vegetable or animal or human, can maintain its organic level, let alone raise it, unless its best be selected for parentage.*

We know that, as individuals, we must struggle or we degenerate. "Work is the law," as Ruskin said, whether for a livelihood or for enjoyment. Living things are the product of the struggle for existence: we are thus evolved strugglers by constitution; and directly we cease to struggle, we forfeit the possibilities of our birth-right. "Thou, O God," said Leonardo, "hast given all good things to man at the price of labour."

The case is the same with races or nations. Directly the conditions become too easy, selection ceases: it is as successful to be incompetent or lazy or vicious as to be worthy. The hard conditions that kept weeding out the unworthy are now relaxed, and the fine race they made goes back again. There even occurs the phenomenon of *reversed selection*, when it is positively fitter to be bad than good, cowardly than brave—as when religious persecution murders all who are true to themselves, and spares hypocrites and apostates; or when healthy children are killed in factories, or by their mother's work in factories, whilst feeble-minded children or deaf-mutes are carefully tended until maturity and then sent into the world to reproduce their maladies. Under reversed selection such results are obtained as a

breeder of racehorses or plants would obtain if he went to work on similar lines; the race degenerates rapidly, and if it be an imperial race its empire comes crashing down about its ears. All empires and civilisations hitherto have involved the risk of partial or complete arrest or reversal of the process of natural selection; and, in the cases where their doom has been irretrievable, it is the racial degeneration so produced that has been its cause.

When a race is making its early way by force, as by incessant war, selection is stringent. The weak, cowardly, diseased, stupid, are ruthlessly expunged from generation to generation. As civilisation advances, a higher ethical level is reached—all true civilisation tending to abrogate and ameliorate the struggle for existence. The diseased and weakly and feeble-minded are no longer left to pay the penalty sternly exacted by Nature for unfitness: they are allowed to *survive*, which is well; and to *multiply*, which is ill. A successful race can apparently afford to permit this, as a race that is fighting for its existence cannot. *But in reality no race can afford this absolutely fatal process*; especially when unchallenged success comes, and even interferes with the natural process of selection to the extent of not merely abrogating, but actually reversing it, so that it may be more advantageous—more fit—to be a coward, or an idler, or diseased, or feeble-minded, than the reverse.

The fittest survive in any case; but fitness is not goodness. It may be, but it may be badness. Fitness is merely the capacity to fit—to fit the environment. That society in which it is fittest to be best is safe; that society in which it is equally fit to be good, bad, or indifferent is doomed; that society in which it is fittest to be worst is already damned. A nation will ascend, under the influence of selection which is such that the fittest selected are also the best; a nation will degenerate, under the influence of selection which is such that the fittest selected are also the worst. A nation will even degenerate if selection be merely abrogated, and universal survival or indiscriminate survival be substituted for any process of selection at all.

If a nation can ascend in any sure way (its surety being dependent upon the fact that the ascent is in the very blood of the people) only when natural selection actively operates in the choice of the best for survival and parentage, then we begin to realise why it is that in the whole course of history hitherto this sure ascent has scarcely been realised. Babylon may have lasted for 4000 years, as the historians tell us, yet at last it fell. If selection had been operating in Babylon throughout that time, choosing only the best, the noblest and the wisest, conferring upon them, and upon them alone, the supreme privilege and duty of parentage, could Babylon have fallen?

Hence the explanation of the truth expressed by Gibbon, "All that is human must retrograde if it do not advance." Why should this be so? Why should it not be possible merely to maintain a

position gained? The answer is, that the civilisation which merely maintains its position is one in which selection has ceased; if selection had not ceased, the position would be more than maintained, there would be advance. But without selection the breed will certainly degenerate, the lower individuals multiplying more rapidly than higher ones, in accordance with Spencer's law that the higher the type of the individual the less rapidly does he multiply; and thus the race which is not advancing is retrograding, as Gibbon declared.

The selection of the best for parentage is the sole factor of inherent or racial progress; but the traditional or acquired progress, which we call civilisation, tends to thwart or abrogate or even invert this process. Thus the conditions necessary for the *secure* ascent of any race, an ascent secured in its very blood, made stable in its very bone, have not yet been achieved in history; *and this is the reason why history records no enduring empire.*

It is not for a moment asserted that there are no other causes of imperial failure than the arrest or inversion of selection. But if this is not the cause, then, in the absence of the transmission of acquired characters, the race has not degenerated, and is capable of reasserting itself. Only by the arrest or inversion of selection can a race degenerate—apart from alcohol and certain diseases. If, then, a civilisation or empire has fallen through causes altogether non-biological—through carelessness, or neglect of motherhood or alteration of ideals—the changes in character so produced are not transmitted to the children, and the race is not degenerate, but merely deteriorated in each generation.

For instance, we have been brought up to believe that there is no possible future for Spain—it is a dying nation, a senile individual, a people of degenerates: it has had its day, which can never return. The historian explains this by a fallacious use of the analogy between a race and an individual, and by the false Lamareckian theory of heredity. But the biologist believes that since Spain has not been subjected—or, at any rate, not subjected long enough—to the only process which can rapidly ensure real degeneration, viz. the consistent and stringent selection of the worst, she is yet capable of regeneration. Regeneration is not really the word, because there has been no real degeneration, but only the successive deterioration of successive and undegenerate generations.

If we took an animal species that *has* degenerated, such as the intestinal parasites, and endeavoured to regenerate them, we should begin to realise the magnitude of our task. That is not the task for Spain, the biologist asserts. Merely the environment must be altered—not the mountain ranges and the rivers, Buckle notwithstanding, but the really potent factors in the environment, the spiritual and psychical and social factors—and the deterioration of each new generation, inherently undegenerate, will cease.

And the biologist is right. The "dying nation" alters its psychical environment. It introduces the practice of education, it begins to shake off the yoke of ecclesiasticism; and what are the consequences?

The new generation is found to be potentially little worse, and little better, than its predecessors of the sixteenth century. There has been no racial degeneration. The environment is modified for the better, *i.e.* so as to choose the better, and Spain, as they say in misleading phrase, "takes on a new lease of life."

But the historian might well write a volume upon the same thesis as applied to China and Japan. The popular belief used to be, that China illustrated the so-called law of nations. It was the decadent, though monstrous, relic of an ancient civilisation; it had had its day; inevitable degeneration, which must befall all peoples, had come upon it. Behold it in the paralysis which precedes death!

But in the light of the facts of Japan, and such a phrase as "the yellow peril," we have discarded our old theories. The metaphor must be changed. This is not paralysis, but merely stupor. It is suspense, not recuperation; but assuredly it is not paralysis. Who now would dare to say that China has had its day, even if he still clings to the old fictions about Spain?

There is another factor of history to which, I believe, the biologist must attach enormous importance, but which no historian yet has adequately reckoned with. The prime assumption of this lecture from beginning to end, is that "there is no wealth but life;" and, in the attempt to suggest interpretations of history based upon this truth, so little recked of by the historian, we have considered the life in question from the point of view of its determination by heredity, and its varying value according to the inherent and transmissible characters selected for perpetuation in each generation. But a word must be said as to the other factor which, with heredity, determines the character of every individual—and that factor is the environment. We must note the most important aspect of the environment of human beings, and observe that historians hitherto have wholly ignored it; yet its influence is incalculable. This is motherhood.

It is man's intelligence that has made him lord of the earth; it is qualities of intelligence that have largely determined the course of history as wrought out between human races and civilisations. Now intelligence is a limitless thing—it can learn everything; *but*, it has everything to learn. The lower animals have instincts—neither needing nor capable of education, but in order that intelligence shall be possible, instinct must make room for it. Thus, at birth we human beings have nothing; intellect being only potential, not actual, and instinct having almost entirely lapsed. We come into

the world more helpless and incompetent than the young of any other living creature ; the human baby is a fraction more helpless even than the baby ape. A later age may reveal a Newton or a Kelvin, a Shakespeare or a Goethe ; but all were helpless ignorant babies at first, unable even to find their way to the mother's breast that was made for them. Thus motherhood, the importance of which has been steadily rising throughout the ages, and is enormous in all the mammals, is supremely important for the highest of the mammals, which is man. No motherhood, no intelligence. You may have the most perfect system of selection of the finest and highest individuals for parentage ; but the babies whose potentialities—heredity gives no more—are so splendid, are always, will be always, dependent upon motherhood. What was the state of motherhood during the decline and fall of the Roman empire ? This factor counts in history ; and will always count, so long as three times in every century the only wealth of nations is reduced to dust, and begins again in helpless infancy. As to Rome we know little, whatever may be suspected : but we know that here, in the heart of the greatest empire in history—and it is at the heart that empires rot—thousands of mothers go out every day to tend dead machines, whilst their own flesh and blood, with whom lies the imperial destiny, are tended anyhow or not at all. To-day our historians and politicians think in terms of regiments, and tariffs, and “Dreadnoughts” ; the time will come when historians think in terms of babies and motherhood. We must think in such terms, too, if we wish Great Britain to be much longer great. A history of motherhood is yet to seek. Meanwhile, who will not deplore the perennial slaughter of babies in this land, the deterioration of many for every one killed outright, the waste of mothers' travail and tears ?

Had all Roman mothers been Cornelias, would Rome have fallen ? Consider the imitation mothers—no longer mammalia—to be found in certain classes to-day—mothers who should be ashamed to look any tabby cat in the face : consider the ignorant and downtrodden mothers amongst our lower classes ; and ask whether these things are not making history. Who will join the new party of one that calls itself *maternalist* ?

These principles find their warrant and application in the unexampled riddle of the persistence and success, throughout more than two thousand years and a thousand vicissitudes, of the Jewish people. It is true that we have here no exception to the *apparent* law that empires are mortal, for there never was a Jewish empire ; the Jews were never subject to the risk involved for racial or inherent progress, by the possession of great acquired powers leading to the arrest of struggle and selection. But just as the fall of empires has often not been the fall of races—various races having at various times carried on the same imperial tradition—so the persistence of the Jews, as

contrasted with the impermanence of empires, has been the persistence of a race.

It has been asserted that that race of people decays in which selection ceases or is inverted: that in the absence of selection of the fittest for life and parentage, no species, vegetable, animal or human, can prosper, much less progress. Now the Jews, the one human race of which we know assuredly that it has persisted unimpaired, have been the most continuously and stringently selected of any race that can be named. Every measure of persecution and repression, practised against them by the peoples amongst whom they have lived, has directly tended towards the very end which those peoples least desired to compass. Other peoples found themselves prosperous through the efforts of their fathers; the struggle for existence abated; it was, so to say, as fit to be unfit as to be fit—with the inevitable result, racial decadence. But this has never been the case of the Jews. They have always had to struggle for life intensely, and their unexampled struggle has been a great source of their unexampled strength. The Jew who was a weakling or a fool had no chance at all; the weaklings and the fools being weeded out, intensity and strength of mind became the common heritage of this amazing people.

Secondly, there was everything to favour motherhood. Here religious precept and ethical tradition joined with stern necessity to the same end—the end which always meant a new and strong beginning for the next generation. Even to-day all observers are agreed that infant mortality is at a minimum among the Jews; their children are superior in height and weight and chest measurement, to Gentile children brought up amidst poverty far less intense in our own great cities—in a *better material environment, but a far inferior maternal environment*. The Jewish mother is the mother of children innately superior, on the average, since they are the fruit of such long ages of stringent parental selection: and she makes more of them because she fails to nurse them only in the rarest cases, when she has no choice, and because in every detail her maternal care is incomparably superior to that of her Gentile sister. Given a high standard of motherhood, in a highly selected race, what other result than that we daily witness and envy can we expect?

Thirdly, the Jews do not abuse alcohol; and thus avoid one of the few causes of true racial degeneration, apart from arrest of selection or selection of the worst, for parentage.

If these principles are valid, it is evident that our redemption from the fate of all our predecessors is to be found only in what Mr. Francis Galton calls eugenics—the selection of the best for parentage. The appropriateness of Mr. Galton's relation to this question is unmistakable. As advocate of the principle of selection, he is the cousin of Charles Darwin, and he is the author of the theory that acquired characters are not transmitted, and therefore that selection alone

changes races—that mere education is a Sisyphean task, which has to be done all over again from the beginning in each generation.

Using the word environment in its widest sense, including, for instance, public opinion—and its use in any sense less wide is always erroneous and misleading—we must surely see that it is our business to provide the environment which selects the best for parentage and discourages the parentage of the worst: say, to begin with, the deaf and dumb, the feeble-minded, the insane, the epileptic, the inebriate, those afflicted with hereditary disease of other kinds, and so forth. Our principles should enable us, also, to define what we mean by good environment. Comprehensive and indiscriminate charity means a good environment for many in a sense, but it may also mean the selection of the worst for parentage—e.g. the feeble-minded. This good environment, then, means the degeneration of the race. We must therefore *appraise environment in terms of its selective action*. A good environment is that which selects the good, and the best environment is that which selects the best: discovers them, makes the utmost of them, and confers upon them the supreme privilege and duty of parentage. That, and that alone, is the best environment; and all other moral judgments upon environment are fallacious, and will be disastrous.

The new law of love need not go, the brutal struggle for existence need not be restored, we need not be damned to be saved. *The unfit must survive, but they must not multiply*. We need only follow the Lancashire society which now cares for the feeble-minded *all their days*, and thus serves the present and the future simultaneously.

Eugenics, or “good breeding,” is Mr. Francis Galton’s name for the science of race-culture, which assumes that there is no wealth but life, and that the first duty of all governments and patriots, and good citizens is, to quote Ruskin again, “the production and recognition of human worth, the detection and extinction of human unworthiness.” The idea is not new-fangled, but was clearly laid down by Plato, and by Theognis two centuries before him. The modern expression of it is now nearly a quarter of a century old, and it has already passed the stage of ridicule, except by the ridiculous.

Eugenics is a project of the most elevated and provident morality, aiming at no object less sublime than the ennoblement of mankind; and if one may suggest its motto, it would be, *The products of progress are not mechanisms but men*. It aims at “working out the beast.” It is based upon the principle of the selection or choice of the superior for parentage, which has been the essential factor of all progress in the world of life, but which all civilisations have tended, in some degree, to abrogate, or even to invert—as when the feeble-minded child is cared for till maturity, and sent out into the world to produce its like, whilst healthy children are daily destroyed by ignorance and neglect.

"Through Nature only can we ascend," and the merit of the eugenic proposal is that it is built upon "the solid ground of Nature."

To the economist, it declares that *the culture of the racial life is the vital industry of any people*.

It is to work through marriage, an institution more ancient than mankind, and supremely valuable in its services to childhood—with which lies all human destiny. Neither Mr. Galton, nor the recently founded Eugenics Education Society, countenances for a moment the insane and vicious proposals which falsely assume that the methods of the stud farm are applicable to man—who is not an erected horse.

Eugenics appeals to the individual, asking for a little imagination—to make us realise that the future will one day be the present, and that to serve it is to serve no fiction or phantom, but a reality as real as the present generation.

It teaches the responsibility of the noblest and most sacred of all professions, which is parentage, and it makes a sober and dignified claim to be regarded as a constituent of the religion of the future.

It goes to the root of the matter: where the well-meaning, but short-sighted, pin their faith on the hospitals, the eugenicist seeks to brand the transmission of hereditary disease as a crime, and thus *literally* to extirpate it altogether.

That its methods are practicable is proved by the fact that it is practised—as by the northern society for the "*permanent* care of the feeble-minded," just referred to.

National eugenics offers, I submit, our sole chance of escape from the fate which has overtaken all previous civilisations; and suggests the principles of a New Imperialism. It honours men and women, by declaring that human parentage is crowned with responsibility to the unborn, and to all time coming: and that man, the animal in body, is also a self-conscious being, "looking before and after," who is human because he is responsible; and to whom the laws of nature have been revealed, not to satisfy an intellectual curiosity, but for the highest end conceivable—the elevation of his race.

Says Wordsworth:—

"Having brought the books
Of modern statist to their proper test,
Life, human life, with all its sacred claims;

And having thus discerned how dire a thing
Is worshipped in that idol proudly named
'The Wealth of Nations'; where alone that wealth
Is lodged, and how increased: and having gained
A more judicious knowledge of the worth
And dignity of individual man,

I could not but inquire.

Why is this glorious creature to be found
One only in ten thousand? What one is,
Why may not millions be? What bars are thrown
By Nature in the way of such a hope?"

Consider how far we have come, the base degrees by which we did ascend, and answer with Shakespeare, "There are many events in the womb of Time which will be delivered."

[C. W. S.]

WEEKLY EVENING MEETING,

Friday, February 21, 1908.

THE RIGHT HON. LORD RAYLEIGH, O.M. P.C. M.A. D.C.L. LL.D.
Sc.D. Pres.R.S., in the Chair.

SIR OLIVER LODGE, LL.D. D.Sc. F.R.S. *M.R.I.*

The Ether of Space.

[ABSTRACT.]

THIRTY years ago Clerk Maxwell gave in this place a remarkable address on "Action at a Distance." It is reported in your Journal, Vol. VII., and to it I would direct attention. Most natural philosophers hold, and have held, that action at a distance across empty space is impossible; in other words, that matter cannot act where it is not, but only where it is. The question "where is it?" is a further question that may demand attention and require more than a superficial answer. For it can be argued on the hydrodynamic or vortex theory of matter, as well as on the electrical theory, that every atom of matter has a universal though nearly infinitesimal prevalence, and extends everywhere; since there is no definite sharp boundary or limiting periphery to the region disturbed by its existence. The lines of force of an isolated electric charge extend throughout illimitable space. And though a charge of opposite sign will curve and concentrate them, yet it is possible to deal with both charges, by the method of superposition, as if they each existed separately without the other. In that case, therefore, however far they reach, such nuclei clearly exert no "action at a distance" in the technical sense.

Some philosophers have reason to suppose that mind can act directly on mind without intervening mechanism, and sometimes that has been spoken of as genuine action at a distance; but, in the first place, no proper conception or physical model can be made of such a process, nor is it clear that space and distance have any particular meaning in the region of psychology. The links between mind and mind may be something quite other than physical proximity, and in denying action at a distance across empty space I am not denying telepathy or other activities of a non-physical kind: for although brain disturbance is certainly physical and is an essential

concomitant of mental action, whether of the sending or receiving variety, yet we know from the case of heat that a material movement can be excited in one place at the expense of corresponding movement in another, without any similar kind of transmission or material connection between the two places: the thing that travels across vacuum is not heat.

In all cases where physical motion is involved, however, I would have a medium sought for; it may not be matter, but it must be something; there must be a connecting link of some kind, or the transference cannot occur. There can be no attraction across really empty space. And even when a material link exists, so that the connexion is obvious, the explanation is not complete: for when the mechanism of attraction is understood, it will be found that a body really only moves because it is pushed by something from behind. The essential force in nature is the *vis a tergo*. So when we have found the "traces," or discovered the connecting thread, we still run up against the word "cohesion," and ought to be exercised in our minds as to its ultimate meaning. Why the whole of a rod should follow, when one end is pulled, is a matter requiring explanation; and the only explanation that can be given involves, in some form or other, a continuous medium connecting the discrete and separated particles or atoms of matter.

When a steel spring is bent or distorted, what is it that is really strained? Not the atoms—the atoms are only displaced; it is the connecting links that are strained—the connecting medium—the ether. Distortion of a spring is really distortion of the ether. All stress exists in the ether. Matter can only be moved. Contact does not exist between the atoms of matter as we know them; it is doubtful if a piece of matter ever touches another piece, any more than a comet touches the sun when it appears to rebound from it; but the atoms are connected, as the comet and the sun are connected, by a continuous *plenum* without break or discontinuity of any kind. Matter acts on matter only through the ether. But whether matter is a thing utterly distinct and separate from the ether, or whether it is a specifically modified portion of it—modified in such a way as to be susceptible of locomotion, and yet continuous with all the rest of the ether, which can be said to extend everywhere—far beyond the bounds of the modified and tangible portion—are questions demanding, and I may say in process of receiving, answers.

Every such answer involves some view of the universal and possibly infinite uniform omnipresent connecting medium, the Ether of space.

It has been said, somewhat sarcastically, that the ether was made in England. The statement is only an exaggeration of the truth. I might even urge that it has been largely constructed in the Royal Institution; for, I will remind you now of the chief lines of evidence on which its existence is believed in, and our knowledge of it is

based. First of all, Newton recognised the need of a medium for explaining gravitation. In his "Optical Queries" he shows that if the pressure of this medium is less in the neighbourhood of dense bodies than at great distances from them, dense bodies will be driven towards each other; and that if the diminution of pressure is inversely as the distance from the dense body, the law will be that of gravitation.

All that is required, therefore, to explain gravity is a diminution of pressure, or increase of tension, caused by the formation of a matter unit—that is to say of an electron or corpuscle: and although we do not yet know what an electron is—whether it be a strain centre, or what kind of singularity in the ether it may be—there is no difficulty in supposing that a slight, almost infinitesimal, strain or attempted rarefaction should be produced in the ether whenever an electron came into being—to be relaxed again only on its resolution and destruction. Strictly speaking it is not a real *strain*, but only a "stress"; since there can be no actual *yield*, but only a pull or tension, extending in all directions towards infinity.

The tension required per unit of matter is almost ludicrously small, and yet in the aggregate, near such a body as a planet, it becomes enormous.

The force with which the moon is held in its orbit would be great enough to tear asunder a steel rod four hundred miles thick, with a tenacity of 30 tons per square inch; so that if the moon and earth were connected by steel instead of by gravity, a forest of pillars would be necessary to whirl the system once a month round their common centre of gravity. Such a force necessarily implies enormous tension or pressure in the medium. Maxwell calculates that the gravitational stress near the earth, which we must suppose to exist in the invisible medium, is 3000 times greater than what the strongest steel could stand; and near the sun it should be 2500 times as great as that.

The question has arisen in my mind, whether, if the whole sensible universe—estimated by Lord Kelvin as equivalent to about a thousand million suns—were all concentrated in one body of specifiable density,* the stress would not be so great as to produce a tendency towards ethereal disruption; which would result in a disintegrating explosion, and a scattering of the particles once more as an enormous nebula and other fragments into the depths of space. For the tension would be a maximum in the interior of such a mass; and, if it rose to the value 10^{33} dynes per square centimetre, something would have to happen. I do not suppose that this can be the reason, but one would think there must be *some* reason, for the scattered condition of gravitative matter.

* On doing the arithmetic, however, I find the necessary concentration absurdly great, showing that such a mass is quite insufficient. See Appendix,

Too little is known, however, about the mechanism of gravitation to enable us to adduce it as the strongest argument in support of the existence of an ether. The oldest valid and conclusive requisition of an ethereal medium depends on the wave theory of light, one of the founders of which was your Professor of Natural Philosophy at the beginning of last century, Dr. Thomas Young.

No ordinary matter is capable of transmitting the undulations or tremors that we call light. The speed at which they go, the kind of undulation, and the facility with which they go through vacuum, forbid this.

So clearly and universally has it been perceived that waves must be waves of something—something distinct from ordinary matter—that Lord Salisbury, in his presidential address to the British Association at Oxford, criticised the ether as little more than a nominative case to the verb to undulate. It is truly *that*, though it is also truly more than that; but to illustrate that luminiferous aspect of it, I will quote a paragraph from that lecture of Clerk Maxwell's to which I have already referred:—

“The vast interplanetary and interstellar regions will no longer be regarded as waste places in the universe, which the Creator has not seen fit to fill with the symbols of the manifold order of His kingdom. We shall find them to be already full of this wonderful medium; so full, that no human power can remove it from the smallest portion of space, or produce the slightest flaw in its infinite continuity. It extends unbroken from star to star; and when a molecule of hydrogen vibrates in the dog-star, the medium receives the impulses of these vibrations, and after carrying them in its immense bosom for several years, delivers them, in due course, regular order, and full tale, into the spectroscope of Mr. Huggins, at Tulse Hill.” (It is pleasant to remember that those veteran investigators Sir William and Lady Huggins are still at work.)

This will suffice to emphasise the fact that the eye is truly an ethereal sense-organ—the only one which we possess, the only mode by which the ether is enabled to appeal to us, and that the detection of tremors in this medium—the perception of the direction in which they go, and some inference as to the quality of the object which has emitted them—cover all that we mean by “sight” and “seeing.”

I pass then to another function, the electric and magnetic phenomena displayed by the ether; and on this I will only permit myself a very short quotation from the writings of Faraday, whose whole life may be said to have been directed towards a better understanding of these ethereal phenomena. Indeed, the statue in your entrance hall may be considered as the statue of the discoverer of the electric and magnetic properties of the Ether of space.

Faraday conjectured that the same medium which is concerned in the propagation of light might also be the agent in electromagnetic phenomena. “For my own part,” he says, “considering the relation

of a vacuum to the magnetic force, and the general character of magnetic phenomena external to the magnet, I am much more inclined to the notion that in the transmission of the force there is such an action, external to the magnet, than that the effects are merely attraction and repulsion at a distance. Such an action may be a function of the æther; for it is not unlikely that, if there be an æther, it should have other uses than simply the conveyance of radiation."

This conjecture has been amply strengthened by subsequent investigations.

One more function is now being discovered; the ether is being found to constitute matter—an immensely interesting topic, on which there are many active workers at the present time. I will make a brief quotation from your present Professor of Natural Philosophy (J. J. Thomson), where he summarises the conclusion which we all see looming before us, though it has not yet been completely attained, and would not by all be similarly expressed:—

"The *whole* mass of any body is just the mass of ether surrounding the body which is carried along by the Faraday tubes associated with the atoms of the body. In fact, all mass is mass of the ether; all momentum, momentum of the ether; and all kinetic energy, kinetic energy of the ether. This view, it should be said, requires the density of the ether to be immensely greater than that of any known substance."

Yes, far denser—so dense that matter by comparison is like gossamer, or a filmy imperceptible mist, or a milky way. Not unreal or unimportant—a cobweb is not unreal, nor to certain creatures is it unimportant, but it cannot be said to be massive or dense; and matter, even platinum, is not dense when compared with the ether. Not till last year, however, did I realise what the density of the ether must really be,* compared with that modification of it which appeals to our senses as matter, and which for that reason engrosses our attention. If I have time I will return to that before I have finished.

Is there any other function possessed by the ether, which, though not yet discovered, may lie within the bounds of possibility for future discovery? I believe there is, but it is too speculative to refer to, beyond saying that it has been urged as probable by the authors of "The Unseen Universe," and has been thus tentatively referred to by Clerk Maxwell:—

"Whether this vast homogeneous expanse of isotropic matter is fitted not only to be a medium of physical interaction between distant bodies, and to fulfil other physical functions of which, perhaps, we have as yet no conception, but also . . . to constitute the material organism of beings exercising functions of life and mind as high or

* See Lodge, *Phil. Mag.*, April 1907.

higher than ours are at present—is a question far transcending the limits of physical speculation.”

And there for the present I leave that aspect of the subject.

I shall now attempt to illustrate some relations between ether and matter.

The question is often asked, is ether material? This is largely a question of words and convenience. Undoubtedly, the ether belongs to the material or physical universe, but it is not ordinary matter. I should prefer to say it is not “matter” at all. It may be the substance or substratum or material of which matter is composed, but it would be confusing and inconvenient not to be able to discriminate between matter on the one hand, and ether on the other. If you tie a knot on a bit of string, the knot is composed of string, but the string is not composed of knots. If you have a smoke or vortex-ring in the air, the vortex-ring is made of air, but the atmosphere is not a vortex-ring; and it would be only confusing to say that it was.

The essential distinction between matter and ether is that matter *moves*, in the sense that it has the property of locomotion and can effect impact and bombardment: while ether is *strained*, and has the property of exerting stress and recoil. All potential energy exists in the ether. It may vibrate, and it may rotate, but as regards locomotion it is stationary—the most stationary body we know—absolutely stationary, so to speak; our standard of rest.

All that we ourselves can effect, in the material universe, is to alter the motion and configuration of masses of matter; we can move matter, by our muscles, and that is all we can do directly: everything else is indirect.

But now comes the question, how is it possible for matter to be composed of ether? How is it possible for a solid to be made out of fluid? A solid possesses the properties of rigidity, impenetrability, elasticity, and such like; how can these be imitated by a perfect fluid such as the ether must be? The answer is, they can be imitated by a fluid in motion; a statement which we make with confidence as the result of a great part of Lord Kelvin’s work.

It may be illustrated by a few experiments.

A wheel of spokes, transparent or permeable when stationary, becomes opaque when revolving, so that a ball thrown against it does not go through, but rebounds. The motion only affects permeability to matter; transparency to light is unaffected.

A silk cord hanging from a pulley becomes rigid and viscous when put into rapid motion; and pulses or waves which may be generated on the cord travel along it with a speed equal to its own velocity, whatever that velocity may be, so that they appear to stand still. This is a case of kinetic rigidity; and the fact that the wave-transmission velocity is equal to the rotatory speed of the material, is typical and

important, for in all cases of kinetic elasticity these two velocities are of the same order of magnitude.

A flexible chain, set spinning, can stand up on end while the motion continues.

A jet of water at sufficient speed can be struck with a hammer, and resists being cut with a sword.

A spinning disk of paper becomes elastic like flexible metal, and can act like a circular saw. Sir William White tells me that in naval construction steel plates are cut by a rapidly revolving disk of soft iron.

A vortex-ring, ejected from an elliptical orifice, oscillates about the stable circular form, as an indiarubber ring would do: thus furnishing a beautiful example of kinetic elasticity, and showing us clearly a fluid displaying some of the properties of a solid.

A still further example is Lord Kelvin's model of a spring balance, made of nothing but rigid bodies in spinning motion.*

If the ether can be set spinning, therefore, we may have some hope of making it imitate the properties of matter, or even of constructing matter by its aid. But how are we to spin the ether? Matter alone seems to have no grip of it. I have spun steel disks, a yard in diameter, 4000 times a minute, have sent light round and round between them, and tested carefully for the slightest effect on the ether. Not the slightest effect was perceptible. We cannot spin ether mechanically.

But we can vibrate it electrically; and every source of radiation does that. An electrified body, in sufficiently rapid vibration, is the only source of ether-waves that we know; and if an electric charge is suddenly stopped, it generates the pulses known as X-rays, as the result of the collision. Not speed, but sudden change of speed, is the necessary condition for generating waves in the ether by electricity.

We can also infer some kind of rotary motion in the ether; though we have no such obvious means of detecting the spin as is furnished by vision for detecting some kinds of vibration. It is supposed to exist whenever we put a charge into the neighbourhood of a magnetic pole. Round the line joining the two, the ether is spinning like a top. I do not say it is spinning fast: that is a question of its density; it is in fact spinning with excessive slowness, but it is spinning with a definite moment of momentum. J. J. Thomson's theory makes its moment of momentum exactly equal to em , the product of charge and pole: the charge being measured electrostatically and the pole magnetically.

How can this be shown experimentally? Suppose we had a spinning top enclosed in a case, so that the spin was unrecognisable by ordinary means—it could be detected by its gyrostatic behaviour to

* Address to Section A of British Association at Montreal, 1884.

force. If allowed to "precess" it will respond by moving perpendicularly to a deflecting force. So it is with the charge and the magnetic pole. Try to move the charge suddenly, and it immediately sets off at right angles. A moving charge is a current, and the pole and the current try to revolve round one another—a true gyrostatic action due to the otherwise unrecognisable etherial spin. The fact of such magnetic rotation was discovered by Faraday.

I know that it is usually worked out in another way, in terms of lines of force and the rest of the circuit; but I am thinking of a current as a stream of projected charges; and no one way of regarding such a matter is likely to exhaust the truth, or to exclude other modes which are equally valid. Anyhow, in whatever way it is regarded, it is an example of the three rectangular vectors.

The three vectors at right angles to each other, which may be labelled Current, Magnetism, and Motion respectively, or more generally E, H, and V, represent the quite fundamental relation between ether and matter, and constitute the link between Electricity, Magnetism, and Mechanics. Where any two of these are present, the third is a necessary consequence. This principle is the basis of all dynamos, of electric motors, of light, of telegraphy, and of most other things. Indeed, it is a question whether it does not underlie everything that we know in the whole of the physical sciences; and whether it is not the basis of our conception of the three dimensions of space.

Lastly, we have the fundamental property of matter called *inertia*, which, if I had time, I would show could be explained electromagnetically, provided the etherial density is granted as of the order 10^{12} grammes per cubic centimetre. The elasticity of the ether would then have to be of the order 10^{33} c.g.s.; and if this is due to intrinsic turbulence, the speed of the whirling or rotational elasticity must be of the same order as the velocity of light. This follows hydrodynamically; in the same sort of way as the speed at which a pulse travels on a flexible running endless cord, whose tension is entirely due to the centrifugal force of the motion, is precisely equal to the velocity of the cord itself. And so, on our present view, the intrinsic energy of constitution of the ether is incredibly and portentously great; every cubic millimetre of space possessing what, if it were matter, would be a mass of a thousand tons, and an energy equivalent to the out-put of a million-horse-power-station for 40 million years.

The universe we are living in is an extraordinary one; and our investigation of it has only just begun. We know that matter has a psychical significance, since it can constitute *brain*, which links together the physical and the psychical worlds. If anyone thinks that the ether, with all its massiveness and energy, has probably no psychical significance, I find myself unable to agree with him.

APPENDIX I. ON GRAVITY AND ETHERIAL TENSION.

Stating the law of gravitation as $F = \gamma \frac{m m'}{r^2}$, the meaning here adopted for etherial tension at the surface of the Earth is

$$T = \int_R^\infty \frac{\gamma E}{r^2} dr = \frac{\gamma E}{R};$$

so that the ordinary intensity of gravity is

$$g = -\frac{dT}{dR} = \frac{\gamma E}{R^2} = \frac{4}{3}\pi\rho\gamma R.$$

Accordingly, near the surface of a planet the tension is $T_0 = g R$, or for different planets is proportional to ρR^2 .

The velocity of free fall from infinity to such a planet is $\sqrt{(2 T_0)}$; the velocity of free fall from circumference to centre, assuming uniform distribution of density, is $\sqrt{(T_0)}$; and from infinity to centre it is $\sqrt{(3 T_0)}$.

Expanding all this into words:—

The etherial tension near the earth's surface, required to explain gravity by its rate of variation, is of the order 6×10^{11} c.g.s. units. The tension near the sun is 2500 times as great. With different spheres in general, it is proportional to the density and to the superficial area. Hence, near a bullet one inch in diameter, it is of the order 10^{-6} ; and near an atom or electron about 10^{-21} .

In order to set up a tension equal to the critical, or presumably disruptive, stress in the ether [10^{33} c.g.s.], a globe of the density of the earth would have to have a radius of eight light years. In order to generate a velocity of free fall under gravity equal to the velocity of light, a globe of the earth's density would have to be equal in radius to the distance of the earth from the sun, or say 26,000 times the earth's radius. If the density were less, the superficial area would have to be increased in proportion, so as to keep ρR^2 constant.

The whole visible universe within a parallax of $\frac{1}{1000}$ second of arc, estimated by Lord Kelvin as the equivalent of 10^9 suns, would be quite incompetent to raise etherial tension to the critical point 10^{33} c.g.s. unless it were concentrated to an absurd degree; but it could generate the velocity of light with a density comparable to that of water.

If the average density of the above visible universe (which may be taken as 1.6×10^{-23} grammes per c.c.) continued without limit, a disruptive tension of the ether would be reached when the radius was comparable to 10^{13} light years: and the velocity of light would

be generated by it when the radius was 10^7 light years. But heterogeneity would enable these values to be reached *more* easily.

It is noteworthy how exceedingly small is the average or aggregate density of matter in the visible region of space; and Lord Kelvin has shown that throughout space in general it must be smaller still—in fact ultimately infinitesimal.

The estimated density of 10^{-23} c.g.s. means that the visible cosmos is as much rarer than a vacuum of a hundred millionths of an atmosphere, as that vacuum is itself rarer than lead.

It is, of course, because ordinary masses of matter likewise consist of separated particles, with great intervening distances in proportion to their size, that we are able to assert the minute aggregate density of ordinary stuff, such as water or lead, as compared with the continuous medium of which all particles are supposed to be really composed. The fundamental medium itself must be of uniform density everywhere, whether materialised or free.

APPENDIX II. EXPLANATORY REMARKS CONCERNING THE DENSITY OF ETHER.

I observe that it is surmised by at least one thoughtful and friendly critic—C. W. S. in the *Westminster Gazette*—that in speaking of the immense density or massiveness of ether, and the absurdly small density or specific gravity of gross matter by comparison, I intended to signify that matter is a *rarefaction* of the ether. That, however, was not my intention. The view I advocate is that the ether is a perfect *continuum*, an absolute *plenum*, and that therefore no rarefaction is possible. The ether inside matter is just as dense as the ether outside, and no denser. A material unit—say an electron—is only a peculiarity or singularity of some kind in the ether itself, which is of perfectly uniform density everywhere. What we sense as matter is an aggregate or grouping of an enormous number of such units.

How then can we say that matter is millions of times rarer or less substantial than the ether of which it is essentially composed? Those who feel any difficulty here, should bethink themselves of what they mean by the average or aggregate density of any discontinuous system, such as a powder, or a gas, or a precipitate, or a snowstorm, or a cloud, or a milky way.

If it be urged that it is unfair to compare an obviously discrete assemblage like the stars, with an apparently continuous substance like air or lead, the answer is that it is entirely and accurately fair: since air, and every other known form of matter, is essentially an aggregate of particles, and since it is always their average density that we mean. We do not even know for certain their individual atomic density.

The phrase “specific gravity or density of a powder” is ambigu-

ous. It may mean the specific gravity of the dry powder as it lies, like snow; or it may mean the specific gravity of the particles of which it is composed, like ice.

So also with regard to the density of matter, we might mean the density of the fundamental material of which its units are made—which would be ether; or we might, and in practice do, mean the density of the aggregate lump which we can see and handle; that is to say, of water or iron or lead, as the case may be.

In saying that the density of matter is small, I mean, of course, in the last, the usual, sense. In saying that the density of ether is great, I mean that the actual stuff of which these highly porous aggregates are composed is of immense, of well-nigh incredible, density. It is only another way of saying that the ultimate units of matter are few and far between—i.e. that they are excessively small as compared with the distances between them; just as the planets of the solar system, or worlds in the sky, are few and far between—the intervening distances being enormous as compared with the portions of space actually occupied by lumps of matter.

Here it may be noted that it is possible to argue that the density of a *continuum* is necessarily greater than the density of any disconnected aggregate: certainly of any assemblage whose particles are actually composed of the material of the *continuum*. Because the former is “all there,” everywhere, without break or intermittence of any kind; while the latter has gaps in it—it is here, and there, but not everywhere.

Indeed, this very argument was used long ago by that notable genius Robert Hooke, and I quote a passage which Professor Poynting has discovered in his collected posthumous works, and kindly copied out for me:—

“As for *matter*, that I conceive in its essence to be immutable, and its essence being expatiation determinate, it cannot be altered in its quantity, either by condensation or rarefaction; that is, there cannot be more or less of that power or reality, whatever it be, within the same expatiation or content; but every equal expatiation contains, is filled, or is an equal quantity of *materia*; and the densest or heaviest, or most powerful body in the world contains no more *materia* than that which we conceive to be the rarest, thinnest, lightest, or least powerful body of all; as gold for instance, and *æther*, or the substance that fills the cavity of an exhausted vessel, or cavity of the glass of a barometer above the quicksilver. Nay, as I shall afterwards prove, this cavity is more full, or a more dense body of *æther*, in the common sense or acceptation of the word, than gold is of gold, bulk for bulk; and that because the one, viz., the mass of *æther*, is all *æther*: but the mass of gold, which we conceive, is not all gold; but there is an intermixture, and that vastly more than is commonly supposed, of *æther* with it; so that vacuity, as it is commonly thought, or erroneously supposed, is a more dense body than

the gold as gold. But if we consider the whole content of the one with that of the other, within the same or equal quantity of expatiation, then are they both equally containing the *materia* or body."

[From the *Posthumous Works of Robert Hooke, M.D., F.R.S.*, 1705, pp. 171-2 (as copied in *Memoir of Dalton*, by Angus Smith).]

Newton's contemporaries did not excel in power of clear expression, as he himself did, but Professor Poynting interprets this singular attempt at utterance thus: "All space is filled with equally dense *materia*. Gold fills only a small fraction of the space assigned to it, and yet has a big mass. How much greater must be the total mass filling that space."

The tacit assumption here made is that the particles of the aggregate are all composed of one and the same continuous substance, practically that matter is made of ether: and that assumption, in Hooke's day, must have been only a speculation. But it is the kind of speculation which time is justifying, it is the kind of truth which we all feel to be in process of establishment now.

We do not depend on that sort of argument however: what we depend on is experimental measure of the mass, and mathematical estimate of the volume, of the electron. For calculation shows that however the mass be accounted for, whether electrostatically or magnetically, or hydrodynamically, the estimate of ratio of mass to effective volume can differ only in a numerical coefficient, and cannot differ as regards order of magnitude. The only way out of this conclusion would be the discovery that the negative electron is not the real or the main matter-unit, but is only a subsidiary ingredient, whereas the main mass is the more bulky positive charge. That last hypothesis however is at present too vague to be useful. Moreover, the mass of such a charge would in that case be unexplained, and would need a further step, which would probably land us in much the same sort of ethereal density as is involved in the estimate which I have based on the more familiar and tractable negative electron.

It may be said why assume any finite density for the ether at all? Why not assume that, as it is infinitely continuous, so it is infinitely dense—whatever that may mean—and that all its properties are infinite? This might be possible were it not for the velocity of light. By transmitting waves at a finite and measurable speed, the ether has given itself away, and has let in all the possibilities of calculation and numerical statement. Its properties are thereby exhibited as essentially finite—however infinite the whole extent of it may turn out to be.

[O. L.]



WEEKLY EVENING MEETING

Friday, February 28, 1908.

GEORGE MATTHEY, Esq., F.R.S., in the Chair.

PROFESSOR WILLIAM ARTHUR BONE, D.Sc. Ph.D. F.R.S.

Explosive Combustion, with Special Reference to that of Hydrocarbons.

It is hardly necessary to remind you that the subject of my discourse will be ever associated with the illustrious name of Davy. Davy turned his attention to the phenomena of flame in the year 1815, in response to an urgent appeal on the part of a committee formed in the North of England, to investigate the causes of accidents arising from the explosion of fire-damp in coal mines, and to devise means for their prevention. The perennial interest of his researches, however, lies not so much in their immediate practical success, great as this undoubtedly was, as in the broader theoretical issues which were disclosed, and brought within the region of experimental enquiry, by so splendid an exercise of genius.

Davy insisted on the necessity of considering flames in all cases "As the combustion of an *explosive mixture* of inflammable gas, or vapour, and air," and he defined flame as "aëriiform, or gaseous matter, heated to such a degree as to be luminous." For the starting and propagation of a flame in an explosive mixture, he showed that each successive layer of gas must be raised to a certain definite temperature, called the "ignition point," and he investigated both the ignition temperatures and the explosion limits of a large number of the commoner combustible gases. He then proceeded to his famous discovery that, notwithstanding the extremely high temperatures of flames, which, in the case of cyanogen, he estimated to be "above 5000° of Fahrenheit," they can be readily extinguished by contact with a cooling surface of sufficient area and heat-conducting power, and that for this purpose metal surfaces are by far the most efficient. How he developed and applied this discovery to the construction of his "safe-lamp" for miners is, of course, a matter of history.

In experimenting upon the ignition temperatures of explosive mixtures, Davy made the important and far-reaching discovery that combustible gases combine with oxygen at lower temperatures without any appearance of flame whatever. He emphasised the importance of a complete investigation of the chemical aspects of this

flameless combustion, and he himself was led to ask whether, seeing that the temperatures of flames far exceed those at which solids become incandescent, a metallic wire can be raised to incandescence by the slow combustion of two gases "without actual flame, but producing heat enough to keep the wire ignited." In this way he discovered the remarkable property of platinum and other metallic wires of inducing surface combustion, and in the course of his further experiments on this subject, he made two notable observations respecting the burning of compounds containing carbon and hydrogen. He found "much carbonic oxide" produced when a platinum wire was kept incandescent by the slow combustion of a mixture of ethylene and oxygen, rendered non-explosive by an excess of the hydrocarbon, and in a similar experiment with ether vapour, he recorded the appearance of "a pale phosphorescent light" accompanied by "the formation of a peculiar acrid volatile substance possessed of acid properties."

Finally, in speculating upon the difficult and thorny subject of the luminosity of hydrocarbon flames, he was "led to imagine" that it "might be owing to the *decomposition* of part of the gas towards the interior of the flame where the air was in smallest quantity, and the deposition of solid charcoal, which, first by its *ignition*, and afterwards by its *combustion*, increased in a high degree the intensity of the light." It is important to observe that not only did Davy rightly attribute the luminosity of a hydrocarbon flame to the presence therein of incandescent carbon, but also that he avoided the error of attributing the separation of carbon to a supposed preferential burning of hydrogen.

In considering the propagation of a flame through an explosive mixture of gases, it is necessary to distinguish between two well-defined conditions. When such a mixture is ignited, the flame travels for a certain limited distance (a few feet only) at a fairly uniform slow velocity, which in the case of a mixture of hydrogen and oxygen in their combining ratios is approximately 34 metres (38 yards) per second. This initial stage of the combustion is called "*inflammation*."

After traversing a few feet, however, the flame begins to vibrate and alters in character. The vibrations become more and more intense, the flame swinging backwards and forwards with oscillations of increasing amplitude. Then one or other of two things happens; either the flame is extinguished, or it goes forward with an exceedingly great and constant velocity, producing the most violent effects. The new condition thus set up is termed "*Detonation*," and the forward movement of the flame is called the "*Explosion Wave*."

The discovery of "*detonation*" in gaseous mixtures was made simultaneously by M. Berthelot and MM. Malard and Le Chatelier, in the year 1881; Berthelot proved that the velocity of the explosion wave is independent of the length of the column of gas traversed, and

that for the same gaseous mixture under given physical conditions it always has a constant value. In this connection I would mention Professor H. B. Dixon's exhaustive researches on the "*rates of explosion*" of gaseous mixtures, which have extended in so many ways our knowledge of explosive combustion.

Experiment I.—Perhaps the best illustration of the outward difference between ordinary "*inflammation*" and "*detonation*" is afforded by the case of a mixture of carbonic oxide and oxygen in their combining ratios. When ignited in an open tube 4 or 5 inches long, the mixture burns quietly with the familiar blue flame. Far otherwise is it, however, when a long column of the mixture is fired in a leaden coil, where the brief initial period of *inflammation* is succeeded by the *explosion wave*, which dashes onwards through the gases at a rate of 1700 metres (about a mile) per second with shattering effect.

Another notable feature of "*detonation*" is the extremely short duration of the flame. In the course of some experiments carried out under Professor Dixon's direction, it was found that the duration of luminosity in each successive layer of gas in the detonation of electrolytic gas does not exceed $\frac{1}{30000}$ th part of a second. But short as this time is, it is something like a million times longer than the interval between successive molecular collisions in a gaseous mixture.

The question of how a hydrocarbon burns, that is to say, precisely how it is attacked by the oxygen, has been the subject of much discussion during the past fifteen years. A hydrocarbon is a compound of the two combustible elements, carbon and hydrogen, and in a sufficient supply of oxygen, both of these are ultimately burnt to carbon dioxide and steam, respectively. Thus, for example, in the case of ethane :—



On kinetic grounds, however, it seems inconceivable that the passage from the initial system of ethane and oxygen to the final system of carbon dioxide and steam can be immediate and direct. It is, therefore, universally recognised that the process involves a number of successive stages. But opinion has been sharply divided as to the nature and sequence of these stages, and I will now endeavour to put before you the main points in dispute. They may be conveniently summarised under three heads.

1. During the greater part of last century the belief prevailed that the hydrogen is much the more combustible of the two elements of a hydrocarbon, and that consequently when combustion occurs in a limited supply of oxygen, the hydrogen is preferentially burnt, as follows :—



Who was the author of this view, or what was originally its experimental basis, is not quite clear, but it received the active support of two such eminent authorities as Thomas Graham and Michael Faraday, and for fifty years it was regarded as one of the most certain articles of chemical faith. Its final overthrow by Dixon and Smithells in the year 1892, caused no small stir in chemical circles.

2. The second theory originated with Kersten in 1861, who as the outcome of experiments on the explosion of a mixture of ethylene and electrolytic gas, asserted that "before any portion of the hydrogen is burnt, all the carbon is burnt to carbonic oxide, and that the excess of oxygen then divides itself between the carbonic oxide and the hydrogen." In other words, Kersten attempted to substitute the idea of the preferential burning of carbon for that of the preferential burning of hydrogen. His views, however, received no serious attention until they were revived and endorsed by Dixon and Smithells in 1892.

The chief experimental basis for this theory is the behaviour of ethylene and acetylene when exploded with their own volume of oxygen. More than a century ago, Dalton found that a mixture of equal volumes of ethylene and oxygen yields mainly carbonic oxide and hydrogen on explosion, without any separation of carbon, in conformity with the equation :—



This fact, after being overlooked for nearly eighty years, was rediscovered by Dixon in 1891; moreover, a few years later, when it was proved that acetylene behaves in a precisely similar manner—



the advocates of the theory were able to claim a strong body of evidence in support of their case.

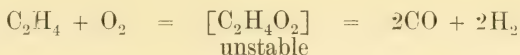
3. But the idea of a "*preferential*" combustion, whether of carbon or of hydrogen, seemed repugnant to well-established principles concerning the nature and conditions of chemical interactions in gaseous systems. Moreover, whilst the assumption of a *direct* passage from an initial system of ethylene and oxygen, $\text{C}_2\text{H}_4 + \text{O}_2$, to the system carbonic oxide and hydrogen, $2\text{CO} + 2\text{H}_2$, implied a simple transaction from the kinetic standpoint, an extension of the idea to the case of such a hydrocarbon as propylene—



would at once raise serious difficulties.

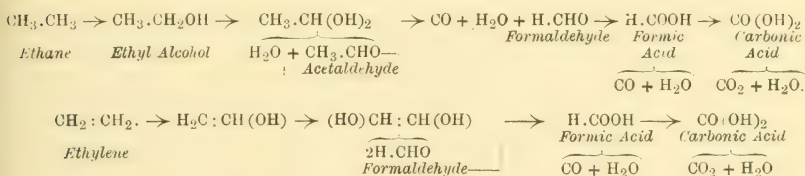
It therefore remained to consider whether the solution of the

problem might not lie in the assumption of an initial *association* of the hydrocarbon and oxygen forming an unstable "oxygenated" molecule, which subsequently rapidly decomposes. Thus, for example, the changes involved in the explosive combustion of an equimolecular mixture of ethylene and oxygen might conceivably be represented somewhat as follows:—



Many years ago, indeed, Professor H. E. Armstrong, suggested that the combustion of a hydrocarbon takes place under the conjoint influence of water and oxygen, and involves the successive formation of intermediate "*hydroxylated*" molecules, which at high temperatures rapidly decompose into simpler products. Little notice was taken of his suggestion at the time, but recent researches have shown that "*hydroxylated*" molecules are probably formed, even in flames, although I think it doubtful whether water vapour is an essential factor in the process.

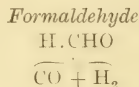
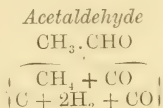
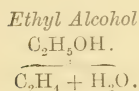
The researches recently carried out at the Manchester University, have covered the entire range of conditions under which hydrocarbons can be burned, from the slow flameless combustion discovered by Davy, right up to the extreme conditions of detonation. An exhaustive study of the slow combustion of methane, ethane, ethylene, and acetylene, at temperatures between 250° and 400° C., afforded decisive evidence against the preferential burning, whether of carbon or of hydrogen. Large quantities of aldehydic intermediate products were isolated, and the balance of evidence was decidedly in favour of the "*hydroxylation*" theory, with the proviso, however, that the oxygen is directly active. Finally, the following scheme was put forward for the slow combustion of ethane, and ethylene at 250° to 350° C.



Translated into words, this means that, in the case of ethane, the initial oxidation product is probably *ethyl alcohol* $\text{C}_2\text{H}_5.\text{OH}$. The alcohol has not, indeed, been actually isolated during the slow combustion at 300° C., chiefly because it is much more rapidly oxidised

under these conditions than is ethane itself ; it is, however, formed in quantity when ethane is oxidised by means of ozone at 100°C . The second stage involves the rapid oxidation of the alcohol to the unstable $\text{CH}_3 \cdot \text{CH}(\text{OH})_2$, which at once decomposes into steam and *acetaldehyde*. The *acetaldehyde* is in turn burnt to carbonic oxide, steam, and *formaldehyde* (possibly through the unstable $\begin{smallmatrix} \text{HO} \cdot \overset{\text{H}}{\underset{\text{HO} \cdot \text{C} : \text{O}}{\text{C}}} \cdot \text{H} \end{smallmatrix}$), and finally the *formaldehyde* is burnt to steam and oxides of carbon, probably through *formic acid* and *carbonic acid*.

As the temperature rises, the intermediate products become more and more unstable, and to an increasing extent decompose into simpler products, which then undergo independent oxidation. Thus *ethyl alcohol* decomposes into ethylene and steam, *acetaldehyde* either into methane and carbon monoxide, or into carbon, hydrogen, methane, and carbon monoxide, according to the temperature, and *formaldehyde* is resolved into carbon monoxide⁶ and hydrogen, as follows :—



With the extension of the research to the conditions existing in hydrocarbon flames and explosions, it became increasingly evident that the mechanism of combustion is essentially the same above as below the ignition point. I do not mean, of course, that the phenomena observed at low temperatures, in slow combustion, are exactly reproduced in flames, but rather that the result of the initial molecular encounter between the hydrocarbon and oxygen is probably much the same in the two cases, namely, the formation of an "oxygenated" molecule. At the higher temperatures of flames, secondary thermal decompositions undoubtedly come into operation at an earlier stage, and play a more important rôle than in slow combustion, but they do not precede the onslaught of the oxygen upon the hydrocarbon, but arise in consequence of it. I am aware that there are eminent critics who, whilst admitting the validity of these views as applied to slow combustion, hesitate to accept entirely their extension to explosive combustion. They find it difficult to believe that such compounds as ethyl alcohol, acetaldehyde, formaldehyde, and the like, which are undoubtedly very unstable at high temperatures, can possibly be formed in flames. But surely this objection involves some confusion of thought as to the factors which govern the formation and stability of chemical compounds ; the fact that a substance cannot permanently exist at a given temperature

does not justify the assertion that it cannot be formed at that temperature by the operation of factors which are not concerned in its decomposition. Therefore, I am not prepared to admit that because the acetaldehyde molecule does not long remain intact at high temperatures, it necessarily follows that it cannot be brought into actual existence as the result of the interaction of ethane and oxygen in flames.

Professor Smithells, in his recent presidential address to the chemical section at the British Association (1907), expressed his dissent from my views, as applied to flames, on the ground that "The isolation of an intermediate product under one set of circumstances is in itself no proof that this product is transitorily formed when the reaction is proceeding under another set of circumstances. . . ." To this I would reply, that whilst the isolation of (say) acetaldehyde in the slow oxidation of ethane is not *by itself* sufficient proof of its transitory formation in the explosive combustion of the hydrocarbon, yet if it can be demonstrated, not only that the facts of explosive combustion can be *best* interpreted on the assumption of its formation, but that, so far as can be judged at present, no other interpretation can be advanced, and, moreover, that aldehydes are actually produced in flames, then it may be justly claimed that the assumption is well founded, and that the onus of its experimental disproof rests with the sceptics.

Having thus, I hope, explained the main issues involved in the controversy, I shall now proceed to perform a series of experiments on the explosive combustion of acetylene, ethylene, and ethane, some of which are crucial as regards the rival theories under discussion.

Experiment II.—I have here three cylindrical bulbs of stout borosilicate glass (capacity = about 60 c.c.), fitted with firing wires, hermetically sealed, and containing respectively equimolecular mixtures of each of the three hydrocarbons with oxygen, that is to say, mixtures corresponding to $C_2H_2 + O_2$, $C_2H_4 + O_2$, and $C_2H_6 + O_2$, respectively.

Now, according to the theory of the preferential combustion of carbon, these mixtures should on explosion, yield nothing but carbonic oxide and hydrogen, without any separation of carbon, or formation of steam, as follows:—

					p_2/p_1^*
(a)	$C_2H_2 + O_2 = 2CO + H_2$.	.	.	1.5
(b)	$C_2H_4 + O_2 = 2CO + 2H_2$.	.	.	2.0
(c)	$C_2H_6 + O_2 = 2CO + 3H_2$.	.	.	2.5

* The symbols p_1 and p_2 , used in this and subsequent tables, denote the initial and final pressures of the *cold* original mixture and gaseous products (dry) at constant volume and at the same temperature.

On firing the mixtures, it is at once evident that something very like this does happen in the cases of (a) and (b). There is absolutely no deposition of carbon, and no appreciable condensation of steam in the cold products. Far otherwise is it, however, in the case of the bulb containing the mixture $C_2H_6 + O_2$. A lurid flame fills the vessel, accompanied by a black cloud of carbon particles, and a close inspection of the cold bulb will reveal a considerable condensation of water. The pressure ratio p_2/p_1 is approximately 1.5, and an analysis of the gaseous products would prove the presence of about 10 per cent. of methane. The bulb will now be opened, rinsed out with water, and the formation of aldehydic products demonstrated by means of Schiff's reagent. It is clear that these results are wholly inconsistent with the theory of the preferential burning of carbon.

As it is obviously impracticable for me to complete the experiment by analysing the gaseous products before you, I will draw your attention to the following tabulated results of three similar experiments carried out some time ago.

TABLE I. — RESULTS OF EXPERIMENTS ON INFLAMMATION IN SEALED GLASS BULBS.

Original Mixture.		$C_2H_2 + O_2$			$C_2H_4 + O_2$			$C_2H_6 + O_2$		
p_1		352 mm.			545 mm.			746 mm.		
p_2		508 "			1053 "			1148 "		
p_2/p_1		1.44			1.93			1.54		
% Composition of Gaseous Products.	CO_2	0.75			0.30			4.20		
	CO	67.10			50.00			33.55		
	$C_2H_2 + C_2H_4$	nil			nil			2.75		
	CH_4	1.55			1.70			10.85		
	H_2	30.60			48.00			48.60		
		C	H	O	C	H	O	C	H	O
Units in original mixture		352	176	176	545	545	272	746	1119	376
Units in gaseous products		352	171	175	547	541	267	621	854	241
Difference		Practically nil.			Practically nil.			125	265	135

Did time permit, I could easily demonstrate to you by other similar experiments, that the *outward* difference here revealed between the burning of ethylene and that of ethane extends to all the other gaseous olefines and paraffins: that is to say, whereas mixtures of olefines and oxygen corresponding to $C_nH_{2n} + \frac{n}{2} O_2$ on explosion

yield mainly carbonic oxide and hydrogen, without separation of carbon, mixtures of paraffin and oxygen corresponding to $C_nH_{2n+2} + \frac{n}{2} O_2$ yield carbon, oxides of carbon, methane, hydrogen, and steam, all in considerable quantities. Are we then to conclude that there is some peculiarity about the constitution or combustion of an olefine which induces a preferential burning of its carbon, whilst the corresponding paraffin is burnt in an entirely different way? The following experiment will show that such a view cannot for a moment be entertained.

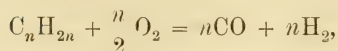
Experiment III.—I will now fire a bulb containing a mixture of 60 per cent. of ethylene and 40 per cent. of oxygen (i.e. $3C_2H_4 + 2O_2$). As might be expected, the flame is accompanied by a large deposition of carbon, but what is of greater importance still is the fact that a considerable amount of water is also formed. The full significance of this experiment may be gathered from the following data.

Original mixture $\left\{ \begin{array}{l} C_2H_4 = 59.65 \text{ per cent.} \\ O_2 = 40.35 \text{ ,,} \end{array} \right. \quad \begin{array}{l} p_1 = 562 \text{ mm.} \\ p_2 = 816 \text{ ,,} \end{array} \quad p_2/p_1 = 1.45$

Gaseous products $CO_2 = 2.5, CO = 37.2, C_2H_2 + C_2H_4 = 6.4, CH_4 = 6.5,$
 $H_2 = 47.4 \text{ per cent.}$

	C	H	O
Units in original mixture	670	670	227
Units in gaseous products	482	572	172
Difference	188	98	55

I think it will be now admitted that such an experiment as this completely destroys the foundations of the theory of the preferential burning of carbon. As I have already stated, the original experimental basis of the theory, was the behaviour of an equimolecular mixture of ethylene and oxygen, yet here is proof that on closer examination the behaviour of ethylene is inconsistent with the theory. I would therefore say to those who may be inclined to cavil at my views as to the mechanism of combustion, that whatever may be the final issue of the controversy, this fact, amongst others, must be explained, namely, that whereas a mixture of an olefine and oxygen corresponding to $C_nH_{2n} + \frac{n}{2} O_2$, yields mainly carbonic oxide and hydrogen on explosion, in harmony with the equation—



a diminution of the oxygen below this limit at once gives rise to steam formation.

Experiment IV.—The next experiment is designed to illustrate the infinitely greater affinity of acetylene and ethylene as compared with that of hydrogen for oxygen at the high temperatures of flames. I have here two bulbs containing mixtures of each of these hydrocarbons with hydrogen and oxygen corresponding to $\text{C}_2\text{H}_2 + 2\text{H}_2 + \text{O}_2$ and $\text{C}_2\text{H}_4 + \text{H}_2 + \text{O}_2$, respectively, and I will ask you to contrast the behaviour of these with that of the equimolecular mixture of ethane and oxygen, $\text{C}_2\text{H}_6 + \text{O}_2$, which was exploded a few minutes ago. It should be noted that whilst all three mixtures contain the same relative proportions of carbon, hydrogen and oxygen, they differ in respect of the proportions between the combined carbon and hydrogen. Asking you to bear in mind how the equimolecular mixture of ethane and oxygen on explosion gave rise to a black cloud of carbon and a considerable formation of water, I will now fire the other two mixtures. You will observe that in neither case has there been any deposition of carbon, and an inspection of the cold bulbs will show that little or no steam formation has occurred. In fact, the hydrocarbon has been burnt to carbonic oxide and hydrogen, leaving the hydrogen absolutely untouched by the oxygen, as the following detailed results show (Table II.).

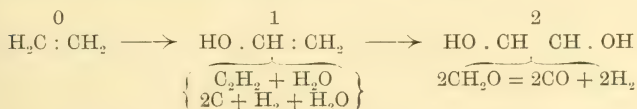
These experiments have an important bearing on the chemistry of flames. Hydrogen is usually considered as one of the most combustible of gases, but here we see it pushed to one side by the all-powerful hydrocarbon, as though it were so much inert nitrogen. This at once raises another question which has lately been occupying my attention. Ever since Davy's experiments on flame, the combustibility of hydrogen has been considered to be superior to that of methane; this, however, cannot be true in regard to slow combustion, since it can be easily proved that between 300° and 400°C . methane is oxidised at a far faster rate than hydrogen. Nevertheless, I have recently observed facts which incline me to think that possibly it may be true in regard to flames. If further investigation confirms this opinion, it will be necessary to enquire into the cause of the peculiar relative inertness and stability of methane as compared with other gaseous hydrocarbons when subjected to the action of oxygen at high temperatures.

It does not I think impose too great a strain on the imagination to picture the probable mechanism of combustion in hydrocarbon flames, and for this purpose ethylene and ethane may be taken as typical examples. It may be assumed that, with the possible exception of methane, the affinity of a hydrocarbon for oxygen is so great at high temperatures that the initial stage of its combustion takes

TABLE II.—EXPERIMENTS ON INFLAMMATION IN SEALED GLASS BULBS.

% Composition of Gaseous Products.	Original Mixture.	$C_2H_4 + H_2 + O_2$			$C_2H_2 + 2H_2 + O_2$		
	p_1	503 mm.			534 mm.		
	p_2	750 "			653 "		
	p_2/p_1	1.49			1.22		
	CO_2	0.35			0.2		
	CO	39.60			39.8		
	$C_2H_2 + C_2H_4$	1.25			nil		
	CH_4	3.65			0.2		
	H_2	55.15			59.8		
		C	H	O	C	H	O
Original mixture	:	341	505	168	267	400	133
Gaseous products	:	346	478	151	262	394	131
Difference	:	—	27	17	Negligible.		

precedence of all other chemical phenomena in flames. This is probably true of the propagation of flame through explosive mixtures of hydrocarbons and oxygen. In the special case of a stream of a hydrocarbon burning in air, partial decomposition may occur in the innermost regions of the flame, where the supply of oxygen is very limited, before combustion begins. But, in general, whenever the hydrocarbon and oxygen are brought together at high temperatures, their mutual affinities will prove superior to any disruptive forces which would otherwise break down the hydrocarbon. It is probably not so much the original hydrocarbon as its hydroxylated molecule which decomposes in flames; the sudden increase in the internal energy of the hydrocarbon molecule, consequent upon its initial association with oxygen, would render the resulting hydroxylated molecule extremely unstable, and, in default of its immediate further oxidation, it would speedily decompose. The explosive combustion of ethylene may, therefore, be represented by the following scheme—

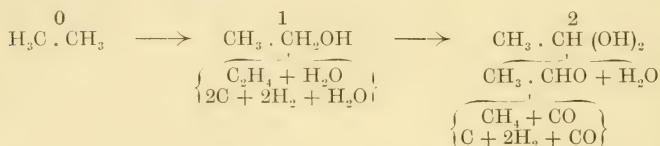


In a sufficient supply of oxygen, the transition from the original hydrocarbon to the *dihydroxy* state is probably so rapid that no breaking down of the ethylenic structure occurs in passing through the initial *monohydroxy* stage. Indeed, it is conceivable that under

the extreme conditions of detonation the passage from 0 to 2 may be effected in a single molecular impact. The *dihydroxy* derivative would at once break down into carbon monoxide and hydrogen, *via* formaldehyde.

But when the oxygen supply is reduced below the equimolecular proportion, it is evident that the initial *monohydroxy* derivative cannot all be oxidised to the *dihydroxy* stage; some of it would, therefore decompose, partly into acetylene and steam, and partly also into carbon, hydrogen, and steam, together with some methane.

In a similar manner, the combustion of ethane would involve the rapid passage through *ethyl alcohol* to *acetaldehyde* and *steam*, with subsequent decomposition of the aldehyde into carbon, hydrogen, methane, and carbonic oxide, with the proviso that a reduction of the oxygen supply below the equimolecular proportion, would bring about in some measure the decomposition of the alcohol into ethylene and steam, etc., at stage 1.



But the cases of ethane and ethylene are typical of all other hydrocarbons, so that it may be said that, in general, the mechanism of explosive combustion involves, (1) the initial formation and subsequent decomposition of hydroxylated (or "oxygenated") molecules; (2) in a sufficient supply of oxygen, the independent oxidation of the decomposition products; (3) in an insufficient oxygen supply, the subsequent breaking down of unsaturated hydrocarbons, interactions between carbon and steam, or between oxides of carbon, hydrogen, and steam, the final system depending on the amount of available oxygen, the temperature of the flame, and the rate of cooling.

Experiment V.—The influence of different rates of cooling of the flame on the final system may be illustrated by firing an equimolecular mixture of ethane and oxygen in two glass vessels, having approximately the same volume, but widely different surface areas. For this purpose I have selected (1) a tube about 1 metre long and 2 cm. internal diameter, and (2) a globe of 8.5 cm. internal diameter. Both these vessels have the same volume (about 300 c.c.), but the surface area of the tube is very nearly 3 times that of the globe. It is therefore to be expected that, in consequence of the more rapid cooling of the flame, there will be a greater accumulation of the primary combustion products in the case of the tube experiment. On comparing the results of the two explosions, it is at once evident that more water and less carbon have been produced in the case of the tube; moreover, the pressure ratio p_2/p_1 , is 1.45 as com-

pared with about 1·75 in the globe experiment, and an examination of the products would show that the lower ratio is accounted for by the much greater survival of acetylene, ethylene, and aldehydic products in the tube experiment. These facts, which are set forth in the following table, are in complete harmony with the hydroxylation theory.

TABLE III.—INFLAMMATION OF AN EQUIMOLECULAR MIXTURE OF ETHANE AND OXYGEN.

—		A. In Long Tube.			B. In Large Globe.		
		701 mm.			685 mm.		
		1018 „			1187 „		
		1·45			1·73 „		
% Composition of Gaseous Products.	CO ₂	4·20			3·40		
	CO	34·80			36·10		
	C ₂ H ₂	5·00					
	C ₂ H ₄	2·65			0·15		
	CH ₄	8·85			7·25		
	H ₂	44·50			53·05		
		C	H	O	C	H	O
Original mixture . . .		694	1041	354	678	1017	346
Gaseous products . . .		643	738	220	558	805	255
Difference . . .		51	303	134	120	212	91
% Difference . . .		7·6	29	37·8	18	20	27·5

Experiment VI.—The experiments I have so far shown you, refer more particularly to the initial period of “*inflammation*” in explosive combustion, that is to say, to the conditions ordinarily prevailing in hydrocarbon flames. The question may be asked whether or not the views I have advanced are applicable to the extreme conditions of “*detonation*” or of explosions under high initial pressures. The question may be put in the following form: Is there any experimental evidence of discontinuity between “*inflammation*” and “*detonation*” as regards the nature and sequence of the chemical changes involved? or, Is the hydrocarbon attacked by oxygen in an entirely different way in explosive combustion at high pressures, or in the explosion wave, to what it is in ordinary flames? This question can, I think, best be answered by a consideration of the behaviour of an equimolecular mixture of ethane and oxygen under these extreme conditions.

It is difficult to set up detonation in this mixture; the gases

must be fired at an initial pressure of about $1\frac{1}{2}$ atmosphere in a stout leaden coil of about 1 inch internal diameter. Even then, it is necessary to start the explosion wave in a special firing piece containing electrolytic gas under pressure. I therefore regret that, owing to the special arrangements requisite for success, it is not possible to make the experiment to-night. I will, however, give you the results obtained on detonating the mixture in my laboratory, but before doing so, I will carry out an experiment on the explosion of the gases at an initial pressure of 15 atmospheres.

The cylindrical steel bomb on the table is part of an apparatus recently installed in the Fuel and Metallurgical Laboratories of the University of Leeds for investigations on gaseous explosions under high pressures. The bomb is about a foot long with an external diameter of 4 inches, and the central cylindrical explosion chamber is 8 inches long by 1 inch in diameter. It has been tested by hydraulic pressure up to 2000 atmospheres, and has been repeatedly used for experiments with mixtures of hydrocarbons and oxygen at initial pressures of as much as 40 atmospheres. The bomb is now connected, through a valve at the top, with a standard Bourdon gauge, and contains an equimolecular mixture of ethane and oxygen at a pressure of 15.8 atmospheres. The valve will now be closed, and the mixture fired by means of an electrical arrangement in the special firing piece.

All that is audible of the explosion is a sharp click, and on opening the valve connecting with the gauge again, the final pressure of the cold products of explosion is recorded. After applying the necessary correction for the "dead space" in the gauge connections, but the final "corrected" pressure is as nearly as possible 30.8 atmospheres, corresponding to a ratio $p_2/p_1 = 1.93$. I would now direct your attention to the tabulated results of a similar bomb experiment carried out a few weeks ago at Leeds at an initial pressure of 25 atmospheres, and also at the same time to those of another experiment in which the gases were detonated in a lead coil at an initial pressure of $1\frac{1}{2}$ atmosphere.

In both these experiments carbon was deposited, and it is evident also, that steam was formed. The ratio p_2/p_1 , was as nearly as possible 2.0 instead of the 2.5 required by the theory of the preferential combustion of carbon. Moreover, a notable feature of the results is the presence of as much as 7 per cent. of methane among the products of the experiment at 25 atmospheres; the fact that so much methane survived when all other hydrocarbons were battered to pieces during the explosion (no traces of either acetylene or ethylene being found in the products) is a remarkable testimony to its relatively great stability at the highest temperatures of explosion flames. There is no evidence in these experiments, of any real discontinuity between the chemical phenomena of ordinary "*inflammation*" and those of "*detonation*." The higher temperatures, and more violent conditions in

TABLE IV.—RESULTS OF EXPLOSION OF AN EQUIMOLECULAR MIXTURE ETHANE AND OXYGEN UNDER HIGH PRESSURES.

		A. Detonation in Lead Coil.			B. Explosion in Steel Bomb.		
		1180 mm.			25·2 atms.		
		2240 "			51·7 "		
		1·90 "			2·05 "		
% Composition of Gaseous Products.	p_1						
	p_2						
	p_2/p_1						
	CO ₂	1·80			2·6		
	CO	39·10			37·2		
	C ₂ H ₂	0·90			nil		
	C ₂ H ₄	0·50					
	CH ₄	7·70			7·0		
	H ₂	50·00			52·2		
		C	H	O	C	H	O
Original mixture . . .		1186	1779	587 mm.	25·35	38·0	12·55 atms.
Gaseous products . . .		1151	1507	488 "	24·50	34·6	11·05 "
Difference . . .		35	272	99 "	0·85	3·4	1·5
% Difference . . .		3	15	17	3·4	9	12·0

"*detonation*" are responsible for the more complete breaking down of unsaturated hydrocarbons, and a greater "unburning" of steam by carbon, but there is probably no difference as regards the mode in which the hydrocarbon is attacked by the oxygen in the two cases.

I therefore believe, that, so far as our present knowledge goes, the views I have put forward, afford a simple and consistent interpretation of hydrocarbon combustion, whether it be the slow flameless kind discovered by Davy, or the more complex phenomena of ordinary flames so wonderfully expounded by him, or finally the extreme conditions characteristic of the explosion wave.*

* I desire to acknowledge the devoted help rendered to me in the conduct of these investigations by the following research students of the Manchester University: Messrs. R. V. Wheeler, W. E. Stockings, G. W. Andrew, Julien Drugman, and H. Henstock.

[W. A. B.]

GENERAL MONTHLY MEETING,

Monday, March 2, 1908.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and
Vice-President, in the Chair.

Colonel David Bruce, C.B. D.Sc. F.R.S.
Miss Dorothy Deane Butcher,
Miss Louisa Dawson Cleghorn,
Mrs. Margaret Hannay Clerk,
Vivian Gordon, Esq.
James Hunter Gray, Esq.
Miss Catherine Hassard,
William Preece, Esq.
Caleb Williams Saleeby, Esq., M.D. F.R.S.E.
Hans Sauer, Esq., M.D.
Mrs. Virginia Schilizzi,
Miss Helena Stefanovich Schilizzi,
Harold Albert Wilson, Esq., M.A. D.Sc. F.R.S.
Charles Edward Wurtzburg, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Sir Andrew Noble, Bart., K.C.B., for his Donation of £100 to the Fund for the Promotion of Experimental Research at Low Temperatures, and to Mr. Shelford Bidwell for his Donation of £5 5s. to the General Fund.

Also to Mr. W. Hugh Spottiswoode for his present of a Manuscript Record containing the early Laboratory Experiments of the late John Peter Gassiot, F.R.S., who was formerly a Manager and Benefactor of the Institution.

Also to Mrs. Kemp for her gift of a portrait of the late Sir Benjamin Baker, K.C.B. F.R.S., a Vice-President of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Secretary of State for India—Agricultural Journal of India, Vol. II. Part 4. 8vo. 1907.

Linguistic Survey of India, Vol. IX. Part 3. 4to. 1907.

Trigonometrical Survey of India, Vol. XVIII. 4to. 1906.

Geological Survey: Records, Vol. XXXII. Part 2. 8vo. 1907.

- British Museum Trustees*—Guide to the Great Game Animals. 8vo. 1907.
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WEEKLY EVENING MEETING,

Friday, March 6, 1908.

DONALD W. C. HOOD, ESQ., C.V.O. M.D. F.R.C.P., Vice-President,
in the Chair.

PROFESSOR A. E. H. LOVE, M.A. D.Sc. F.R.S.

The Figure and Constitution of the Earth.

THE subject of this lecture is the figure and constitution of the Earth. I have chosen this title in order to draw attention to the theory which asserts that the shape of the earth is an outward and visible sign of its inward structure. We know that the shape of the earth is a very good sphere. It would be difficult to make so exact a sphere. If we could make a model 25 feet in diameter the inequality of the surface would have to amount to no more than one inch. That is to say, the longest diameter of the model would have to exceed the shortest by one inch. The inequalities of the surface, trifling as they are in comparison with the dimensions of the earth, are very important to mankind, and we try to understand how they came to be what they are.

The greatest interest attaches to those inequalities which are concerned in the distribution of continent and ocean : but, before passing to the consideration of these, I must advert to that inequality which, although it is the greatest of all, has hardly any influence upon this distribution. A rotating body of planetary dimensions cannot be a perfect sphere ; but, owing to the rotation, the equatorial parts must be driven outwards from the axis, and the formation of the equatorial protuberance must be compensated by the flattening of the parts near the axis. Newton determined the shape as an *oblate spheroid*, the figure formed by the revolution of an ellipse about its shortest diameter. The relative situation of an oblate spheroid of small ellipticity and a sphere of equal volume may be illustrated by an ellipse and a concentric circle, adjusted so that the diameter of the circle exceeds the shortest diameter of the ellipse by twice as much as the longest diameter of the ellipse exceeds the diameter of the circle. [The figure was shown on a lantern slide.] In the case of the earth, the elevation all round the equator is about $4\frac{1}{2}$ miles, the depression at either pole about $8\frac{3}{4}$ miles. The result that there is equatorial protuberance as well as polar flattening, and that the one is half as great as the other, is as much a part of the theory, and is as well

verified by observation, as the result that there is polar flattening. The inequality is named *ellipticity of the meridians*.

The existence of this inequality has often been supposed to prove that the interior of the earth is fluid, or that, if not fluid now, it was so once. The actual amount of the inequality, as specified by the excess of the equatorial diameters above the polar diameter, is about 26 miles. Newton computed it on the hypothesis that the earth is composed of homogeneous incompressible fluid, and found that with that constitution the amount of the inequality would be about 33 miles. Many years ago, Lord Kelvin pointed out that the earth would have elliptic meridians if the matter of which it is composed were as rigid as steel, and he showed that, if the substance were homogeneous and incompressible, and had this degree of rigidity, the amount of the inequality would be about 11 miles. [The numbers were thrown on the screen.] As the substance is neither homogeneous nor incompressible, these results do not decide the question of internal fluidity. So far it has not been possible to take account theoretically of the compressibility or of the heterogeneity, but it is probable that both would increase the computed ellipticity. If this could be proved, the actual amount of the inequality would show that the interior of the earth has a high degree of rigidity.

Rigidity of a substance may be defined qualitatively as capacity to stand in a shape, or in a state, which requires the existence, within the substance, of different pressures in different directions. There is a corresponding quantitative definition in terms of the amount of force required to produce a given change of shape.

Substance.	Rigidity.
Water	0
Sandstone	0·15
Glass	0·29
Granite	0·38
Copper	0·55
Steel	1·0
Earth as a whole	1·66

This table [thrown on the screen] shows the rigidities of some substances, steel being regarded as a standard. The last entry in the table is the average rigidity of the materials of the earth, as determined by the rate of transmission of earthquake shocks to great distances. The conclusion that the earth is a very rigid body was reached by Lord Kelvin, by investigations on the tides, and by Sir George Darwin, by investigations concerning the stress produced in the interior by the weight of continents and mountains, and it has been confirmed in a very striking way by recent investigations in

seismology. The very great rigidity of the matter within the earth is doubtless due to the very great pressure to which this matter is subjected.

The detection of the ellipticity of the meridians requires rather refined means of observation, because it is very nearly the same for the general mass of the earth as it is for the waters of the ocean; but there are other inequalities of the surface, manifested by the elevation of the continents and the depression of the ocean basins, which are much more obvious. For information in regard to them we have recourse to maps, to observations of the heights of places above sea level, and to soundings. [A map of the world on Mercator's projection was shown and reasons for using other projections indicated.] In order to reduce to a mathematical theory the inequalities in question, we must begin by getting an arithmetical acquaintance with the facts. Our first question must be as to the sizes of the inequalities. The height of the highest mountain is between five and six miles, the greatest depth yet sounded anywhere in the ocean is less than six miles. Thus the amounts of these inequalities are less than those answering to ellipticity of the meridians. But the corresponding gradients are steeper. On very many coasts the gradient from the shore to very deep water (2000 fathoms) is about 1 in 150. The ellipticity of the meridians gives a maximum gradient, estimated by rate of descent towards the centre, of 1 in 300. Our next question must be as to the sizes of the depressed and elevated portions of the surface. A map on an equal area projection shows that much the greater portion of the surface is covered by water. [Maps of two separate hemispheres, each true in area, were shown by slides.] The great expanse of the ocean hides from us many of those features of the somewhat irregular surface of the earth which are partially manifested in the continental elevations and oceanic depressions. This surface projects beyond the spheroid appropriate to the rotation in some places, in others it runs inside it. Where it projects we say there is *elevation*; where it runs inside we say there is *depression*. Depression does not imply concavity. The surface is almost everywhere convex, though it is flatter in some parts than in others. The oceans rest on the depressions and extend upwards over parts of the elevations, and it is the shape of the parts that are covered by water, much more than the shape of the mountainous continental surface, that really determines the shape of the earth.

Our next question must be as to the amounts of the area of the surface of the earth which are at various heights above or depths below the level of the sea. The most important information is summarised in this table (p. 95). [Thrown on screen.]

Since only about 2 per cent. of the total surface is more than 6000 feet above the level of the sea we need not pay much attention to mountains, but we must pay great attention to the depth of the sea in different parts. It is remarkable that although the depth exceeds

2000 fathoms over considerably more than half of the oceanic area, yet the areas over which the depth exceeds 3000 fathoms amount in the aggregate to only about 3 per cent. of the total surface, and these areas, 40 or more in number, are scattered irregularly over the globe.

Total area	100
More than 6000 feet above sea level	2
Above sea level	28
Below sea level	72
More than 6,000 feet below sea level	57
„ 12,000 „ „	43
„ 18,000 „ „	3

In seeking to appreciate the general features of the shape of the earth we may disregard these “deeps,” just as we disregard mountains. Except in certain deeps, the surface of the earth beneath the sea is everywhere convex. The most important data for determining the shape of the earth are the coast line and the contour lines at 1000 and 2000 fathoms depth.

If on a map of the world we draw the contour line at 1000 fathoms depth, we find that the continents are not only widened on all sides, but that, with the exception of the Antarctic continent, they form a single continuous region of elevation. At this depth the Antarctic regions become a continuous region of elevation nearly as far north as the 60th parallel all round. The Arctic Ocean is reduced to two enclosed patches of deep water, one to the north of Great Britain, the other to the north of Russia and Siberia. South America does not taper to the south, but spreads out to the south-east. The ridge joining North and South America is widened out across the West Indies, and the Caribbean Sea and Gulf of Mexico are reduced to a few enclosed patches of deep water. North America is joined on the northern side to Europe and to Asia. The Mediterranean and adjacent seas are represented by a few patches of deep water. Asia is joined to Australia by way of Borneo and New Guinea, and Australia extends southwards over Tasmania and south-eastwards over New Zealand. If we proceed to draw the contour line at 2000 fathoms depth we find that the continents are further widened, and such seas as the Arctic Ocean, the Caribbean and the Mediterranean, almost entirely disappear; but the most important fact is that at this depth the Antarctic continent is not detached, but is united both to South America and to Australia. [Maps of the world in hemispheres with the contour lines at 1000 fathoms and 2000 fathoms depth were shown by slides.] Such maps teach us that the continents are to be regarded as one continuous region of elevation. If we neglect small isolated areas of depression and elevation, we can draw between the 1000-fathoms line and the 2000-fathoms line a curve which divides

the surface of the globe into two regions of equal area: the continental region and the oceanic region. [A slide was shown of a map of the world, true in area, spread out on a rectangle, with the curve drawn.] The continental region is continuous and contains all the continents. The oceanic region consists of two separate portions: the basin of the Pacific Ocean, and the basin of the Atlantic and Indian Oceans. This curve may be called the *boundary of the continental region*. It is a cardinal feature in any geometrical description of the earth's surface.

The gravitational theory of the figure of the earth asserts that the cause of the inequalities expressed by continental elevation and oceanic depression is deep-seated. It is sometimes abbreviated into the formula "heavier matter under the oceans." The average density of surface rocks is 2.8 times the density of water. The average density of the earth as a whole is 5.5 times the density of water. [The numbers were thrown on the screen.] The formula does not mean that the surface rock of density 2.8 has been stripped off the parts where the oceans are, but it means that the denser matter lies rather nearer the surface under the oceans, rather deeper down under the continents. The fact that the figure of the earth is a very good sphere shows that the inequalities in the arrangement of the denser and rarer matter are small, or that the distribution of mass is very nearly symmetrical about a centre. If it were exactly symmetrical, specimens of the material brought from different parts of any spherical surface described around the centre would all have the same density. The spherical surface would be described as a *surface of equal density*. The notion of surfaces of equal density is important in the description of unsymmetrical arrangements as well as symmetrical ones. If on any section of the earth, cut right through the middle, the points at which a particular density exists could be joined up, a curve would be formed. The different curves answering to different densities would lie one inside another, like isobars on a weather chart. The density at any point inside a particular curve would be greater than the density at any point on the curve, the density at any point outside it would be less. To join up all the points where the same density is found in the cubic space within the earth would require a surface, just as to join up points in a plane section requires a curve. The surface is a surface of equal density. The series of surfaces of equal density within the earth resemble the coats of an onion, but with the differences that arise from the distinction between a geometrical surface and a thin sheet of matter. The thing we know about these surfaces is that they are nearly spherical and nearly concentric. The surface of water resting on the earth must be everywhere at right angles to the direction of gravity. If the surfaces of equal density were concentric spheres, the surface of the ocean would be a sphere concentric with them, and the whole earth would be covered by water. If the surfaces of equal density

are only nearly spherical, or nearly concentric, the surface of the ocean must have inequalities of the nature of a heaping up of the waters over areas of oceanic dimensions. The inequalities of the surfaces of equal density determine those of the surface of the ocean, and have a decided influence upon those of the surface of the earth.

In the simplest imaginable case the surfaces of equal density would be accurately spherical but not accurately concentric, crowded together on one side, spaced out on the opposite side. This arrangement may be illustrated by a diagram of a system of circles one inside another, with their centres in a straight line. [Shown by a slide.] The surface of water resting on a body with such a distribution of mass would be a sphere with its centre at, or very near to, the centre of gravity of the body. [Shown by a slide.] It would cut the surface of the body so as to yield a land hemisphere and a water hemisphere. A map of one hemisphere of the earth, with its centre about the middle of France contains all the continents except the southern part of South America, the Antarctic continent, Australia; the other hemisphere is nearly covered by water. [Stereographic maps of these two hemispheres were shown by slides.] It is certain that the distribution of mass within the earth has an inequality of the type considered—the type characterised by eccentric position of the centre of gravity. This is the simplest kind of inequality which a nearly spherical and symmetrical body can have. There is a mathematical theory by which we can connect the inequalities of the surface with the distribution of density. The standard patterns of inequalities are called *spherical harmonics*. The kind of inequality which we have been considering is specified by a spherical harmonic of the *first degree*. It is as if the earth were drawn up out of the sea towards one side, the effect being produced by gravity acting unsymmetrically, and drawing the sea to one side of the earth.

An inequality of density specified by a spherical harmonic of the first degree, means that the surfaces of equal density are crowded to one side without change of shape. Inequalities of density specified by spherical harmonics of higher degrees, mean that the surfaces of equal density are distorted according to one or more of the standard patterns. If this were the case in the earth, the surface of the earth, and the surface of the sea, would be distorted according to the same pattern, but the amounts of distortion would be different for the two surfaces. This was exemplified in the case of the body with non-concentric spherical surfaces of equal density, where the distortion is replaced by a shifting to one side, and the surface of the body was shifted to one side, the surface of the ocean to the other side. The defects of the arrangement of land and water on the earth, as a land hemisphere and a water hemisphere, enable us to detect other inequalities of the surface specified by spherical harmonics of higher degrees. The defects are best shown by means of a map of the two hemispheres drawn one on the top of the other. Thus, we may draw

a map of one hemisphere with its centre in the middle of France, and, over this map, a map of the other hemisphere *turned upside down*, so that any point of the second map coincides with its antipodes in the first. [Shown by a slide in which the outlines of the two maps were coloured differently.] The combined map shows oceans nearly everywhere at the antipodes of continents. But it shows Borneo and some adjacent islands at the antipodes of parts of South America; it shows the southern extremity of South America antipodal to a part of Asia, and it shows the Antarctic region of elevation at the antipodes of the Arctic one. Thus certain parts of the continental region of elevation are antipodal to certain others. Now this arrangement assures us that the surface is somewhat *ellipsoidal*, or that it has an inequality specified by a spherical harmonic of the *second degree*. You know that an ellipsoid is a surface specified by means of three principal directions, which I may refer to as right and left, back and front, up and down. In one of these, say the right and left direction, the ellipsoid projects beyond a sphere of the same volume; in the second, back and front, it runs inside the sphere; in the third, up and down, it may project beyond or run inside according to circumstances. The right and left direction obviously corresponds with the diameter from Borneo to the north-east corner of Brazil; the up and down with the earth's axis. The main feature of a nearly spherical ellipsoid is the existence of two great areas of depression, antipodal to each other, corresponding with the parts where the ellipsoid runs inside the sphere. I make out that the best fit is obtained by fixing one of these so as to coincide nearly with the basin of the Pacific Ocean. The antipodal depression must then contain Africa, the Mediterranean, some neighbouring countries, and parts of the Atlantic and Indian Oceans. It appears that the ellipsoidal inequality, though it certainly exists, is less important than the inequality of the first degree. [Slides were shown of maps of two hemispheres with the two areas of depression marked.]

Now we go back to our double map and observe that Australia is antipodal to the central part of the Atlantic Ocean. [Double map with centre of one hemisphere in Australia shown by slide.] Near the middle of the water hemisphere we have a continent surrounded by ocean. Near the middle of the land hemisphere we have an ocean almost surrounded by continents. This arrangement cannot be expressed by the harmonic of the first degree, or by that of the second degree, or by any combination of the two. It shows to a mathematical eye that there must be an inequality specified by a spherical harmonic of the third degree. The simplest kind of deformation of a sphere which can be expressed by a spherical harmonic of the third degree is shown in Fig. 1, p. 99, where the dotted circle represents the sphere. [Shown by slide.] The figure has been described as pear-shaped. Beginning at the top we see the elevated *stalk*, the depressed *waist*, the protuberant *ring*, and the flattened *crown*. A

sphere with an inequality expressed by this harmonic would show in one hemisphere a central elevation surrounded by a zone of depression, in the other a central depression surrounded by a ring of elevation. [Shown by slide.] This is something like what we observed in the case of Australia in one hemisphere and the central Atlantic in the other, but it is much too symmetrical. A more general type

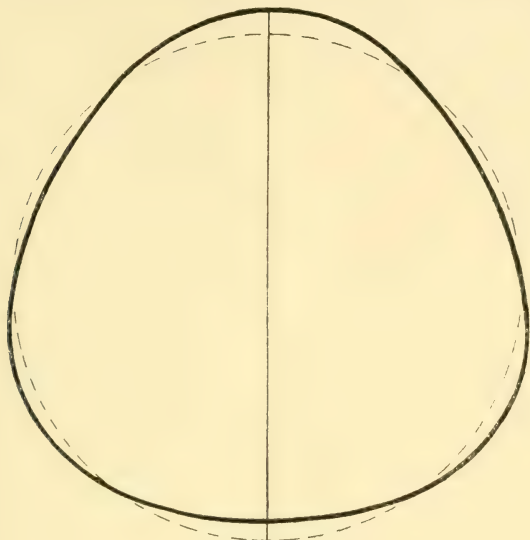


FIG. 1.

of spherical harmonics of the third degree gives us an unsymmetrical pear-shaped figure (Fig. 2); it is more like a natural pear than the other. [Shown by slide.] The stalk is rather to one side, the waist is higher on one side than the other, so is the protuberant ring, and the crown is askew. When it is said, as it is sometimes, that the figure of the earth is pear-shaped, it ought to be meant that the surface has an inequality of this type. This figure is obtained by combining the inequality which we examined just now with another (Fig. 3), which represents another special type of spherical harmonics of the third degree. [Shown by slide.] Here we have, on one side, elevation above and below and a central depression; on the other side, depression above and below, and a central elevation. A sphere with an inequality expressed by this harmonic would show in either hemisphere a circle surrounded by a ring; half of the circle and the alternate half of the ring are elevated, the other halves are depressed. [Shown by slide.] Seen from a different point of view it would show in one hemisphere a central band of elevation bordered by

semicircular caps of depression, in the antipodal hemisphere a central band of depression bordered by semicircular caps of elevation. [Shown by slide.] A map of one hemisphere with its centre in latitude 15° N. and longitude 30° W., shows the Arctic and Atlantic oceans as a somewhat irregular central band of depression, with regions of elevation on either side of it. [Map shown by slide.] Australia would

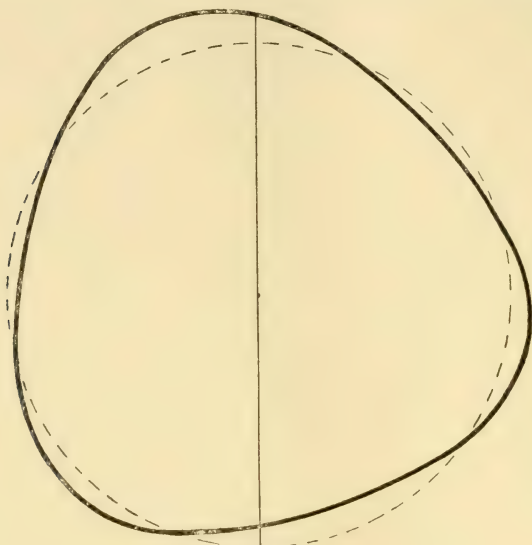


FIG. 2.

be near the middle of the antipodal band of elevation. Our unsymmetrical pear-shaped figure was obtained by one way of combining the two types of spherical harmonics of the third degree. Many other figures can be so obtained. The most important are these: A division of the sphere into octants alternately depressed and elevated, and a division of the sphere into six equal sectors alternately depressed and elevated. [The mode of generation of these figures was shown by slides.] Traces of both can be found on the earth. A map of the world spread out on a rectangle, and divided into eight equal parts by the equator and the meridians of longitude 5° E., 95° E., 175° W., 85° W., shows four northern octants roughly coinciding with North America, the Northern Atlantic, Asia, the Northern Pacific, and the four corresponding southern octants roughly coinciding with the Southern Pacific, South America, the Indian Ocean, Australasia. [Map shown by slide.] A map of the southern hemisphere shows three separate continental masses, South America, Africa, Australasia, running out towards the equator, arranged with

some approach to symmetry, and on the whole widening as they go. [Map shown by slide.] From this discussion we conclude that the surface of the earth presents all the types of inequalities which can be specified by spherical harmonics of the third degree or represented by pear-shaped figures. Since each of the types gave us elevation in Australia and antipodal depression in the central Atlantic, we must

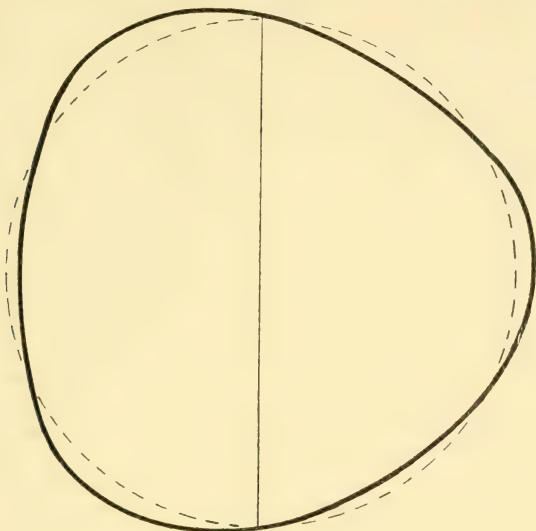


FIG. 3.

not entertain a prejudice in favour of Australia as a sort of stalk for a pear-shaped figure. We shall find that it is better to regard it as a part of the protuberant ring of such a figure.

I will not proceed to describe the spherical harmonics of the fourth and higher degrees, because it is possible to go some way towards accounting theoretically for the existence of inequalities of the first three degrees, but not for those of higher degrees, and, if we found these to exist, we should have to infer that they are not due to gravitational causes, but to local tectonic accidents. We know that the actual shape, whatever it may be, can be reproduced by combining harmonics of all degrees; but, if we find that a fair approximation to the actual shape can be obtained by taking those of the first three degrees only, we may conclude that the main features of the shape are due to gravitational causes. If the depth of the sea, or height of the land, at every point were accurately known, we could use a definite, straightforward and very laborious, mathematical process to determine the proportions in which the various harmonics must be combined in

order to reproduce the actual shape. As the heights and depths are not known everywhere, we use the same process in a rough kind of way, by treating all the land as at one height, the parts of the sea that are less than 1000 fathoms deep as at one depth, those that are between 1000 and 2000 fathoms deep as at a second depth, those that are more than 3000 fathoms deep as at a third depth. By this rough process we can get a suggestion as to the best kind of combination, and then we can modify it so as to get a better fit.

The first harmonic gives us elevation over one hemisphere, depression in the other. In the best fit I have found, the centre of the elevated hemisphere is in the Sudan, not far from Wady Halfa. The hemisphere includes practically the whole of the Arctic, Atlantic and Indian Oceans, Europe, Asia and Africa. Its boundary runs just north of Bering's Strait, cuts across North America to a point near Cape Hatteras, and cuts across South America from a point near the mouth of the Amazon to a point near the mouth of the Rio de la Plata. The depressed hemisphere includes practically the whole of the Pacific Ocean, the greater part of North and of South America, Central America and the West Indies, the Antarctic continent, Australia and New Guinea. [Maps of these hemispheres were shown by slides.] I described earlier the situation of the elevations and depressions yielded by the second harmonic according to the best fit that I have found. The third harmonic gives us a central region of elevation of an oval shape, occupying parts of Europe, Asia and Africa, a sort of stalk of an unsymmetrical pear-shaped figure. This is surrounded on all sides by a zone, in which the third harmonic gives us depression—a sort of waist for the pear. The Arctic, Atlantic, and Indian Oceans are in the waist, and so also are the southern and western extremities of Africa, northern and western Europe, and a great part of Asia. The crown of the pear is in the south-western Pacific, and the protuberant ring contains all but the most north-easterly parts of North America, all South America, except the eastern extremity of Brazil, the Antarctic continent, all Australia except the most westerly part, and New Guinea. The boundary between the waist and the protuberant ring of the pear runs near to the boundary between the elevated and depressed hemispheres of the first harmonic. [Maps illustrating the position of the stalk, waist, etc., of the pear-shaped figure were shown by slides.]

Any one of the harmonics by itself gives depression in some wide tract of actual continent and elevation in some large ocean, and the same is true of any combination of two. When the three are combined it is found that almost every bit of actual continent is included in the region of computed elevation, and that the boundary of this region runs everywhere near to the actual boundary of the continental region. The computed elevation is found to exceed 10 per cent. of its maximum value in three regions, which coincide roughly with (i) Europe, Asia and Africa, (ii) North and South America, (iii) Parts of

Australia and the Antarctic continent. The defects of the computed map admit of an interpretation connecting them with geological events. The conclusion appears to be warranted that the main features of the shape of the earth can be regarded as due to the causes which would give rise to inequalities expressed by spherical harmonics of the first, second, and third degrees. [Statement illustrated by four maps shown as slides.]

I proceed to explain how the three inequalities are accounted for, and I take first the harmonic of the second degree, characterised by ellipsoidal figure and antipodal continents. [A table showing the characters of the harmonics was thrown on the screen]. The ellipsoidal figure means that there is ellipticity of the equator as well as ellipticity of the meridians. The ellipticity of the meridians is due to the rotation; and the fact that both the Arctic and the Antarctic regions are parts of the continental region, shows that the ellipticity is rather greater for the mobile waters of the ocean than it is for the very rigid earth, as we should expect. The ellipticity of the equator is more difficult to account for. It has been suggested that it may be a survival from the state of the earth at the time when the moon had but recently broken away. If the inequality of the first degree, characterised by the eccentric position of the centre of gravity, could be accounted for, it would be easy to account for the inequality of the third degree, answering to the pear-shaped figure, by the interaction of the causes which give rise to harmonics of the first and second degrees. A rotating body with its centre of gravity at a distance from its centre of figure would have its surface deformed in an unsymmetrical way. The denser parts would recede from the axis more than the rarer parts, and the surface would get an inequality specified by a spherical harmonic of the third degree, or it would become pear-shaped. It remains to account for the eccentric position of the centre of gravity. The only dynamical theory which has been put forward to account for this is the theory of gravitational instability. According to this theory the earth was once, when it was less compact than it is now, what may be called too heavy for its strength. If this were the case it would sway to one side, just as a plank, set up on end, and held fast at the bottom, may be too long to stand straight up, and then it bends over to one side. It has been shown to be probable that this condition of things would arise if a certain fraction, here denoted by p/e , approaches a certain critical value. The heaviness of the planet is specified appropriately by the pressure that would be exerted at its centre by the superincumbent material if the density were the same all through. This pressure is denoted by p . In the case of the earth, p is about one and three quarter millions of atmospheres. The appropriate measure of the strength is the elastic resistance called into play when waves of compression travel through the substance. This elastic resistance is denoted by e . [Definitions of p and e were thrown on the screen.] If the substance were granite, in the same condition

as we have it at the earth's surface, e would be about five hundred thousand atmospheres. Owing to the great pressures in the interior, the strength of the materials of the earth is on the average much greater than the strength of granite at the surface. The critical value of the fraction p/e is about 3.5. The value for the earth in its present state is about 0.31, which is far removed from the critical value. The value for the earth if its strength all through were that of granite is about 3.5. If it were bigger and less compact, so that its average density was 3.3 instead of 5.5, and its strength all through were that of granite, the value would be about 1.75; but, if the strength were that of sandstone, it would be about 3.2. It seems quite probable that if the earth was once less compact, and consequently less strong, than it is now, its condition might have been critical, and that the centre of gravity might then have taken up an eccentric position. It is natural to examine other planets from the same point of view. If Mars with his actual density had a strength all through equal to that of granite, the value of p/e for him would be very small (0.03). This result suggests that any elevations and depressions which may exist on the surface of Mars are not due to eccentric position of the centre of gravity. [The numbers were thrown on the screen.]

We conclude that the eccentric position of the earth's centre of gravity, like the ellipticity of the equator, may be a survival from a past state in which this inequality was greater than it is now. The pear-shaped figure has been traced to the interaction of two sorts of causes: the eccentric position of the centre of gravity and the causes which give rise to the ellipsoidal figure, among them the rotation. The inequality specified by harmonics of the third degree must therefore have been at one time less prominent than it is now, in comparison with the inequalities specified by harmonics of the first and second degrees. In attempting to trace the general effects of this change in the relative prominence of the various inequalities, we must observe in the first place that the ellipticity of the meridians, which is one of the characters of the harmonic of the second degree, is subject to fluctuations owing to the diminishing speed of the earth's rotation. As the speed of the rotation diminishes the equatorial protuberance of the surface of the ocean diminishes, and so does the equatorial protuberance of the surface of the earth. The equatorial protuberance is greater for the ocean, and the excess tends to diminish, so that there is a constant tendency for the ocean to inundate the Arctic and Antarctic regions; but this tendency must be checked from time to time by subsidences in equatorial regions, for the equatorial protuberance of the earth's actual figure must be progressively diminished. The changes that have taken place in the shape and size of the ellipsoidal inequality are therefore of great complexity, but fortunately the actual influence of this inequality upon the distribution of continent and ocean is not very great, and we can form a fairly satisfactory notion

of some of the most important changes that can have been produced by gravitational causes, by leaving this inequality out of account. We seek then the changes that can have been produced by the actual diminution of the inequality of the first degree, resulting in an increase of the relative prominence of the inequality of the third degree. The changes, being of the nature of the relief of strain, must take place somewhat spasmodically, but at any one place they would be progressive, not fluctuating, and we should expect that the actual reduction in the inequality of the first degree would be greatest where the inequality is greatest, that is near the centres of the two hemispheres, where it gives us elevation and depression respectively. The regions where the computed elevation is greatest contains the Mediterranean and adjacent seas. The idea of a progressive diminution in the amount of the first harmonic suggests, that the formation of these seas, and of the neighbouring mountain ranges, is not to be ascribed entirely to a series of tectonic accidents, but that these movements were at least in part conditioned by a gravitational change. The increase in the relative prominence of the third harmonic would bring it about that where the first harmonic gives us elevation and the third depression oceans might be formed, and, if formed, they would get wider and deeper with the lapse of time. This state of things is found in the Arctic, Atlantic and Indian oceans. Where ancient continents are destroyed by inundation we should expect that some remnants would survive to mark the ancient sites. In such places the computed elevation would be apt to be too small. This is the case, for example, in South Africa, West Africa, Brazil and the north-eastern part of North America. On the other hand, where the first harmonic gives us depression and the third elevation, there would be a tendency for the continental region to encroach upon the oceanic. This state of things is found around the shores of the Pacific, especially on the American side, and on the greater part of the coast of Australia. Where an ancient ocean is contracted by retreat of the sea, we should expect to find that in some parts the new elevation would fail to be formed and the sea would retain its mastery. In such places the computed elevation would be apt to be too great. This is the case for example, between Australia and the Antarctic continent and to the west of Central and South America. It appears that a change in the actual amount of the inequality answering to the first harmonic, combined with an increase in the relative prominence of the inequality answering to the third harmonic, accounts for most of the defects of the theoretical map obtained by combining harmonics. [The statement was illustrated by two maps shown as slides.] The conclusion that the Atlantic and Indian Oceans are modern in comparison with the Pacific, is one to which geologists have been led by independent evidence, and the differing characters of the coasts around the two great ocean basins has been especially emphasised by Suess. The difference consists in the relations of the coasts to the mountains.

Around the margin of the Pacific the mountain chains generally run parallel to the coasts, as if their formation had been the result of a series of incidents in the advance of the continent to enroach upon the ocean. Around the margin of the Atlantic and Indian Oceans the coast generally cuts across the strike of the mountains, as if the sea had advanced to inundate previously existing continents.

The changes that have taken place in the face of the earth have been ascribed to two kinds of causes: gravitational and tectonic. The gravitational effects are due to changes in the distribution of the earth's mass, producing a shift of position of the oceans. The tectonic changes consist in the formation of mountains, and, possibly, of deeps. Except in so far as they may be incidents accompanying spasmodic but progressive gravitational changes, they are usually associated with the contraction of the earth as it parts with its internal heat, and consequently they are outside the purview of the theory which I have been trying to explain. The gravitational changes have been regarded as a mystery. I hope that this theory may prove to contain the key of the mystery.

[A. E. H. L.]

WEEKLY EVENING MEETING,

Friday, March 13, 1908.

SIR WILLIAM CROOKES, D.Sc. F.R.S., Honorary Secretary and
Vice-President, in the Chair.

COMMENDATORE G. MARCONI, LL.D. D.Sc. *M.R.I.*

Transatlantic Wireless Telegraphy.

BEFORE I go into my subject, it might interest you to know that the invitation from the Royal Institution to deliver this lecture was sent to me by transatlantic wireless telegraphy on October 19, when I was in Canada.

The following is the text of the message :—

“MARCONI, Glace Bay.

“Hearty congratulations on behalf of Royal Institution, the home of Faraday. We invite you to give first Friday evening discourse on January 17 next. Please reply by wireless.

“SIR WILLIAM CROOKES,

“Royal Institution, London.”

To which I replied, also by wireless :—

“By means of ether waves across Atlantic I thank you for honour invitation to lecture at Royal Institution. Owing uncertainty my future plans greatly obliged if you will permit me postpone acceptance until I return to London.

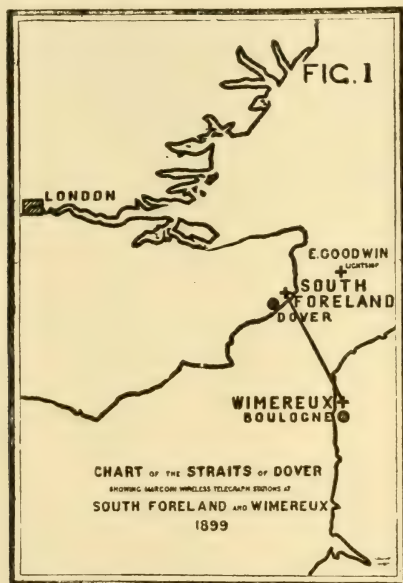
“MARCONI.”

I have had the honour on previous occasions of describing before this Institution some of the stages through which the application of electric waves to telegraphy through space has passed. This evening I propose to confine myself chiefly to describing the results and observations recorded during the numerous tests and experiments which I and my collaborators have been carrying out with the object of proving that wireless telegraphy across the Atlantic was possible, not merely as an experimental feat, but as a new and practical means for commercial communication.*

* Journ. Inst. Elec. Eng., xxviii. 1899, p. 291.

In March, 1899, communication was established by means of my system of wireless telegraphy across the Channel between England and France (see Fig. 1), and the *Times* of March 29 of that year published the first press telegram ever transmitted to England from abroad by means of electric wave telegraphy.

At that time a considerable discussion took place in the Press as to whether or not wireless telegraphy would be practicable for much longer distances than those then covered, and a general opinion prevailed that the curvature of the earth would be an insurmountable obstacle to long-distance transmissions, in the same way as it was, and



is, an obstacle to signalling over considerable distances by means of optical signals such as flashlights, the heliograph, or the semaphore.

Other difficulties were anticipated as to the possibility of being able practically to employ and control a transmitter capable of radiating an amount of electrical energy large enough to actuate a receiver at really great distances, and, granting the possibility of this, whether such a powerful radiator would not interfere with the working of all other wireless stations which might be established on shore or ships within the sphere of influence of the long-distance sender.

What so often occurs in most pioneer work has repeated itself in the case of long-distance wireless telegraphy—the anticipated obstacles and difficulties were either imaginary or else easily surmountable ; but

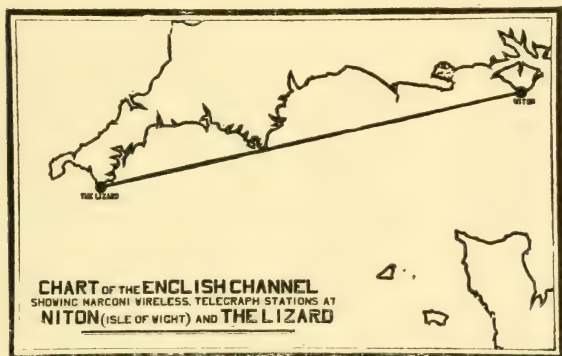
in their place unexpected barriers manifested themselves, and my efforts and those of my collaborators have been mainly directed to the solution of problems presented by difficulties which were not anticipated when the tests over long distances were first initiated.

In January 1901, wireless communication was established between St. Catherine's Point in the Isle of Wight and Lizard in Cornwall, over a distance of 186 miles.

The height of these stations above the sea level did not exceed 300 feet (100 metres), whereas to clear the curvature of the earth a height of more than a mile at each end would have been necessary.

The result of these tests went far to convince me that electric waves produced in the manner I had adopted were able to make their way round the curvature of the earth, and that therefore it was not likely that this factor would constitute a barrier to the transmission

FIG. 2



of waves over greater distances. At this time I had achieved a considerable measure of success, by means of syntonics or tuning devices, in preventing mutual interference between stations, and Professor Fleming described, in a letter to the *Times* dated October 4, 1900, the results obtained, and which he and others had witnessed.*

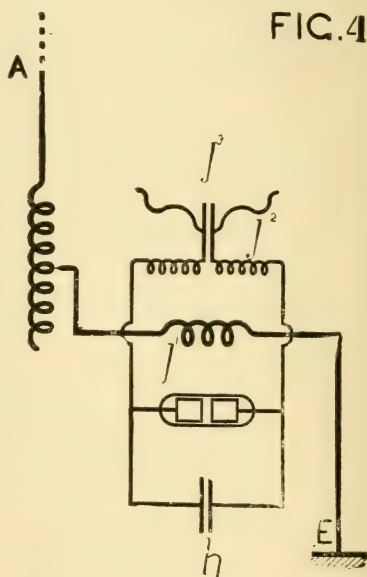
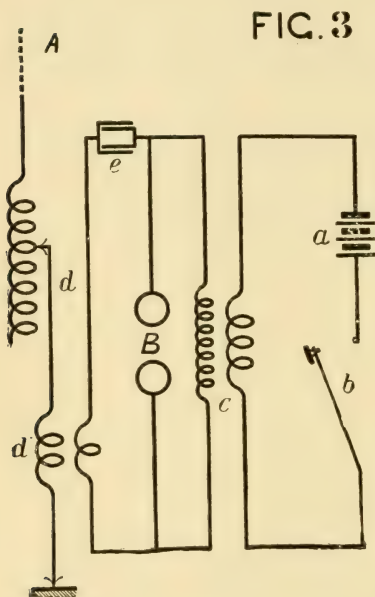
The principle on which the transmitters and receivers at St. Catherine's Point and the Lizard were worked is shown in diagrams 3 and 4.

At the transmitting end a condenser, usually taking the form of a battery of Leyden jars, had one terminal connected to one spark-ball of an induction coil or transformer, and the other to the primary circuit of an oscillation transformer. The opposite terminal of this transformer circuit was joined to the second spark-ball. The condenser was charged to the potential necessary to produce a suitable

* Journ. Soc. Arts, xlix, No. 2530, 1901.

spark by means of an induction coil. The secondary circuit of the oscillation transformer was inserted between the vertical conductor or aerial wire, and earth, and an adjustable inductance coil included in the circuit.

The circuits, consisting of the oscillating circuit and radiating circuit, were more or less closely "coupled" by varying the distance between the primary and secondary of the oscillation transformer. By the adjustment of the inductance inserted between the elevated conductor and earth, and by the variation of the capacity of the primary circuit of the oscillation transformer, the two circuits of the



transmitter could be brought into resonance, a condition which I first found was absolutely necessary in order to obtain efficient radiation.

The receiver consisted also of a vertical conductor or aerial connected to earth through the primary of an oscillation transformer, the secondary of which included a condenser and a coherer, or other suitable detector, it being necessary that the circuit containing the aerial and the circuit containing the detector should be in resonance with each other, and also in tune with the periodicity of the oscillations transmitted from the sending station.

The energy employed to signal over a distance of 186 miles could be brought as low as 150 watts, and even less if a higher or larger aerial had been used.

The facility with which distances of over 100 miles could be covered prior to 1900, and the success of the methods for preventing mutual interferences,* led me to advise that two large power stations be constructed, one in Cornwall and the other in North America, in order to test whether it was possible to transmit messages across the Atlantic Ocean.

I have often been asked why I did not first endeavour to establish commercial communication between places situated at a shorter distance. The answer is very simple. The cables which connect England to the Continent, and between most Continental nations, are Government-owned, and these Governments would not, and will not, allow the establishment of any system, wireless or otherwise, which might in any way tamper with the revenue derived from these cables.

As regards transatlantic communication, however, the conditions were different. There was no law either here, in Canada, or in the United States, to impede the working of wireless telegraphy across the Atlantic.

A further potent reason, moreover, an economical reason, prompted me to attempt communication with America. Notwithstanding the cost of high-power stations, I am convinced that it is more profitable to transmit messages at 6*d.* a word to America than at, say, $\frac{1}{2}$ *d.* a word across the Channel, and that the economical advantage of wireless over cables and land-lines increases instead of diminishing with the distance.

A site suitable for a long-distance station was chosen at Poldhu, in Cornwall, and here in 1900 work was commenced in earnest—work in which I was ably assisted by Professor J. A. Fleming, of the University of London.

The transmitter at Poldhu was similar in principle to the one I have already described, but it is obvious that the considerable distance over which it was proposed to transmit signals necessitated the employment of more powerful electro-magnetic waves than those ever previously used.

These were obtained by means of a generating plant consisting of an alternator capable of an output of about 25 kilowatts, which, through suitable transformers, charged a condenser having a glass dielectric of great strength.

Time does not permit me to describe in detail all the engineering difficulties which were encountered in controlling electrical oscillations of a power which at that time was certainly unprecedented, and as the tests were made possible by commercial organisation, the objects of which do not consist solely in the advancement of science, you will understand that a detailed description of the plant used at the transatlantic stations cannot, for the present at least, be made public.

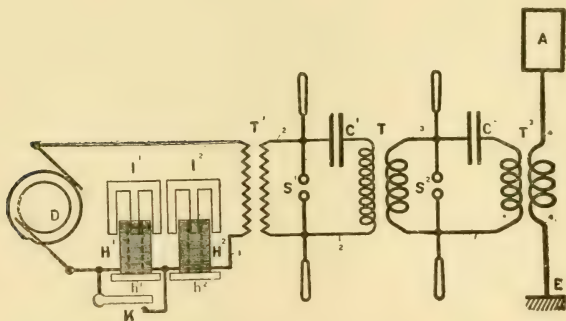
* Journ. Soc. Arts, xlix. No. 2530, 1901.

My early tests on wireless transmission by means of the elevated capacity method had convinced me that when endeavouring to extend the distance of communication it was of little utility merely to increase the power of the electrical energy applied to the transmitting circuits, but that it was also necessary to increase the area or height of the transmitting and receiving elevated conductors.

As it was economically impracticable to use verticable wires of very great height, the only alternative was to increase their size or capacity, which, in view of the facts I had first noticed in 1895, seemed likely to make possible the efficient utilisation of large amounts of electrical energy.*

The form of aerial which I first proposed to employ consisted of a conical arrangement of wires insulated at the top and gathered together at a lower point in the form of a funnel. This aerial was

FIG. 5



supported by a ring of 20 masts each 200 feet high, arranged in a circle of 200 feet in diameter.

During the first tests an arrangement of circuits (Fig. 5) proposed by Dr. Fleming, and consisting of a modification of the system shown in Fig. 3, was employed. In this arrangement, in place of one high-frequency oscillation circuit two are employed, and the constants of the two circuits are so arranged that very high tension discharges can be obtained from one of the condensers—the one which is inductively connected with the aerial—without danger of damage to the circuits of the generator.†

Simultaneously with the construction of the station at Poldhu, the erection of another one on substantially the same plan was undertaken at Cape Cod in the United States of America.

The completion of the arrangements was delayed owing to a storm

* Journ. Inst. Elec. Eng. xxviii. 1899, pp. 278-9.

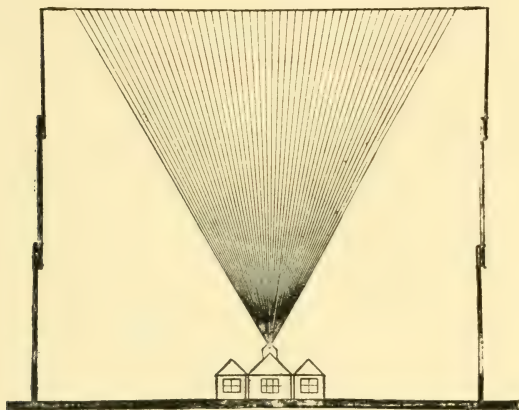
† 'The Principles of Electric Wave Telegraphy,' 1906, p. 506.

which wrecked the masts and aerial at Poldhu on September 18, 1901, but by the end of November the aerial was sufficiently restored to enable me to complete the preliminary tests which I considered necessary prior to making the first experiment across the Atlantic.

Another accident to the masts at Cape Cod seemed likely to postpone the tests for several months more. I therefore decided that in the meantime I would use a purely temporary receiving installation in Newfoundland for the purpose of testing how far the arrangements in Cornwall had been conducted on right lines.

The transmitting elevated conductor employed at Poldhu during the experiments with Newfoundland consisted of 50 almost vertical copper wires supported at the top by a horizontal wire stretched between two masts 48 metres high and 60 metres apart. These

FIG. 6



wires converged together at the lower end in the shape of a large fan, and were connected to the transmitting instruments situated in a building (Fig. 6).

The transmitting condenser used with this aerial had a capacity of $\frac{1}{50}$ th of a microfarad, and was charged to a potential sufficient to produce a suitable spark discharge between spheres 3 inches in diameter, $1\frac{1}{2}$ inch apart, the wave-length being 1200 feet.

The actual power employed for the production of the waves was about 15 kilowatts.

I left for Newfoundland on November 27, 1901, with two assistants. As it was impossible at that time of the year to set up a permanent installation with poles, I decided to carry out the experiments by means of receivers connected to elevated wires supported by balloons or kites—a system which I had previously used when

conducting tests across the Bristol Channel for the Post Office in 1897.*

It will be understood, however, that when it came to flying a kite on the coast of Newfoundland in the month of December, this method was neither an easy nor a comfortable one. When the kites were got up much difficulty was caused by the variations of the wind producing constant changes in the angle and altitude of the wire, thereby causing corresponding variations in its electrical capacity and period of electrical resonance. My assistants at Poldhu, in Cornwall, had received instructions to send on and after December 11, during certain hours every day, a succession of S's followed by a short message, the whole to be transmitted, at a certain pre-arranged speed, every ten minutes, alternating with five minutes' rest.

Owing to the constant variations in the capacity of the aerial wire in Newfoundland, it was soon discovered that an ordinary syn-tonic receiver was not suitable, although, at one time, a number of doubtful signals were recorded. I therefore tried various microphonic self-restoring coherers placed either directly in the aerial or included in the secondary circuit of an oscillation transformer, the signals being read on a telephone.

On December 12 the signals transmitted from Cornwall were clearly received, at the pre-arranged times, in many cases a succession of S's being heard distinctly, although probably in consequence of the weakness of the signals and the constant variations in the height of the receiving aerial no actual message could be deciphered.

The following day we were able to confirm the result. The signals were actually read by myself and by my assistant, Mr. G. S. Kemp.

I have often been asked why I adhered to the practice of transmitting series of the letter S for these tests. The reason is that the switching arrangements at the sending station at Poldhu were not constructed at that time in such a manner as to withstand long periods of operation—especially if letters containing dashes were sent—without considerable wear and tear, and that if S's were sent, an automatic sender could be employed. Moreover, the immediate object of these experiments was not to transmit actual messages across the ocean, but to ascertain the possibility of detecting the effects of electric waves at a distance of 2000 miles.

The result obtained, although achieved with imperfect apparatus, was sufficient to convince me and my co-workers that by means of permanent stations (that is, stations not dependent on kites or balloons for sustaining the elevated conductor) and by the employment of more power in the transmitters it would be possible to send messages across the Atlantic Ocean with the same facility with which they were being sent over much shorter distances.

* 'Signalling through Space without Wires,' lecture by Sir William Preece, Royal Institution, June 4, 1897. *Proc. R.I.* xv. p. 467.

About two months later, in February 1902, further tests were carried out between Poldhu and a receiving station on board the American Liner *Philadelphia*, en route from Southampton to New York. The sending apparatus at Poldhu was the same as that used for the Newfoundland experiments. The receiving aerial on the ship was fixed to the mainmast, the top of which was 60 metres above sea-level.

As the elevated conductor was fixed and not floating about with a kite, as in the case of the Newfoundland experiments, good results were obtained on a syntonic receiver, and the signals were all recorded on tape by the ordinary Morse recorder.

On the *Philadelphia* readable messages were received from Poldhu up to a distance of 1551 miles, S's and other test letters as far as 2099 miles.

The tape records of the signals are in my possession, and some of

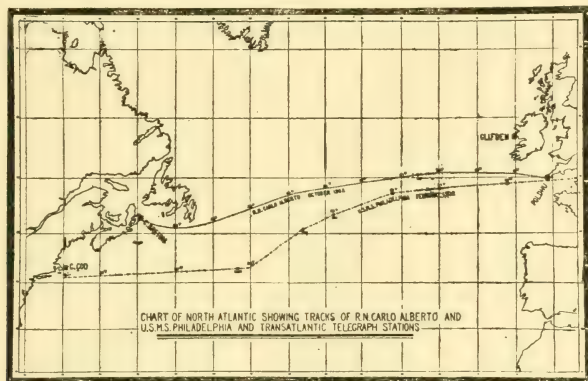


FIG. 7.

them are here exhibited to-night. The distances at which they were received are all verified and countersigned by the Captain and Chief Officer of the ship who were present during the tests.

Captain Mills, of the *Philadelphia*, was also good enough to mark on a chart, which I have here to-night, the various positions of the ship between England and America at which the communications from Poldhu were received.

Although I never had the slightest doubt in my mind as to the genuineness of what was accomplished between Poldhu and Newfoundland, the results obtained on the *Philadelphia* amply prove that the station at Poldhu was capable at that time of transmitting signals to a distance of at least 2000 miles, which is the distance separating Cornwall from Newfoundland, and that if it was practicable to send a message over 2000 miles of sea from shore to ship, it should also be practicable to send it over the same space of ocean from shore to shore.

A result of some scientific interest which I first noticed during the tests on the s.s. *Philadelphia* was the very marked effect of sunlight on the propagation of electric waves over great distances.*

At the time of these tests I was of opinion that this effect might have been due to the loss of energy at the transmitter by daytime, caused by the dis-electrification of the highly charged transmitting elevated conductor operated by the influence of sunlight. I am now inclined to believe that the absorption of electric waves during daytime is due to the ionisation of the gaseous molecules of the air effected by ultraviolet light, and as the ultraviolet rays which emanate from the sun are largely absorbed in the upper atmosphere of the earth, it is probable that the portion of the earth's atmosphere which is facing the sun will contain more ions or electrons than that portion which is in darkness, and therefore, as Professor J. J. Thomson† has shown, this illuminated and ionised air will absorb some of the energy of the electric waves.

The fact remains that clear sunlight and blue skies, though transparent to light, act as a kind of fog to powerful Hertzian waves. Hence the weather conditions prevailing in this country are usually suitable for long-distance wireless telegraphy.

Apparently the amplitude of the electrical oscillations and the lengths of waves radiated have much to do with the interesting phenomena, small amplitudes and long waves being subject to the effect of daylight to a less degree than large amplitudes and short waves. I never considered that this daylight effect would be an insuperable obstacle to transatlantic telegraphy, as sufficient sending energy could be used during daytime to make up for the loss of range of the transmissions.

Turning again to Newfoundland, I ought to add that the experiments could not there be continued or extended in consequence of the hostile attitude of the Anglo-American Telegraph Company, which claimed all rights for telegraphy, whether wireless or otherwise, in Newfoundland.

However, as I had received an offer of assistance from the Canadian Government, it was decided to resume the tests between Great Britain and Canada, and these tests were very greatly facilitated by the subsidy of £16,000 granted by the Canadian Government to support my experiments.

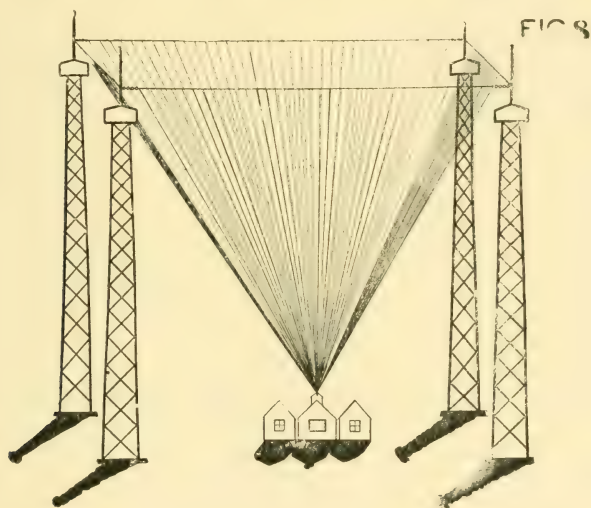
The construction of another long distance station was, therefore, commenced at Glace Bay in Nova Scotia, and very extensive tests and experiments were carried on with Poldhu during the latter part of 1902.

* Proceedings Royal Society, lxx. p. 344, 'A Note on the effect of Daylight upon the Propagation of Electro-magnetic Impulses over long distances.' Paper read June 12, 1902.

† Phil. Mag., August 1902, Ser. 6, iv. p. 253, J. J. Thomson.

Contemporaneously with the construction of the station at Glace Bay, alterations and modifications were executed at Poldhu. Four wooden lattice towers, each 210 feet high, were erected at the corners of a square of 200 feet side. The towers carried insulated triatic stays from which was suspended a conical arrangement of four hundred copper wires forming the aerial, put up in sections so that more or less could be employed (Fig. 8). The buildings for the generating plant were placed in the middle of the space between the towers. Additional machinery was obtained, and alterations carried out in accordance with the experience obtained from previous tests.

Identical towers and aerial arrangements were at that time adopted at the stations at Glace Bay, and at the similar installation in course of erection at Cape Cod, Mass.



In most of the experiments carried on from Poldhu the capacity of the sending condenser was $\frac{1}{30}$ th of a microfarad, the spark length $1\frac{3}{4}$ inch, and the wave-length 3600 feet. In these and subsequent tests the double condenser arrangement of Dr. Fleming was replaced by a single condenser, the arrangement being similar to that shown in Fig. 3.

During the time that constructional work was in progress at Glace Bay, I carried out some tests with Poldhu over considerable distances, and these tests were greatly facilitated by the interest taken in them by the Italian Government, which placed the cruiser *Carlo Alberto* at my disposal.

During these experiments the interesting fact was observed that, when using waves of over 1000 metres in length, intervening land or

mountains do not bring about any considerable reduction in the distance over which it is possible to communicate. Thus messages and press despatches were received from Poldhu at the positions marked on the map (Fig. 9), which map is a copy of the one accompanying the official report of the experiments.*

In December 1902, messages were for the first time exchanged at night between the stations at Poldhu and Glace Bay, but it was found that communication was exceedingly difficult and unreliable from England to Canada, whilst it was good in the opposite direction. The reason for this is that the Glace Bay station was equipped with more powerful and more expensive machinery—a condition rendered possible by the subsidy granted by the Canadian Government; whilst as regards Poldhu, owing to the uncertainty of what might or might not be the attitude of the British Government at that time towards the working of the station, my company was unwilling to expend



large sums of money for the purpose of increasing its range of transmission.

As, however, messages could be sent then for the first time by wireless telegraphy from Canada to England, inaugural messages were despatched to the Sovereigns of England and Italy, both of whom had previously given me much assistance and encouragement in my work, and who, by their gracious replies, attested their appreciation of the results which had been achieved.

Other messages were also sent to England by the Government of Canada.

I should perhaps mention that officers delegated by the Italian Government, and a representative of the London *Times*, were present at the transmission of the messages.

Further tests were shortly afterwards carried out with the long-distance station at Cape Cod in the United States of America, and a

* *Revista Marittima*, Rome, Oct. 1902.

message from President Roosevelt was transmitted from that station to His Majesty the King in London.

It is curious to note in regard to the transmission of this message that the energy employed at Cape Cod was barely 10 kilowatts; and it was not anticipated that this amount of energy would be sufficient to carry direct to Poldhu. The message was therefore transmitted from Cape Cod, instructions having been given to the operators at Glace Bay to be on the look-out, and to repeat wirelessly on to Poldhu any message received from Cape Cod, and my assistant, Mr. P. J. Woodward, at Poldhu, took in the message on one of my magnetic detectors.* The electro-magnetic waves conveying this message travelled therefore 3000 miles through space over the Atlantic, which distance included about 500 miles of land, following an arc of 45 degrees on a great circle.

In the spring of 1903 the transmission of news messages from America to the London *Times* was attempted, in order to demonstrate that messages could be sent from America by means of the new method, and for a time these messages were correctly received and published in that newspaper.

By reference to the files of the *Times* I find that 267 words of news, transmitted across the Atlantic by wireless, were published in the London *Times* during the latter part of March and the early part of April of that year.

A breakdown in the insulation of the apparatus at Glace Bay made it necessary, however, to suspend the service, and unfortunately further accidents made the transmission of messages uncertain and unreliable.

In consequence of this it was decided not to attempt for the time being the transmission of any more public messages until such time as a reliable service could be maintained in both directions under all ordinary conditions.

As I found that many improvements evolved during the course of the numerous tests and experiments could not be readily applied to the plants at Poldhu and Cape Breton, it was decided to erect a completely new long-distance station in Ireland, and to transport the one at Glace Bay to a different site in the vicinity, where sufficient land was available for experimenting with aerials of much larger dimensions than had been hitherto employed.

Experiments were, however, continued with Poldhu, and in October 1903 it became possible to supply the Cunard Steamer *ip Lucania* during her entire crossing from New York to Liverpool with news transmitted direct from the shore.

In November of the same year, tests, similar to those carried out

* Proceedings Royal Society, lxx. p. 341, 'Note on a Magnetic Detector of Electric Waves which can be employed as a Receiver for Space Telegraphy.'

with the Italian Cruiser, took place on behalf of the British Admiralty between Poldhu and H.M.S. *Duncan*.

Communication with Poldhu was maintained during the entire cruise of this battleship from Portsmouth to Gibraltar, and further communication was established between Poldhu and the Admiralty station situated on the Rock of Gibraltar.

It should be noted that the distance between Cornwall and Gibraltar is 1000 miles—500 over land and 500 over water.

The aerial at Poldhu was shortly afterwards extended by the addition of wires sloping downwards, umbrella-fashion, as shown in Fig. 10. This increased the capacity of the aerial, and some further tests were carried out with a station at Fraserburgh, in the north of Scotland. From these tests considerable advantage appeared to be

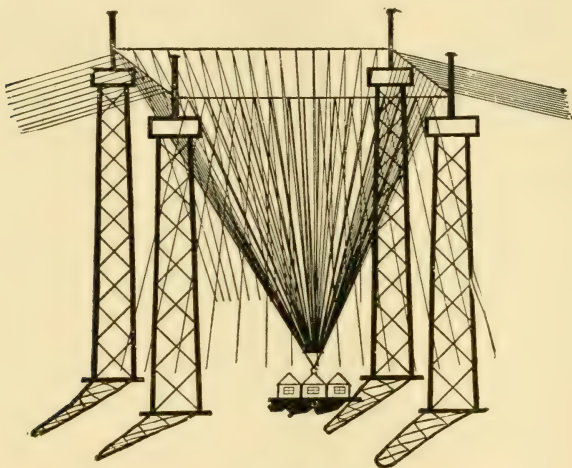


FIG. 10.

derived, at least for communication over land, by the adoption of much longer waves than had been hitherto employed, and with a wave-length of 14,000 feet it was found possible to telegraph over a distance of 550 miles with an expenditure of energy of about 1 kilowatt.

The operation of the long-distance stations in England and America made it possible to transmit messages to ships whatever their position, between Europe and North America; and to the Cunard Company belongs the credit of having greatly encouraged the long-distance tests, a circumstance which enabled them to commence, in June 1904, the regular publication on their principal vessels of a daily newspaper, containing telegraphic messages of the latest news from Europe and America.

This daily newspaper has now been adopted by nearly all the large Liners plying to New York and the Mediterranean, and it obviously owes its entire existence to long-distance wireless telegraphy. Therefore the tranquillity and isolation from the outside world, which it is still possible to enjoy on board of some ships, is rapidly becoming a thing of the past. But, however much travellers may sigh over the innovations which have lately been brought about, they seem anxious enough to avail themselves of the new method of communication on all possible occasions.

Early in 1905, the construction of the new station at Glace Bay was sufficiently advanced to allow of preliminary tests being carried out. The aerial was very large, and consisted of a vertical portion in the middle 220 feet long supported by four towers and attached to horizontal wires, 200 in number, each 1000 feet long, extending radially all round, and supported at a height of 180 feet from the ground by an inner circle of 8 and an outer circle of 16 masts (Fig. 11).

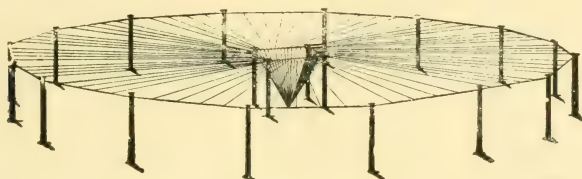


FIG. 11.

The natural period of oscillation of this aerial gave a wave-length of 12,000 feet.

The capacity employed was 1·8 microfarads, and the spark length $\frac{3}{4}$ inch.

Signals and messages from this station were received at Poldhu by day as well as by night, but no commercial use of the station was made at that time, in consequence of the fact that although the signals came through by day as well as by night they were exceedingly weak and faint, and also because the corresponding station on the same plan had not yet been erected in Ireland.

A further step in advance was the adoption at the transatlantic stations of the directional aerial shown in Fig. 12.* The ordinary wireless telegraph aerials, which I have already described, send out electric radiation equally in all directions. This is, however, in many cases a disadvantage. Many suggestions respecting methods for limiting the direction of radiation have been made by various workers, notably by Messrs. Artom, Braun, and Bellini Tosi.

* 'On methods whereby the radiation of electric waves may be mainly confined,' etc. Proceedings of the Royal Society, G. Marconi, A. lxxvii. 1906.

In some of my earliest experiments, in 1896, I used copper mirrors, by the aid of which it was possible to project a beam of electric radiation in a certain direction, but I soon found that this method would only work over short distances.

About three years ago I again took up the subject and was able to determine that by means of horizontal aerials, disposed in a particular manner, it was possible to confine the effects of electric waves mainly to certain directions as desired. True, the limitation of transmission to one direction is not very sharply defined, but it is nevertheless very useful. The practical result of this method has been so far that messages can be sent over considerable distances in the desired directions, while they travel only over a comparatively short distance in other directions, and that, with aerials of moderate height, greater efficiency in a given direction can be obtained than can be obtained all round by means of the ordinary aerials.

When this type of aerial was adopted at Glace Bay a considerable strengthening of the received signals at Poldhu was noticed. It was therefore decided to adopt the directional aerial at all long-distance stations.



FIG. 12.

A further improvement introduced at Clifden and Glace Bay consisted in the adoption of air condensers, composed of insulated metallic plates suspended in air at ordinary pressure. In this manner it is possible to prevent the dissipation of energy due to losses caused by the dielectric hysteresis in the glass dielectric of the condensers previously employed; and a very appreciable economy in working, resulting from the absence of breakages of the dielectric, is effected. These air condensers, which have been in use since May of last year, have been entirely satisfactory.

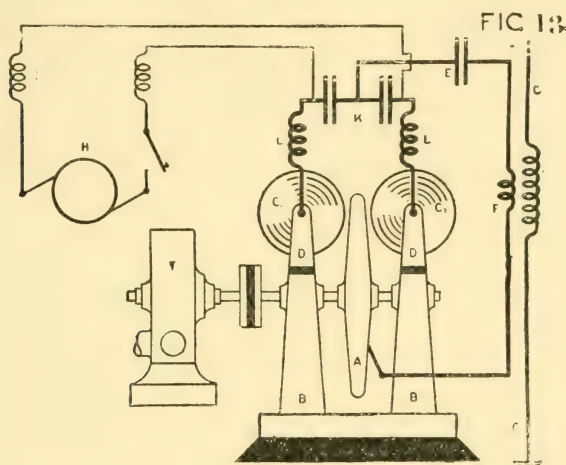
After very considerable delay and expense, the new station at Clifden was got ready for tests by the end of May 1907, and experiments were then commenced with Glace Bay.

I ought to say that in the constructional and experimental work carried out at the Canadian station I have received valuable assistance from Mr. R. N. Vyvyan; and at Poldhu and Clifden Mr. W. S. Entwistle has assisted me in carrying out much original research work, required in the designing of novel apparatus necessary for long-distance transmission. Mr. P. J. Woodward has likewise rendered me valuable help in the receiving arrangements, and has on nearly every occasion, been in charge of the receiving apparatus during

the initial long-distance tests. I must also add that I have, for many years, had the benefit of the advice of Dr. J. A. Fleming, of the University of London, whose experience on all matters dealing with high-tension work and electrical oscillations is universally acknowledged.

The wave-length used during these tests was 12,000 feet, the capacity employed 1.6 microfarads, and the potential to which the condenser was charged 80,000 volts.

Good signals were obtained at Cape Breton from the very commencement of the tests, but some difficulty was encountered in consequence of the effects of atmospheric electricity due to the prevalence of thunderstorms in the eastern part of Canada during the first few days of the tests.



Simultaneously with these tests others were carried out from Poldhu to Glace Bay with a new system of transmitting apparatus, by means of which continuous or semi-continuous oscillations could be produced.

Proportionately to the energy employed the signals from Poldhu were so much better than those from Clifden that I decided at once to adopt this new method of transmission at Glace Bay and Clifden. The apparatus which I have been using for producing continuous or closely adjacent trains of electric oscillations is as follows :—*

A metal disc A (Fig. 13), insulated from the earth, is caused to rotate at a very high speed by means of a high speed electric motor or steam turbine.

Adjacent to this disc, which I shall call the middle disc, are

* Patent Application No. 20,119, September 9, 1907.

placed two other discs C_1 , C_2 , which may be called polar discs, and which also can be rotated at a high rate of speed.

These polar discs should have their peripheries very close to the surface or edges of the middle disc.

If a small amount of energy is used, stationary knobs or points may be used in place of the side discs.

The two polar discs are connected respectively through suitable brushes to the outer ends or terminals of two condensers K , joined in series, and these condensers are also connected through suitable inductive resistances to the terminals of a generator, which should be a high-tension continuous-current dynamo.

On the high speed or middle disc a suitable brush or rubbing contact is provided, and connected between this contact and the middle point of the two condensers is inserted an oscillating circuit consisting of a condenser E in series with the inductance, which last is connected inductively or conductively to the aerial.

If the necessary conditions are fulfilled, and a sufficient E.M.F. is employed, a discharge will pass between the outer discs and the middle disc, which discharge is neither an oscillatory spark nor an ordinary arc, and powerful oscillations will be created in the signalling condenser E and oscillatory circuit F .

I have found that in order to obtain good effects a peripheral speed of over 100 metres per second is desirable; therefore particular precautions have to be taken in the construction of the discs. Electrical oscillations of a frequency as high as 200,000 per second can be obtained.

The apparatus which I have had so far constructed is on a large scale, and unsuitable for demonstrations in a lecture hall. I hope, however, very soon to be able to show before some scientific society the effects produced.

The apparatus works probably in the following manner:—

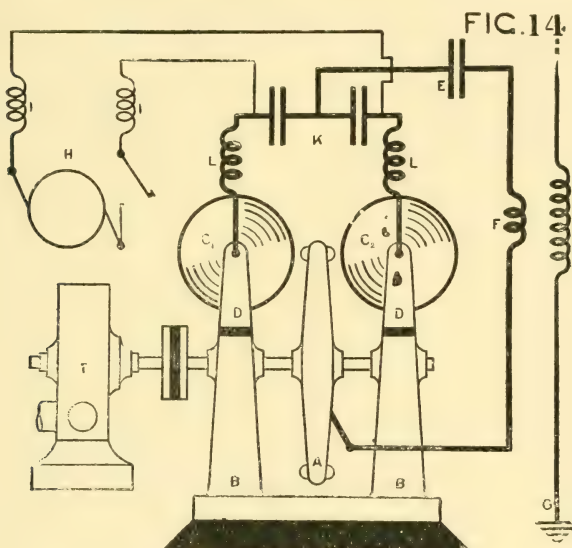
Let us imagine that the source of electricity is gradually charging the double condenser and increasing the potential at the discs, say C_1 positively and C_2 negatively; at a certain instant the potential will cause the charge to jump across one of the gaps, say between C_2 and A . This will charge the condenser E , which will then commence to oscillate, and the charge in swinging back will jump from A to C_1 , which is charged to the opposite potential. The charge of E will again reverse, picking up energy at each reversal from the condensers K . The same process will go on indefinitely, the losses which occur in the oscillating circuit $E F$ being made good by the energy supplied from the generator H .

If the disc is not rotated, or rotated slowly, an ordinary arc is at once established across the small gaps, and no oscillations take place.

The efficient cooling of the discharge by the rapidly-revolving disc seems to be one of the conditions necessary for the production of the phenomena.

By means of this apparatus, tests were carried out, but it was found, as was to be expected, that the oscillations were too continuous and of too high a frequency to affect a receiver, such as the magnetic detector, unless an interrupter was inserted in one of the circuits of the receiver. A syntonic coherer receiver would, however, work, in consequence no doubt of the considerable rise of potential which occurred at its terminals through the cumulative effect of resonance.

The best results over long distances have, however, been obtained by a disc as shown in Fig. 14, in which the active surface is not smooth, but consists of a number of knobs or pegs, at the end of which the discharges take place at regular intervals. In this case, of



course, the oscillations are not continuous, but consist of a regular succession of trains of undamped or slightly damped waves.

In that manner it is possible to cause the groups of oscillations radiated to reproduce a musical note in the receiver, distinguishable in a telephone, and thereby it is easier to differentiate between the signals emanating from the transmitting station and noises caused by atmospheric electrical disturbances. By this method very efficient resonance can moreover be obtained in appropriately designed receivers.

A few tests with apparatus based on the principle described were carried out between Glace Bay and Clifden, and on October 17 of last year a limited service for press messages was commenced between Great Britain and America. Difficulties were experienced, however,

over the question of rates with the telegraph companies working the land-lines between Glace Bay and the principal towns of Canada and the United States, and at present the strange anomaly exists that the rates for press messages on the American land-lines are much cheaper for messages going from England to New York than in the reverse direction.

On February 3 this service was extended to ordinary messages between London and Montreal.

The stations at Clifden and Glace Bay are not complete, and the necessary duplication of the running machinery has not yet been executed, but nevertheless communication across the Atlantic has never been interrupted for more than a few hours since the commencement of commercial working on October 17 last.

There have, however, been several serious interruptions at Clifden, due to the unreliability of the land-lines connecting Clifden to the ordinary telegraph system. On one occasion one of these interruptions lasted from 5.20 p.m. to 10.30 a.m., a duration of 17 hours; and on another occasion the land telegraph wires were struck by lightning and disabled for 12 hours. There have also been recorded numerous other interruptions of shorter duration, which resulted in delays to private and press messages.

Further delays have also been caused through interruptions on the land-lines connected with the Canadian station.

During the first months, on account of imperfections in the auxiliary apparatus connected principally with the operating keys and switches, only a fraction of the available transmitting power was used. In consequence of this the speed of transmission was slow, and short interruptions somewhat frequent.

Many of these difficulties have now been overcome, and in a few more months, when it should be possible to utilise the full power available, a very much greater speed and efficiency is likely to be attained.

Messages can now be transmitted across the Atlantic by day as well as by night, but there still exist certain periods, fortunately of short duration, when transmission across the Atlantic is difficult and at times ineffective, unless an amount of energy greater than that used during what I might call normal conditions is employed.

Thus, in the morning and evening when, due to the difference in longitude, daylight or darkness extends only part of the way across the Atlantic, the received signals are weak and sometimes cease altogether.

It would almost appear as if illuminated space possessed for electric waves a different refractive index to dark space, and that in consequence the electric waves may be refracted and reflected in passing from one medium to the other. It is therefore probable that these difficulties would not be experienced in telegraphing over equal distances from north to south, or *vice versa*, as in this case the passage

from daylight to darkness would occur almost simultaneously in the whole of the medium between the two points.

In the same manner a storm area in the path of the signals often brings about a considerable weakening of the received waves, whilst if stormy conditions prevail all the way across the Atlantic no interference is noticeable. Electric wave shadows, like sound shadows, may be formed by the interference of reflected waves with the direct waves, whereby signals may be much less effective or imperceptible in the area of such electric wave shadow.

In the same manner as there exist periods when signals across the Atlantic are unusually weak, there exist other conditions, especially at night, which make the signals abnormally strong. Thus on many occasions ships, and stations equipped with apparatus of a normal range of 200 miles, have been able to communicate over distances of over 1000 miles. This occurred recently when a ship in the English Channel was able to correspond with another in the Mediterranean. But the important factor about wireless telegraphy is that a service established for a certain distance shall be able to maintain reliable communication over that distance.

The erection of long-distance stations for the purpose of telegraphing across the Atlantic met at the outset with the severe criticism of a certain important section of the English technical press, which, although one would imagine it existed for the purpose of encouraging and promoting the progress of electrical science and industry, always seemed more inclined to champion the particular interests of the cable companies.

Without wishing to enter into any controversy on this point, I venture to predict that some of the statements published with reference to long-distance wireless telegraphy will make very amusing reading in a few years to come.

I am here reminded of an anecdote related to me about Michael Faraday. He had been displaying what appeared to be clumsily made rings or coils which, as we all now know, were the forerunners of the present-day dynamo. It appears that someone, pointing to the coils, asked whatever could be the use of such apparently useless articles. Faraday's answer was in the form of a question: "What is the use of a baby?" he said.

The analogy is, of course, not quite applicable to wireless telegraphy at the present day. This new method of communication is no longer an infant, but has now reached the stage of vigorous youth, and is fast approaching manhood.

Long-distance stations are now in course of erection in many parts of the world, the most powerful of all being that of the Italian Government at Coltano, and I have not the slightest doubt but that telegraphy through space will soon be in a position of affording communication between distant countries at cheaper rates than can be obtained by any other means.

As to the practicability of wireless telegraphy working over long distances, such as that separating England from America, there is no longer need for any doubt. Although the stations have been worked for only a few hours daily, 119,945 words of press and commercial messages had been transmitted across the ocean by this means up to the end of February last since the service was opened.

The best judges of a service are those who have made use of it, and amongst newspapers, the chief users have been the *New York Times* and the *London Times*, which have already publicly expressed their opinion of this new method of communication.

I might mention that recently I received by wireless from the *New York Times* a message of which the following is an extract :—

“ . . . during the five months since it opened in October last . . . the *Times* has received from its correspondent in England and on the Continent news despatches totalling 68,404 words, sixty-eight thousand four hundred and four words, promptly and efficiently transmitted by your system. *N.Y. Times.*”

Whether the new telegraphy will or will not injure or displace the cables is still a matter of conjecture, but in my opinion it rests a good deal on what the cables can do in the way of cheaper rates.

It is not, as some appear to imagine, either the business or the wish of those concerned in the development of wireless telegraphy to injure the cable industry.

They are endeavouring at present to demonstrate the new method is not only valuable for shipping, but that it should be also regarded as a new and cheaper method of communicating with far distant countries.

Whatever may be the view as to its shortcomings and defects there can be no doubt but that wireless telegraphy across the Atlantic has come to stay, and will not only stay but continue to advance.

Cable telegraphy across the Atlantic was subjected at the commencement to a series of discouraging failures and disappointments, but whatever its difficulties I think I am not unjust in saying that it enjoyed one advantage over wireless telegraphy, namely, that it was free from the natural hostility of vested interests representing over 60 millions sterling, now invested in cables, which rightly or wrongly consider long-distance telegraphy as menacing their interests.

In seven years the useful range of wireless has increased from 200 miles to 2500. In view of that fact, he will be a bold prophet who will venture to affirm what may not be done in seven years more.

I shall not presume to say that at the present moment the wireless telegraph service between London and New York is as efficient and as rapid as that supplied by the cables. For nearly 50 years the Transatlantic cable organisation has been in existence, and there are now 16 cables working across the North Atlantic, so that in the case of a breakdown of one cable the traffic is sent by one of the others,

Moreover, long experience has served to bring their land-line connections to a high state of perfection. Nevertheless, I am convinced that if there were only one cable and the present wireless service, interruptions would be more frequent and much more serious in the case of the cable than in that of wireless.

We have only to look towards those parts of the Globe, such as India, South Africa, and so forth, where trans-oceanic communication is dependent upon only one or two cables, and the force of my remarks will be more readily appreciated. The cases of delay in regard, not only to commercial messages, but also to Government despatches, are only too frequent, as no doubt you have observed from time to time in the daily press.

Among many people there seems to be a rooted conviction that wireless telegraphy is not suitable for the handling of code or cipher messages. Whatever gave rise to this idea I do not know, but I wish to emphasise that it is purely fictitious. Code messages can be sent just as well by wireless as by ordinary methods of telegraphy.

I need hardly say that most of the wireless messages passing between war ships are now expressed in code, as are likewise the majority of the commercial messages handled by the Clifden and Cape Breton stations.

I do not wish to claim that wireless telegraphy is infallible, and although errors do sometimes occur, it is absolutely certain that, having regard to the London and Montreal service, most of the mistakes can be traced to the land-line telegraph transmission between London and Clifden, and between Glace Bay and Montreal.

I find, however, that probably the greatest ignorance prevails in regard to what is termed "tapping," or intercepting wireless messages.

No telegraph system is secret. The contents of every telegram are known to every operator who handles it.

It is incorrect to suppose that anyone can at will pick up wireless messages. On the other hand, it is easy for anyone knowing the Morse Code to step into many telegraph offices and read off the messages by the click of the instruments.

Further, it is practicable, but illegal in this country, to make arrangements so that messages which pass over a telegraph line can be read by persons who are not operating the line at all.

It is also expensive to erect a tall pole or tower and fix up all the instruments which are necessary before wireless messages can be taken in, and, moreover, such proceeding is contrary to the law of the land.

It should be remembered, too, that any ordinary telegraph or telephone wire can be tapped, and the conversation going through it overheard, or its operation interfered with. Results published by Sir William Preece show that it is possible to pick up at a distance, on another circuit, the conversation which may be passing through a telephone or telegraph wire.

At Poldhu, on a telephone connected to a long horizontal wire,

the messages passing through a Government Telegraph Line a quarter of a mile away can be distinctly read.

In a paper on his method of Magnetic Space Telegraphy, Sir Oliver Lodge mentions an occasion on which he was able to interfere, from a distance, with the working of the ordinary telephones in the city of Liverpool.

Many instances can be enumerated showing that Electric Light and Tramway Power Stations have interfered with cables and land-lines.

Nevertheless, there are penalties attached to the tapping of a telegraph wire, and it ought to be as well known that, since the passing of the Wireless Telegraphy Act, there are penalties involved if any wireless stations are erected or worked without the consent of the Postmaster-General.

In conclusion I may say that I am very confident that it is only a question of time, and that not a very long time, before wireless telegraphy over great distances, possibly round the world, will become an indispensable aid to commerce and civilisation.

[G. M.]

WEEKLY EVENING MEETING,

Friday, March 20, 1908.

DONALD W. C. HOOD, ESQ., C.V.O. M.D. F.R.C.P.,
Vice-President, in the Chair.

MR. JOHN MILNE.

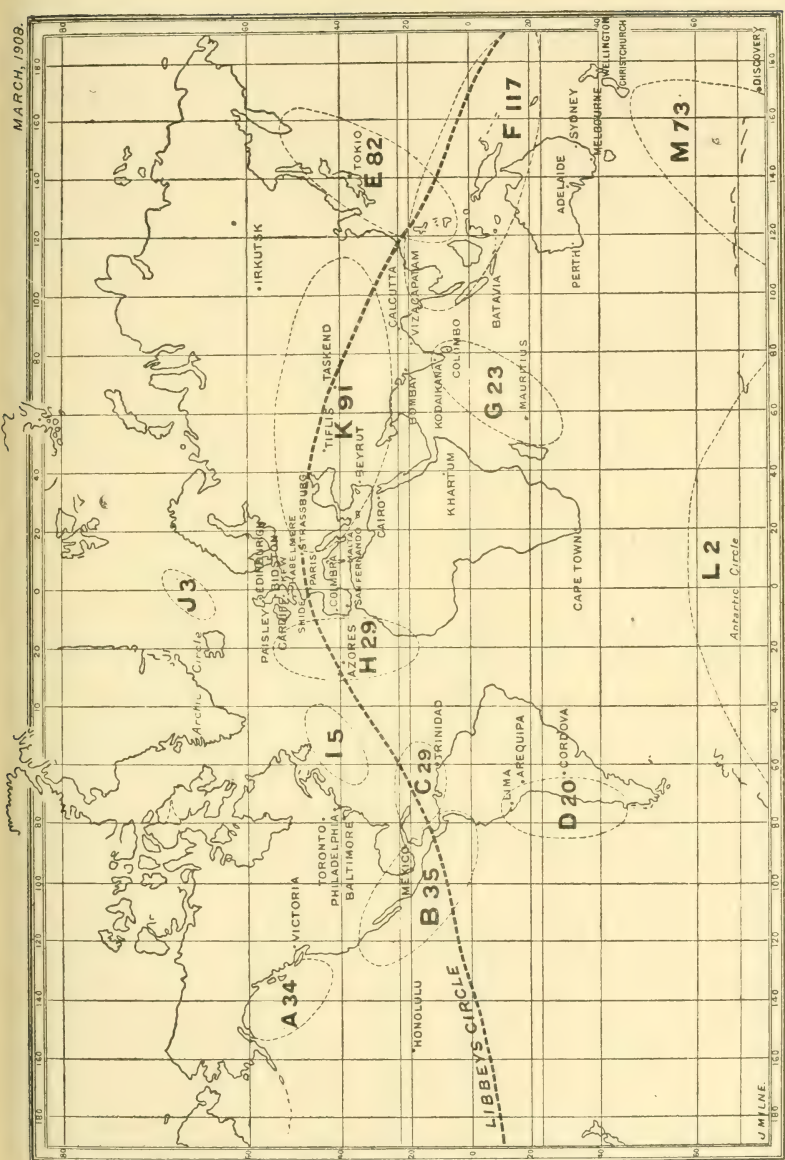
Recent Earthquakes.

UNTIL recent years the attitude of the ordinary Englishman with regard to earthquakes has been one of apathy. He argued that although every year 30,000 earthquakes might occur in the world, his country only contributed about half a dozen, and these because they were so small did more to excite curiosity than to create alarm. Although in 1883 Colchester, and in 1896 Hereford lost a few chimney pots, and buildings were unroofed, also at intervals, reckoned by one or two hundred years, London has been shaken, still England could not be regarded as an earthquake-producing country. British made earthquakes may be of rare occurrence, but should there be any relief of seismic strain similar to that of 1883 or 1896 in the synclinal on which our great metropolis stands, we might find as many chimney pots in the streets as there are inhabitants. A suggestion of this kind, however, does not disturb the mind of our ordinary Englishman. Hints respecting the possible instability of his country produce no effect, and he fails to see why he or his government should be called upon to support seismological investigations. Recent earthquakes, have, however, modified his opinion, and although England may be free from earthquakes he finds he has to insure against and pay for the effects which these disturbances have caused in distant places. By observations on what has stood and what has been destroyed after violent shakings of the ground, and, as the result of investigations together with elaborate and costly experiments carried out entirely in Japan, not only have new methods of construction been formulated, but these have had extensive applications. Experience has shown that the new types of buildings stand whilst the old ones are shattered. At the present moment, Valparaiso, San Francisco, Kingston, and very many other cities, towns and villages, not only in America, but also in Europe and Asia, have by recent earthquakes been reduced to heaps of debris. When these are reconstructed, it is extremely likely that the well-tested rules and methods, the outcome of applied seismology, will not be neglected.

Seismological investigations have been made, not only for scientific reasons, but to minimise the loss of life and property. In con-

nection with the destruction of San Francisco alone, we are told that British insurance companies are called upon to meet claims amounting to 12,000,000*l.*, while losses of like character may have to be met in other parts of the world. The Englishman living on his own little patch of *terra firma* is continually paying for earthquake effects all over the world. The thinking man now realises that insurance rates in many countries must vary with the seismicity of a district together with the character of the structures to which they refer. Sub-oceanic seismic activity frequently results in the failure of cables. It is therefore of extreme importance that the sites of these submarine disturbances should be located (see map). In these and in many other ways it is easy to show that England has probably a greater practical interest in the results of seismological investigation than any other country. Finance and earthquake effects are close relations. Another incentive to the removal of apathy in regard to seismology lies in the fact that the mind of the public like that of the individual becomes fatigued by repetition. What is asked for is something new, and if possible, it should be sensational. Newspapers and magazines do all they can to relieve this craving, with the result that the public is liberally supplied with stories about big catastrophes and deductions based thereon. A new "*hors d'œuvre*" has been added to the daily scientific menu, and the halfpenny paper and the sixpenny magazines have given a stimulus to investigations bearing upon earth physics.

In countries where earthquakes have been severe and where by their frequency they are continually forcing themselves upon public attention, a desire to investigate is furnished by the earth itself. Chili is now arranging to have a system of observing stations. Jamaica is speaking about the same, whilst the United States are extending what they now possess. Three recent earthquakes have awakened three different Governments to the fact that, although schoolmasters may not flog their children, nature is not always as indulgent to its people. Japan, in addition to establishing stations in Formosa, Saghalin, China and Korea, has already more than 1000 observing stations, 120 of which have instruments for recording local shocks. For seismological investigations the government of that country annually allocates 1000*l.* to 5000*l.*, and this is outside expenditure in connection with the chair of seismology and concomitant with investigations of earthquakes in foreign countries. During the last 10 or 12 years Japan has issued about 70 quarto volumes bearing upon seismological investigations. Russia has a series of well-equipped stations within its borders. For very many years Italy has given great attention to the movements of the ground. These are recorded at several hundreds of stations, 160 of which are provided with instruments. Austria, Germany and many other States are also devoting great attention particularly to the collection of earthquake statistics. I fail, however, to see that these statistics,



STATIONS IN CO-OPERATION WITH THE BRITISH ASSOCIATION WORKING WITH SIMILAR INSTRUMENTS (MILNE PENDULUM).
 NUMBER OF WORLD-SHAKING EARTHQUAKES SINCE 1899 WHICH HAVE ORIGINATED IN DISTRICTS MARKED A, B, C,
 ETC., ARE INDICATED.

which are necessarily imperfect, will pass beyond the borderland of local interest. So far as I am aware, all foreign stations are subsidised by their respective governments. Great Britain enjoys the co-operation of 45 stations provided with similar instruments, which are distributed fairly evenly over the four quarters of the world. The names and positions of these stations are shown upon the accompanying map. The home stations are supported by the British Association, the Royal Society, the "Daily Mail," Mr. M. H. Gray and other private individuals. So far as the recording of world-shaking earthquakes is concerned, I believe the British co-operation to be, at the present time, quite equal to a combination of the stations of all other countries. The last outcome in connection with observational seismology has been the establishment of an International Seismological Association. The central bureau is in Strassburg, its president is Prof. A. Schuster, and its general meetings take place once every four years. I am not aware that France has formally announced its adherence. The British Government, by subscribing 160*l.* a year to the central bureau has accepted a shelter from a Continental Aegis. For nearly 50 years the British Association has encouraged seismological research, but whatever prestige it may have gained, together with its attendant commercial and other advantages, these are passing under a new *régime* across the Channel.

A government of a country does not wish to seek abroad for an explanation why telegraphic messages have ceased to flow. To confirm, extend, or disprove a cablegram, a government, a business house, or the public of a given country would like to obtain information within its own boundaries. When a country or a colony finds itself cut off from the outside world in consequence of cable interruption, that country or colony together with other countries would like to have a ready means of saying whether the interruption had been due to submarine disturbances or to some other operation, as for example, war. Those who lay cables would prefer to have information as to positions of sub-oceanic sites of seismic activity from records made in their own country rather than those which had been made abroad. When after great convulsions cities have to be rebuilt, and there are many at the present moment, it is natural that information bearing upon reconstruction to reduce earthquake effects would be sought for at the world's central office, and those who supply information would in all probability supply engineers and material. Insurance companies who wish to apportion rates to risks when insuring against earthquake effects, might also think it best to seek their information at a central bureau. After an earthquake when such companies are called upon to pay the insured, many difficult questions arise which can only be answered by seismograms. Millions of pounds sterling are dependent upon these records, and it is therefore important that the same should be readily accessible. A seismogram which travels quicker than a telegram may affect the stock exchange. We no more



THE FOLDS AND PROBABLE DIRECTION OF FAULT LINES IN THE ATLANTIC.

require a central bureau to discuss applied seismology than we do to discuss the construction of torpedoes or flying machines.

A discovery which during the last few years has done much to popularise Seismology is the fact that a very large earthquake originating in any one part of the world may be recorded in any other portion of the same. This means that the opportunity for carrying on Seismological research is not a monopoly enjoyed only by those who reside in earthquake countries. Although only a few persons in Great Britain have been privileged to feel one of its home-made tremors, everyone of its inhabitants are very many times per year moved by earthquakes. Back and forth motion of the ground is performed too slowly for us to feel, while, if there is a movement like the swell upon an ocean, the undulations are too long and flat for us to see.

Waves start out from their epicentral area, which is a district that has been fissured and shattered by the formation or extension of large faults in all directions. Observation, however, shows that these waves are propagated farthest in one particular direction. For example, the chief movement following the San Francisco earthquake which originated from fault lines running parallel to the coast of California, was much more marked in countries lying to the east or west of California than in countries lying towards the south. England and Japan obtained large records of the disturbance, while in Argentina the records were extremely small. In the case of the Jamaica earthquake where the lines of origin ran east and west, the phenomenon was reversed. Toronto received a large quantity of motion, and England a very little. Another peculiarity of this phase of earthquake motion is that it may be propagated in one direction round the world to a greater distance than in an opposite direction. The suggestion is that the initial impulse was delivered in the direction towards which motion was propagated farthest. If for illustration we assume that the slip on a fault line has been downwards towards the east, then the motion would travel towards the east farther than it would towards the west. That which happens, corresponds to what we see if we dip the blade of a spade in water and suddenly push the blade in some particular direction. The water waves thus created travel farthest in the direction of the impulse.

Another curious phenomenon connected with the large waves of certain earthquakes is that they can pass their equatorial or quadrantal region unobserved. They may be very marked for 1000 miles round their origin, and recordable, but much reduced in size, about their antipodes, but not recordable in between. For example, an earthquake originating near New Zealand may be recorded in that country, but not in India, Egypt, West Asia, or east of Europe, but in Britain it may make itself evident by the thickening of a photographic trace. The phenomenon may be compared to a water wave running down an expanding estuary. At the mouth of such an estuary it may have

become so flat that it is no longer recognisable. Should it, however, run up a second estuary, we can imagine concentration taking place, so that near the top of the second estuary, it would eventually become instrumentally recordable. In these antipodean survivors, we see the final efforts of a dying earthquake. It is only occasionally that the precursors and the followers of these large waves have sufficient energy to reach their antipodes. They die *en route*. The former, notwithstanding their comparative feebleness, because they throw considerable light upon the internal constitution of our earth, are the most interesting feature in a seismogram. They are of two kinds, a first phase and a second phase. These are usually regarded as compressional and distortional modes of wave propagation. The large waves are probably quasi-elastic gravitational waves, something like an ocean swell, which travel round the world with a constant velocity of about 3 km. per sec., causing continental surfaces to rise and fall like huge rafts upon a heaving ocean. The precursors behave quite differently. Phase I. may commence with a velocity of 3 or 4 km. per sec., but as the length of the wave path increases, this quickly rises to 10 and thence to a maximum of 12 km. per sec. These paths are assumed to be along chords, and so long as these chords do not lie at a depth greater than 20 or 30 miles, the speeds are such as we should expect to find in materials like those composing the outer surface of our earth. These waves, therefore, indicate a thickness for the earth's crust, comparable to thicknesses which have been arrived at by other lines of argument. The rapid approximation to uniform speed suggests that below a depth of 20 or 30 miles we enter a nucleus which is very rigid and fairly homogeneous. The second phase waves, up to a distance of 120° from their origin, have a speed of about 6 km. per sec. For longer paths, Mr. R. D. Oldham points out that their velocity is apparently suddenly reduced. He seeks for an explanation of this by postulating the existence of a central core in the earth where waves are retarded and refracted with the result that the wave paths no longer follow chords. These waves may, therefore, emerge on the surface of the earth after having passed relatively to their starting point on the farther side of its centre. Whether we do or do not accept this central core, it is clear that the new seismology has added in a very marked manner to the knowledge we formerly possessed respecting the interior of the globe upon which we live. Our ideas respecting its homogeneity and its great rigidity have been changed by seismological investigations.

When large earth waves sweep round the world, it is found that at particular stations magnetic and electrometer needles have been disturbed. Magnetometers, when installed at Toronto, do not appear to have responded to the slow undulations of the earth's surface while the same instruments after being removed to Agincourt, only 10 miles distant, are now affected. The inference from this and observations in other parts of the world is that the movements, rather than being

caused mechanically, may be due to the disturbance of some adjacent magnetic magma. If this is the case, then at particular stations where the movements due to teleseisms correspond with unusual disturbance of magnetic needles inasmuch as a magnetic magma is denser than common rock, at these stations the value for g should be higher than that which would be anticipated. For certain stations this appears to be the case.

Another series of investigations which may widen our knowledge respecting conditions and operations beneath our feet, are based upon the light effects which have been so frequently observed at the time of large earthquakes. Accounts of luminosity in the heavens and on hills as accompaniments to large earthquakes are common. At the time of the Valparaiso earthquake, August 17, 1906, the attention of very many people was attracted to lights which appeared upon the hills. Captain Taylor, of the R.M.S. "*Orissa*", compared these to chain lightning which extended as far as the eye could reach. An acquaintance of mine, Mr. G. E. Naylor, of Valparaiso, told me that he saw the lights repeatedly, and they took place immediately before a shock, there being only a fraction of a second of time between the two. He described them as having a bluish tinge, to others, however, they appeared yellowish. An ordinary explanation for these appearances is that they are due to the rubbing together of rock surfaces or the discharge of frictionally produced electricity. These observations suggest that with a megaseismic collapse not only do we get mechanical disturbances which pass through and over the surface of the world, but that part of the initial energy at the origin is converted into some other form of energy which possibly may find a response at very distant places. This latter transmission would, however, take place with a velocity comparable with that of light. If anything of this sort has a real existence seismologists may hope to record earthquakes at the moment they take place. This consideration and the observation that from time to time a quarry in the Isle of Wight, known as Pan Chalk Pit, appeared to me to be luminous suggested the possibility of hypogenic activities giving evidence of their existence in the form of light. Pan Pit faces north and in winter it is not reached by the sun. Its glowings apparently rise and fall in intensity, and are most noticeable after a dull damp day. The experiments I made were as follows: at the end of a chamber 20 yards from the mouth of a tunnel driven into the chalk, a hole about 2 feet square was excavated. Into this a box with a light-proof door was cemented. The back of the box which touched the chalk was made of zinc. In the zinc three holes of different sizes were made along a vertical line. A cylindrical drum, covered with bromide paper and driven by clockwork, was brought up to within one-eighth of an inch to these holes. A rim on the bottom part of the drum had a clearance given to it by cutting a horizontal slit in the zinc plate beneath the holes. Neither the drum, the paper, or the rim touched the zinc plate or the chalk. The rate of movement of the paper was 90 mm. per day. A small electric

lamp moved about outside the box produced no effect upon the paper inside. A self-recording thermometer and a hygrometer showed that the temperature and the moisture in the chamber were practically constant. A similar piece of apparatus was installed at a depth of 160 feet, in the King Edward Mine, Camborne, Cornwall. These experiments were commenced at Pan Chalk Pit in February 1903, and were continued for four months. They were taken up again in the middle of August 1906, and lasted eight months. A sheet of paper on development was frequently quite clear, but at times it was partly or entirely marked with dark bands, black lines, round black spots, or semi-circular spots along the lower edges. At Shide, the dark bands have not been numerous, but they occurred on nearly all the sheets from Camborne. In certain cases we appear to have three bands, the positions of which apparently coincide with the three holes in the zinc plate. In some of these bands there are hard black lines broken along their length and made up of black spots.

The black spots vary in diameter from a fraction of a millimetre to 8 millimetres. In the centres of some of these there is a small white or brownish spot. As pointed out by Mr. W. H. Bullock, of Newport, these closely resemble spots which can be produced on bromide paper by a tiny electric spark. During a week we may have either no spots, one spot, or a hundred spots. The semicircular spots which I have called *singeings*, are found on the lower edge of the paper where the brass cylinder joins the aluminium rim. There may be two or three of these per week, whilst at other times they occur at intervals of about half-an-hour. As only ten black spots occurred at the time of large earthquakes we can only regard these as coincidences. Neither dark bands, spots nor *singeings* appear to be connected beyond what I have mentioned with any particular meteorological conditions. Neither is there any reason for supposing that these effects are due to radio-activity. If a piece of bromide paper is sealed up in a black envelope and another piece is placed in a black envelope, which has a thin glass window, and these are laid on a surface of chalk, the glass window touching the same, say for a period of several days, it was found after development that one piece of paper showed the image of the window, whilst the other had only stains which might be attributed to dampness. With the object of determining whether micro-organisms played any part in the phenomena observed, my friend, Dr. R. C. Brown, M.D., of Parkhurst, has made cultures from scrapings from the surface of the chalk before which my cylinder was exposed. Cultures were also made from scrapings taken from the open chalk. Micro-organisms were found in both. These have been exposed to a moving photographic surface similar to that used in the pit, but they gave no evidence of luminosity. The conclusion for the present is that the luminosity occasionally seen at Pan Pit may result from a very feeble brush or glow-like electrical discharge. If this be the case it would also account for the bands on the photographic paper, the other markings being due to minute sparks.

Moreover, if this is so, and we assume that silent electrical adjustments have a real existence, it is difficult to escape the conclusion that these must have an effect on what we call "climate," and hence upon everything that lives upon the surface of the globe. We have many instances of places only separated by a few miles, as for example, Newport and Sandown in the Isle of Wight, or Bournemouth and Swanage, the climates of which are said to be very different. The thermometer, barometer, and hygrometer do not explain these differences, the only apparent difference between such places appears to be one of soil and the moisture in the same. Inasmuch as we find great differences in the emanations from granite, clayslate, and chalk, it would seem extremely probable that we should find differences in the relative electrical conditions of different soils.

To determine whether earthquakes are increasing or decreasing, it is not only necessary to turn over the pages of many histories, but also to consult the geologist. Jules Verne might perhaps have dipped deeper into time than a geologist or physicist, and drawn pictures of the reactionary effect which might accompany the collision of one world with another, bombardments of great meteorites, a click that announced the birth of our moon, the sudden yieldings of a primitive crust covering an ocean of molten rock, and of many other things that float through the brains of those who entertain us with the results of their imaginations. The greater number of earthquakes, and certainly all that are large, originate from the formation or extension of faults. These operations have been most marked when secular movement amongst rock masses is in progress, as, for example, during the growth of mountains. Should this be in operation near large bodies of waters, volcanoes and earthquakes are found in the same region. If, therefore, we wish to know when earthquake frequency and intensity was at a maximum, we turn to those periods in geological history when mountain ranges were built, when volcanic activity was pronounced, and when great faults were made. The first of these periods would be coincident with the creation of the Urals, the Grampians and other ancient mountain ranges. This took place in Palaeozoic times. Another period of mountain formation was in early Tertiary times, when the Himalayas and the Alps were slowly, but intermittently brought into existence. In both these periods volcanic activity was pronounced and beds of coal were formed. When the crust of the earth was crumbling, mountains grew spasmodically, faults gave rise to earthquakes, volcanic forces found their vents, and conditions existed which gave rise to the accumulation of materials to form coal.

In quite recent times, many large faults have been created at the time of earthquakes. In 1891 the Mino-Owori fault was created in Central Japan, 10,000 people lost their lives, and 128,000 buildings were destroyed. On April 18, 1906, San Francisco and other towns were ruined by movements along a fault which can be traced for a distance of 200 miles. One estimate suggests that it may be 400

miles in length. The largest fault which has been created in extremely recent geological times seems to be the Great Rift Valley of Central Africa. We are told that it commences in the south near Lake Nyassa, passes northward through Tanganyika, the great lakes of Central Africa, branches north-eastward towards Lake Rudolph, up the Red Sea, through Akaba to the Jordan Valley, a distance of 4000 miles. In certain districts it shows itself as a strip of country let down between two parallel fractures. It has been compared to the cracks which can be seen in the moon. If we accept this as a reality, we have only to imagine this Great Rift fault to be extended as regards its length and breadth, and we have a trough in many respects similar to that which holds, not 30 lakes, but the waters of the Atlantic. If we look at the Atlantic, either as shown on a Mercator's chart or on a globe, we notice the complimentary resemblances between the contours of the old world and the new. Then, if we draw a line down the submerged backbone of this ocean, we see that this is the reflection of the European and North African western coast line. Next, if these old world contours are pushed westwards towards this median line, while the contours of the two Americas are pushed eastwards, we find that one approximately fits in with the other. The fit becomes more marked if we bring together the submerged edges of Continental shelves or lines representing the general direction of the opposing coast lines. Another point not to be overlooked, is that the rock formations on the west side of the Atlantic are very similar to those in the same latitude on the eastern side. It is as if we had a street with the shops on one side of it exactly similar to those on the other side. In Northern Spitzbergen and again in Greenland, we find a large development of crystalline and palaeozoic rocks, and these continue southwards through Labrador, Newfoundland, Maine and southwards through the Alleghanies. They again appear in Brazil as far south as Monte Video. On the eastern frontier of the Atlantic from Scandinavia through Scotland and Ireland, Wales, Western France and Western Africa as far as Cape Town, we see a replica of the two Americas. The Atlantic is a canal, the opposing banks of which are symmetrical in form and geological material. An idea, but one which is not very probable, which this suggests is that at some very early period in the world's history two Rift Valleys, one parallel to the eastern submerged backbone of the Atlantic, and the other parallel to its western frontier, were formed. Separation subsequently took place along these faults, and these under the influence of surface and underground activities, have continually increased. If, then, the Atlantic had an origin due to Rift Valley formation, rather than to folding or contraction, then the greatest earthquake in the history of the world may have taken place when east became east and west became west, and our world was cracked from pole to pole.

Just as the frequency of earthquakes has fluctuated during geological time, similar fluctuations have taken place during historical

time. In Central Japan earthquake frequency had a maximum in the ninth century, and since that time century after century, violent shakings have become less and less. In January 1844, at Comrie, in Perthshire, twelve earthquakes were recorded. Now there may not be one per annum. At the present time, in consequence of the destruction of several large cities, the popular idea is that earthquakes are on the increase. As a matter of fact, the world as an earthquake-producing machine has a steady output. On the average about 60 very large disturbances are recorded, and the greater number of these, fortunately for humanity, have their origins beneath ocean-beds or in sparsely inhabited regions. In addition to these megaseismic efforts, it is estimated that about 30,000 small earthquakes take place per year, England's annual contribution to this number being about half a dozen. If we had records like these extending backwards through several ages, we might readily estimate the time when seismic activity would cease. When this ceases, rock folding will also cease, and the degrading processes resulting in surface denudation will be unopposed. Bit by bit land areas will be reduced to sea level, and the habitable surfaces, as we now see them, will be no more.

An interesting observation bearing upon megaseismic frequency is found in the analyses of registers relating to the North Pacific. On the west side of that ocean seismic frequency is greatest in the summer, while on the east side it is greatest in the winter. An explanation for this is sought for in the seasonal alteration in the flow of ocean currents, the oscillations of sea level and changes in the direction of barometric gradients, which phenomena are inter-related. In summer off the coast of Japan, the Black Stream runs perhaps 500 miles farther north than it does in winter, while Dr. Omori points out that although barometric pressure may on the Japan side of the Pacific be low in summer, this decrease in load is more than compensated for by the increased height of ocean level: the inference is that the pressure on the ocean bed is greater in summer than in winter, and this is the time of the greatest seismic frequency.

Another factor bearing upon earthquake frequency may perhaps be found in the change in position of the earth's pole. A chart showing the path of the earth's north pole indicates that its movements are by no means always uniform. Although at times these may be nearly circular, it also shows sharp changes in its direction of its motion. It has even been retrograde. If on a chart showing these pole displacements we mark the time positions of world-shaking earthquakes, it is seen that these are grouped round the sharper bends of the pole-path. World-shaking earthquakes have in fact been most numerous when the pole path has deviated farthest from its mean position. The observations embrace a period of 13 years during which 750 large earthquakes were recorded. Although these earthquakes represent large mass displacements, it is not supposed that they would be sufficient to produce the observed pole movement. The pole movement,

however, may have given relief to seismic strain, or both effects may arise from some common cause.

Mass displacements accompanying a megaseismic effort must, however, tend to produce some pole displacement, and thus set up strains. From time to time these should find relief in the weaker portions of the earth's crust. Large earthquakes should therefore occur in pairs, triplets or in groups, after which we should expect a period of quiescence. This idea is due to the Rev. H. V. Gill, S.J. I find that the British Association registers lend considerable support to the hypothesis. The author of the idea, however, goes a step farther, and points out that if all matter within our globe or that which constitutes its crust was equally free to move, the secondary displacement should, with regard to the earth's axis of rotation, be symmetrically located in regard to the position of the primary disturbance. Out of 126 large earthquakes recorded between 1899 and 1905, I find that 20 of these appear as 10 pairs, the members of each pair being in symmetrically located districts. This may, or may not have been a matter of chance. The observation that a marked relief of seismic strain in one part of the world has frequently been followed by a smaller relief in some distant region, also suggests the idea that earthquake begets earthquake. In my own mind the relationship of earthquake to earthquake has been fairly well demonstrated, but to place the matter beyond the borderland of doubt large earthquakes must be compared in regard to space and time with their kind, with small earthquakes and with volcanic eruptions. All the volcanic eruptions of the West Indies have closely followed on the heels of great earthquakes which have originated, not in the West Indies, but on the neighbouring coasts of Central and South America. One general inference is that the faultings and freckles on the face of our world should have a distribution as symmetrically disposed as wrinkles are on the face of an elderly person.

Already when speaking about the length of faults which have been created at the time of large earthquakes, we have indicated at least one dimension of the earth block which has been disturbed. For instance, the earth block which was disturbed at the time of the San Francisco earthquake may have had a length of 400 miles, its breadth might be determined by the width of the country which had been broken up by branching and parallel faults. Harboe suggests that in a meizoseismic area hidden faults may be assumed to exist along lines drawn half way between pairs of groups of places which have been struck at about the same time. R. D. Oldham attributes the Assam earthquake of 1897 to the sudden shifting of 10,000 square miles of territory over a thrust plain. The molar displacement determined by the method suggested by Harboe would be that 50,000 square miles had been disturbed. The fact that so many earthquakes shake the whole world, or will agitate an ocean like the Pacific for many hours, indicates that the initial impulse must have been delivered over a large area, or that sudden alterations have taken place in the contour

of ocean beds. With regard to the magnitude of the latter changes we have learnt much from cable engineers, who have given us many instances where cables lying in parallel lines, ten or fifteen miles apart, have been simultaneously interrupted, and ocean depths over considerable areas have been increased. The depth to which these large faults extend is a matter of inference. We may well imagine them as passing through the whole thickness of the earth's crust and the displaced block falling to give up its energy to a nucleus which we know transmits undulatory movements all over our globe with uniform velocity. If we take this crust to be 30 miles in thickness, then with Harboe's area for the superficial disturbance, the block which was disturbed at the time of the Assam earthquake would be represented by $1\frac{1}{2}$ million cubic miles.

Following the initial impulse of a large earthquake it frequently happens a few minutes later that a second severe movement is felt. In Japan this is popularly spoken of as the *Yuri Kaishi* or the return shaking. This may be a second yielding within the disturbed district, but from its resemblance to the main shock it suggests an echo-like reflection. If we drop a bullet into a large tub of water, waves travel outwards to the sides of the tub, where they are reflected, and converge at the centre from which they set out. With the earthquake waves, the reflecting surface may be represented by the roots of mountain ranges. If these are at varying distances from the origin, the reflected waves would give rise to complications at the focus. The transmitting medium for these waves I take to be the more or less homogeneous material which lies beneath the heterogeneous crust of our world. This transmits large waves with a constant velocity. In the case of the Californian earthquake, which originated on fault lines on the western side of that country, I should imagine the reflecting surface to be the Sierras, 200 miles distant. The wave group would travel to these mountains and back in about four minutes, and this is approximately the time interval between the two first large wave groups in seismograms I have of that disturbance. After the first echo or echoes, an earthquake usually dies out as a series of surgings which frequently have a striking similarity to each other. One explanation of these rhythmical recurring groups is that they simply represent times when the movement of the ground has synchronised with the natural period of the recording instrument. Although the terminal vibrations seen on a seismogram may be attributed to this cause, it does not exclude the idea that rhythmical beats at an origin may result in rhythmical responses at a distance.

Side issues of seismology are quite as instructive as the information we derive from the records of earthquakes. We have already referred to light effects which accompany large earthquakes. This, as we have seen, led up to investigations connected with micro-organisms. A long series of experiments which commenced in Japan and were continued in the Isle of Wight, involved a series of investigations bearing upon the transpiration of plants. The fundamental object of these experi-

ments was to determine whether valleys always retained the same form. Did they open and shut? To answer the question I set up on the two sides of a valley horizontal pendulums identical with those which are used to record teleseismic motion. These instruments which are by photographic means self recording, are exceedingly sensitive to small changes in level. What I found was that on fine days the booms of these instruments moved in opposite directions, each away from the bed of the valley. At night the motions were reversed, and the booms moved towards each other, that is towards the bottom of the valley. Several instruments were employed and the records were confirmed by the movements of the bubbles of sensitive levels. During the day the records indicated that the sides of the valley opened and at night they closed. The two valleys I worked upon behaved like ordinary flowers, they opened when the sun was shining and closed at night. The best explanation I can offer is that the phenomenon is largely dependent upon the transpiration of plants. This is marked during the day, but not at night. On a bright day a sunflower or a cabbage may discharge 2 lb. of aqueous vapour. A square yard of grass will give off 10 or 12 lb. The result of this is that during the day underground drainage has not received its full supply of water to load the bottom of the valley. At night time when plants' transpiration is reduced, subsurface drainage is increased, and the load at the bottom of the valley is also increased. Therefore, at night the bottom of a valley, in consequence of its increased water load, is depressed, and this is accompanied by a closing of its sides. During the day the load runs off, and the valley opens. This may also explain why soak wells in valleys and streams carry less water during the day than they do at night, and at the same time it suggests that the side of a valley is a bad place for an observatory. Every day as the world turns before the sun, lamp-posts and tall structures salute the same, whilst many valleys open. At night time these movements are reversed.

One phenomenon which accompanies all large earthquakes, which, however, has never yet received the attention it deserves, is the influence which great disasters have exercised upon the emotions. Immediately after the Kingston earthquake, we read of the dazed and almost insane condition of the people. Many were affected with an outburst of religious ecstasy, thinking the last day had come. The negro population camped on the racecourse, and spent their time in singing hymns. Somewhat similar scenes took place in Chili: men and women ran hither and thither, mad with terror and devoid of reason. Amid shrieks and sobs, and the wailing of a multitude an *Ora pro Nobis* or a *Pater Noster* might now and then be heard. In early civilisations underground thunderings have so far excited the imagination that subterranean monsters or personages have been conjured into existence, and these in many instances have played a part in primitive religions. At the time of an earthquake in Japan, the

children are told that the shaking is due to the movement of a fish which is buried beneath their country, and in Japan we find references to this fish in the pictorial art, glyptic art, literature, and everyday conversation, all of which would be unintelligible, if we did not know the story of the earthquake fish. In other countries, the subterranean creature will be a pig, a tortoise, an elephant, or some other animal. The most interesting myths, however, relate to underground personages. The 45 Grecian Titans, who were of gigantic stature and of proportionate strength, were confined in the bowels of the earth. According to the poets the flames of Etna proceeded from the breath of Enceladus, and when he turned his weary side the whole island of Sicily was shaken to its foundations. Neptune was not only a god of the oceans, rivers and fountains, but with a blow of his trident he could create earthquakes at pleasure. The worship of Neptune was established in almost every part of the Grecian world. The Livians in particular venerated him, and looked upon him as the first and greatest of the gods. The Palici were born in the bowels of the earth and were worshipped with great ceremonies by the Sicilians. In a superstitious age the altars of the Palici were stained with the blood of human sacrifices. In Roman mythology, two very familiar deities are Pluto and Vulcan. These and a host of other deities, the outcome of imagination, excited by displays of seismic and volcanic activity, we meet with every day in picture galleries, in museums, in literature, and in our daily papers. The fact that we are enjoined not to make any graven image of that which is in the earth beneath suggests that in the time of Moses a certain form of worship called for some correction. Over and above adding a clause to the decalogue large earthquakes have in very many ways affected religions. After the earthquake which shook England on April 6, 1580, the then Archbishop of Canterbury drew up a form of prayer which was approved by the Privy Council, and ordered by them to be read in all dioceses in the Kingdom. In the world there are many instances of religious services being held on the anniversary of an earthquake, it being regarded as an exhibition of God's vengeance upon a wicked people. The belief that earthquakes are signs or warnings owes its origin in part to prophecies in the Bible, where for example, we read that "there shall be famines and pestilences and earthquakes" as portending future calamities. Earthquakes have led to the abolition of oppressive taxation, the abolition of masquerades, the closing of theatres, and even to the alteration in fashions. A New England paper, of 1727, tells us that "a considerable town in this province has been so far awakened by the awful providence in the earthquake that the women have generally laid aside their hooped petticoats."

[J.M.]

WEEKLY EVENING MEETING,

Friday, March 27, 1908.

THE RIGHT HON. LORD RAYLEIGH, O.M. P.C. M.A. D.C.L.
LL.D. Sc.D. Pres.R.S., in the Chair.

THE HON. ROBERT JOHN STRUTT, M.A. F.R.S.

Radio-active Changes in the Earth.

You will have already heard during the present Lecture Session from Professor Rutherford, of the recent developments of the new science of radio-activity. I venture to hope, however, that there may be room to say something of aspects of the subject not treated by him. I wish particularly to refer to manifestations of radio-activity which are observed not in artificially prepared materials like radium, but in the rocks and minerals of the earth's crust, as we find them in nature. Let us consider in the first place the most conspicuous cases of this kind. The source from which radium is obtained is the mineral pitch-blende. This mineral occurs in veins, like the majority of the useful metals; I may refer particularly to the mineral veins of Cornwall, so long famous as a source of tin. These veins are of the nature of cracks, running through the granite and through the slate which adjoins it. The cracks have been filled up by the various metallic ores which have been introduced by precipitation or sublimation, the exact nature of the process being somewhat obscure.

I will now show you an experiment, due to Sir W. Crookes, which illustrates the radio-activity of pitch-blende in a very beautiful manner. A flat polished slab of pitch-blende intergrown with a variety of other material which is not radio-active was laid face to face with a photographic plate which was developed, after the lapse of about a week of contact. The radium and other radio-active substances contained in the pitch-blende have acted photographically upon the plate, while, of course, those portions of the material which are not radio-active have exerted no such action. Thus pitch-blende has, as it were, taken its own portrait, which I now show you on the screen.

Pitch-blende, the principal radium ore, contains, as you know, only an infinitesimal percentage of radium, the bulk of the substance being made up of oxide of uranium. Uranium is commonly spoken of as a rare metal; but terms of this kind are comparative only, and in contrast with radium, which is more than a million times scarcer

it seems common enough. Now I wish to speak for a little about this association of uranium and radium in pitch-blende. Is it accidental, or has it some special significance? I hope to be able to convince you that it has.

In the early days of radium, it was common to hear the difficulty emphasised that while there was no reason for doubting that the radium which was found in the earth had been there as long as other metals, a substance that was continually giving out energy in this way was obviously defying the greatest physical generalisation of the nineteenth century—the law of the conservation of energy. We cannot, however, afford to sacrifice this law so easily, and a ready mode of escape offers itself, if we suppose that a continual waste of radium is occurring. In that case, it becomes necessary to suppose also that the supply is in some way replenished, for otherwise all the radium would have wasted long ago. From what material are the fresh supplies of radium derived? They must be derived from some other substance contained in the mineral where the radium is found, and there is now reason to feel sure that uranium is the substance in question.

We have convincing proof of this, in the fact that the amount of radium found in the mineral is always in direct proportion to the quantity of uranium which it contains. I should, perhaps, say, to avoid misconception, that there is good reason for believing that several transitional stages exist through which uranium passes on its road to become radium. It is not necessary, however, to take into account the existence of these intermediate products in order to form a clear idea of the process by which the supply of radium is kept up. Uranium changes spontaneously though very slowly into radium, and the amount of radium produced per annum, for example, will be proportionate to the amount of uranium present. On the other hand, a certain fraction of the total amount of radium present decays per annum, and the balance of this account of profit and loss will represent the amount of radium found in the mineral at any time that we examine it. There will be no difficulty in seeing that on this theory the amount of the radium in the mineral should be proportionate to the amount of uranium; and experiment fully confirms the theory by showing that such is in fact the case. We have here a clear and distinct case of the transmutation of metals, so long unsuccessfully searched for.

Let us now come back to the pitch-blende.

What was the source of metalliferous ores found in mineral veins is a very much vexed question, and no solution of it which has yet been proposed can be said to be altogether free from difficulty. One of the most plausible theories, however, supposes that the metals have been derived from the rocks by which the veins are traversed. We are not here concerned with metalliferous ores in general, but only with those which carry radio-active material. In deciding

whether the granite of Cornwall can be supposed to furnish the uranium of pitch-blende, it is, of course, fundamental to know whether any uranium is present in the rock. It should be said, by way of preface, that the quantity must, at best, be very small, and certainly too small for detection by the methods of chemical analysis as ordinarily applied. We have seen that uranium in nature is invariably accompanied by a proportionate quantity of radium, and as it is in practice much easier to detect minute quantities of radium than to detect the corresponding quantities of uranium, it is best to look for the former only, and to be content to infer the presence of the latter.

I have made a large number of experiments to find out how much radium there may be, not only in Cornish granite, but in a large variety of other rocks. In every case the presence of radium has been established, though only to the extent of about one-millionth part of what is found in pitch-blende, and even that, it will be remembered, is not much. If we take into account the very large bulk of the granite and the very small bulk of the pitch-blende veins running through it, there is no difficulty in admitting that the granite was capable of supplying the radio-active material of the pitch-blende.

Granite, of course, consists of a variety of different minerals, which give it its mottled appearance. These minerals, there is no reason to doubt, have been formed in the successive stages of crystallisation of an originally molten mass. There is a mineral called zircon, of which the jacinths sometimes set by jewellers are a variety, which is present in very minute crystals in granite. These minute crystals of zircon have a very characteristic geometrical shape: a square prism terminated at each end by a pyramid, as you see in the photograph. The fact that they have this perfect shape is a proof that they have been perfectly free to assume their natural form, and have not been hampered for want of space by other minerals surrounding them. The inference is plain that zircon has been one of the first minerals to crystallise in the consolidation of granite.

I have found that this zircon is very much richer in radium than the granite generally, though, on the other hand, it is poor compared with pitch-blende. It seems clear that the minerals which crystallise first take an unfair share of the radio-active elements, leaving the rest of the magma impoverished.

In the light of this observation, Professor Joly, of Dublin, has been enabled to explain a curious appearance which is seen when a section of the granite thin enough to be transparent is examined under the microscope. You see upon the screen one of Professor Joly's photographs of this appearance. This is a minute crystal of zircon, which is imbedded in a large crystal of mica. You will observe that the material surrounding the zircon for a definite distance outwards has become darkened in colour. The altered region

round the speck of zircon is practically circular, and is reminiscent of a spot of grease on cloth.

Professor Joly has pointed out that this alteration in the surrounding materials must be due to the radio-activity of the zircon. That radio-active materials are capable of producing such colorations has been known from the early days of radium. You see, for instance, projected on the screen, the image of a glass bottle, in which a radium preparation has been kept. Though originally of colourless glass, it has been stained a deep purple by long continued action of radium.

It may, perhaps, be thought that this idea, though plausible, is no more than a guess. It is however, much more than that. We know, from the investigations of Professor Bragg and Mr. Kleeman, that the α particles of radium, which constitute the most important feature of radio-active emission, are only able to penetrate a limited and definite distance into solid materials. They then lose their characteristic properties, if, indeed, they are not altogether stopped. This distance has been measured experimentally, and Professor Joly has shown that the distance is just the same as the distance to which the alteration round the zircon crystals extends. Thus we have full quantitative confirmation of the theory which attributes it to radio-activity.

I will now pass from the discussion of a very minute phenomenon to the discussion of a large scale one. It will be familiar to many of you that, in the opinion of some, at least there is reason for changing the views which have been held for two generations concerning the earth's internal heat. We know that there is, at any rate, some radium in the earth, and that radium gives out heat. Thus it cannot be disputed that some part of the earth's internal heat must be due to this cause; the only question which remains is whether this part is large or small; whether in fact the earth's internal heat is chiefly to be accounted for as a small remnant of the much greater internal heat which it once possessed, or whether there is enough radio-active material in the earth to supply most of the annual loss by conduction through the crust and radiation into space.

As I mentioned before, I have made a large number of determinations of the quantity of radium in the rocks of which the superficial portions of the earth are constituted. These are found to be so rich in radium that the difficulty is not so much to account for the internal heat of the earth, as determined by underground observations of temperature, but rather to understand why it is not much hotter. I have suggested, as an explanation, that this general distribution of radio-active material, which pervades the outer parts of the earth, is in reality superficial, extending only to some moderate number of miles in depth, though no doubt much deeper than the deepest mines. I am not wholly satisfied, however, of the sufficiency of this explanation. Radium, and the series of products of which it

is one, are not the only radio-active materials in the earth: there is another series, of which thorium is a member; and there is good reason to suppose that thorium is present in rocks in such quantity as to add appreciably to the evolution of heat. Taking this into account, we should probably find, if we had exact data for calculation, that the thickness of rock containing radio-active material was so small that the material of the interior would somewhere have exuded, in the course of the violent dislocations and earth movements which geology reveals to us. No material, however, appears anywhere at the earth's surface which can plausibly be regarded as representative of the unknown interior, if the suggested hypothesis is accepted. It cannot be denied that the subject is at present obscure. Possibly an explanation may be found by supposing that the activity of uranium may be arrested at high temperatures. We have at present no adequate experimental evidence on the subject. It is known that there is very little effect of this kind on radium. If, however, the activity of uranium were arrested at a high temperature, the supply of radium and all the other members of the series would fall off, and thus the aggregate heat production of the whole series might be greatly diminished.

I shall now pass to another branch of the subject. The investigations of Sir William Ramsay and Mr. Soddy have proved that there is continuous evolution of helium from the radium emanation. We have good reasons, into which, however, I do not propose to enter, for considering that the same is true of radio-active changes in general, at all events those in which there is an emission of radiation. Helium is probably evolved at each stage of the transformation of uranium, and at each stage of the transformation of thorium; and it results that the natural minerals and ores in which these elements are found, contain a store of helium, which has accumulated in them, and remains locked up in their pores.

As already mentioned, I have succeeded in determining the presence of radium in granite. Thus it becomes natural to enquire whether the corresponding amount of helium is to be found there too. Nothing of the kind had ever come under observation before, and it was, therefore, with some interest that I made the experiment. You see before you a vacuum tube of helium prepared from ordinary granite. The characteristic yellow glow will satisfy anyone acquainted with the appearance of a helium discharge of the presence of the gas.

The facility with which helium was detected in granite suggested further experimental problems. The undoubtedly radio-active elements are at present confined to uranium and thorium, and their respective families of descendants. Evidence has been produced, by myself among others, which suggests that lead and some other elements possess a feeble radio-activity of their own. But this evidence is somewhat equivocal. It seemed highly desirable to attack the question in a new way, and the idea suggested itself of looking

for helium in the naturally occurring ores of all the elements, common and rare. This had indeed been done, to some extent, from quite a different point of view, by Sir William Ramsay and his coadjutors, in their first investigations on helium; but their observations were directed to finding a practical source of the gas, and were not carried out with anything approaching the minuteness required for the present purpose.

The upshot has been to prove the presence of helium in almost every mineral examined, and even in such unpromising materials as rock crystal, or common quartz sand. The quantity found in the various cases has varied very widely. In fact, minerals may be found having any helium content, from thorianite, which contains 10 cubic centimetres per gram, down to rock crystal, which contains about a ten-millionth part of that quantity.

I have here a small tube of helium obtained from clear, colourless rock crystal, and you will have no difficulty in seeing the characteristic yellow glow as before.

Are we to regard the helium in common minerals as due to a feeble radio-activity of the common elements? No doubt such an hypothesis is tempting, but it must be rejected. Radium is present everywhere in traces, and these traces are in general sufficient to account for the minute quantities of helium. This is illustrated in the table below, which gives in round numbers the actual amount of helium extracted from various minerals by heat, and the amount of helium reckoned relatively to the radium.

		Helium present.		Helium ratio,	
		c.mm. per kil.		i.e. ratio of helium	
				to radium.	
				Arbitrary scale.	
Normal	{ Samarskite	1,500,000	14	
	{ Hämatite	700	9	
	{ Galena	2	17	
	{ Quartz	2	10	
Abnormal	{ Beryl	33,000	954	

There is reason to think, as already mentioned, that the presence of thorium would constitute another source of helium. But it is believed that this complication does not produce any appreciable effect in these cases. You will see that minerals like quartz, though they contain actually only an infinitesimal quantity of either substance, still show about the same proportion of helium to radium as the minerals which are rich in both. We may conclude that helium is connected with radium in the poor minerals as in the rich ones.

I have, however, encountered an interesting exception to this rule in the mineral beryl. Beryl is, in all essentials, the same as emerald: the latter name is kept for stones which are of a clear

deep green colour; but scientifically the distinction is of no importance. Some beryls contain enormously more helium than can be accounted for by the small traces of radium in them. Nor do they contain any appreciable quantity of other radio-active material. What view, then, can we take of the presence of helium in this mineral? It is, to me at least, difficult to believe that the gas can have been introduced from without. If not, can it have been generated from radium formerly existing in the beryl, but now exhausted? This, too, seems unlikely, for it would imply that beryls are older than other minerals, and there is no plausibility in such a theory from the geological standpoint. My own opinion is that, in all probability, an element hitherto unknown exists in the mineral, from which the helium is generated. It may be objected that, in that case, the mineral ought to be radio-active. If, however, the radiation were emitted with less than the critical velocity, we should not be able to detect it, and nothing is known to make such an hypothesis improbable.

In conclusion, I shall be well content if I have convinced you that there is still something to be learnt from careful examination of the most commonplace materials. If there is nothing new under the sun, there are, at least, unsuspected things going on inside the earth, where the sun cannot penetrate.

[R. J. S.]



WEEKLY EVENING MEETING,

Friday, April 3, 1908.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L.
F.R.S., President, in the Chair.

THE RIGHT HON. LORD MONTAGU OF BEAULIEU, D.L. V.D. J.P. etc.

VICE-PRESIDENT, ROYAL AUTOMOBILE CLUB; EDITOR OF THE 'CAR.'

The Modern Motor Car and its Effects.

THE evolution of the modern motor car has been proceeding for thirteen years past, and although much progress has been made, mechanically speaking, in the machine of to-day, excellent as it is, it is far from being perfect. Another decade will probably see still more progress made in simplification, efficiency, and increased cheapness in operation.

Historically speaking, it is not altogether correct to say that the motor car only commenced its career in 1895, for from 1820 to 1835, steam carriages ran every day on the highways of this country, carrying passengers, goods, and mails, and if it had not been for the opposition of the horse-breeders and owners of that time, and the commencement of the railway era in 1836-40, both roads and road mechanical locomotion would have been in a few years a long way ahead of the rest of the world to an extent which would probably have retarded the development of railed as against free-wheeled locomotion for some time. But between 1840 and 1895, a period of fifty-five years, no attempt was made to use mechanical power for any kind of road traction, except in connection with what are called traction engines and steam rollers. This in itself is a curious fact when one considers the great progress that was made during those years in adapting the power of steam and explosive engines in thousands of useful ways for the use of mankind. Nor was the adaptation of the internal combustion engine in connection with heavy self-propelled vehicles coincident with any particular new invention, for the so-called Otto "cycle" and Gottlieb Daimler, the inventor of the modern vertical explosive engine, had been before the world many years anterior to 1895, and thousands of gas engines, working on the same principle as in the modern petrol engine, were operated all over the country quite successfully. The fact was that ill-advised and restrictive legislation against mechanical locomotion on the roads was the chief bar to further progress, and the

prejudice existing in England against any vehicle not drawn by animal power was a drawback which took many years to surmount from a legislative point of view. Even at the present moment it has not altogether disappeared. Anything which interferes with the exclusive use of horses on the highway has for a long time been looked upon in this country as a development to be strenuously opposed. It was this feeling which led to the widespread hatred of, and opposition to, the tricycle and bicycle, and which prevented Parliament from modifying earlier the law that the speed of mechanically-propelled vehicles should not exceed four miles an hour, and the ridiculous provision that such vehicles must be preceded by a man with a red flag, a restriction which will make England in future years a laughing stock in the eyes of scientific historians.

I can testify myself that less than ten years ago many of my friends and relations looked upon me as a nasty vulgar person who had lost caste beyond all hope, because I went about in motor cars, and it is only eight years ago since my car was stopped by the police on entering the precincts of the House of Commons, although I was then a member, and had the right, by sessional order, to demand "free egress and ingress." An appeal to the Speaker eventually, however, put the matter right. The motor car of to-day is, therefore, very modern indeed, for it has only just survived its infantile troubles, and has not yet even reached that stage in which its existence is accepted without question. The very fact that one often hears the phrase—the motor car has come to stay—makes this apparent. For no one has ever heard the remark that the horse has come to stay, or that any other accepted fact has come to stay.

The effects of the motor car on our every-day life are beginning to be important. As to the direct trade, of which the motor car is the cause, I estimate that a sum of over twelve millions sterling is already invested in this country in motor car plant and machinery, without taking into consideration the accessory trades which are also important financially. Moreover, the output of the motor-car industry in this country will be worth not less than six millions sterling during the present year. In other directions the decrease in the number of horse vehicles, and the way in which the new kind of locomotion is changing the course of existing trades, such as the carriage building industry, give food for reflection. Coach builders are now building more bodies for motor cars than horse vehicles—in itself a sign of the changing current of trade.

The decrease in the number of horse vehicles is shown as under :—

On March 31, 1896, there were	549,630	{ Horse drawn carriages on which duty was paid.
On December 31, 1906, there were	532,452	
<hr/>		
17,179 decrease.		

Again, our system of road-making and the details of road expenditure have, during the last few years, been brought greatly into prominence, mainly owing to the increasing use of motor cars, while hotels, the value of land and houses, the dress of ladies and gentlemen, the creation of a new class of skilled drivers, the conditions of insurance against accident, the earnings of railway companies, and incidentally the speeches of railway chairmen at half-yearly meetings, and the attention and advice of physicians have all in their turn been affected by the coming of this new means of locomotion. Nor is it to be wondered at that it is so, for since the earliest dawn of civilisation, animals have been the only means by which mankind could progress on land faster than by the use of their own legs. It is difficult, moreover, to foresee the magnitude of the changes which the motor car will eventually bring about in social and political conditions. But it is quite safe to assume that cheaper, quicker, and more comfortable locomotion on roads will tend to decentralise towns, to repopulate the country, and to introduce the British public to a knowledge of their own country. These processes are already beginning.

As figures are somewhat dull and uninteresting when read out by a lecturer, I have prepared some slides which will illustrate to the eye of the spectator the progress which automobilism has made during the last ten years.

INCREASE OF MOTOR CARS AND MOTOR CYCLES.
(3 yearly periods.)

1897.	1900.	1903.	1906.
650.	9,500.	48,000.	125,000.
Estimated Amount of Capital invested in the Motor Industry £12,000,000.			
Estimated Number of Persons Employed in the Motor Industry 300,000.			
Estimated Number of Miles run in a year 3,150,000,000.			
Estimated Value of Motor Cars and Cycles used in Great Britain to-day £29,320,000.			

As regards the mechanical side of motoring, it is interesting to record that the need for specially hardened steels capable of bearing great stresses, has induced British steel manufacturers to make experiments, which have mainly been successful in the direction of producing such materials. In the early days of motoring, gears required to be renewed constantly owing to the case hardening on the steel teeth wearing off and exposing the softer metal below. Nowadays all speed rings, and many other wearing parts are made of the hardest possible material, and there are cases in which even after the heavy strains caused by motor-bus work, these gear wheels have shown practically no wear after running thousands of miles.

Pressed steel frames are now everywhere used instead of the weak metal or wooden frames of earlier days, while improvements in carburation, or in other words improvements in the volatilisation of petrol spirit by the passage of air through it, are being made every day. Apparatus for speed changing has been greatly improved, while the addition of a small dynamo, generally called a magneto, actuated from the secondary shaft, is common enough in most cars nowadays, enabling the storage battery to a great extent to be dispensed with. Toughened axles have added their share to the increased safety of motorists, while last, but not least, is the improvement made in pneumatic tyres, the prices of which are becoming cheaper, while the durability is becoming greater with every succeeding year.

Perhaps the most remarkable improvement has been in the direction of the production of more and more horse-power for the same weight. Eleven years ago a 6 h.p. Daimler car weighed about 28 cwt., and was fitted with an engine producing little more than 6 h.p. To-day there are four-cylinder cars, which, while weighing less than 25 cwt. have engines capable of developing at least 50 h.p., while there are many which weigh, with the carriage work all complete, about 22 cwt., which have engines ranging from 15 h.p. to 30 h.p. This has naturally made for greater economy in upkeep. I here give a table showing the fall in the price of horse-power :—

PRICES PER HORSE-POWER.

	Nov. 1905 Price per h.p.			Nov. 1906 Price per h.p.			Nov. 1907 Price per h.p.		
	£	s.	d.	£	s.	d.	£	s.	d.
Daimler	25	6	8	21	11	8	15	6	11
De Dietrich	33	0	10	26	5	8	25	0	0
De Dion Bouton	30	2	8	29	6	9	25	19	7
Maudslay	30	0	0	25	8	10	22	10	0
Napier	28	5	7	19	5	8	17	2	4
Rolls-Royce	43	4	3	26	1	0	24	12	6
Thornycroft	28	18	0	24	1	3	19	7	1

To deal with the effects of the motor car on road construction and maintenance, it is interesting to reflect that up to twelve years ago there were many main roads in this country which were almost grass-grown in the summer, while in other places they were often in such a bad condition that it was almost impossible to drive a vehicle at any pace along them. The road, for instance, between Inverness and Perth, ever since the construction of the Highland Railway between these two points, had fallen into such a state of disuse that when I first crossed it, some ten years ago, there were long stretches

of roadway near the summit of the Grampians upon which heather, grass, and rushes were growing, and the appearance of the road clearly showed that very few, if any, vehicles used it. It may be taken for granted, now, that between the beginning of July and the end of October this road is used by thousands of motor cars, and even at other times of the year a considerable number of automobilists, who are bound North or South, cross this bleak region. To put the facts in another way, at least 90 per cent. of the road traffic to-day between Blair Athol and Inverness consists of motor-cars. To take another stretch of road nearer London, some of my audience, in these days of touring, doubtless know the road between Basingstoke and Winchester. Ten years ago, except for a very small amount of local traffic, there was hardly any use made of this fine main road between Stratton and Kemshott Hill, for the highway runs through a piece of country destitute of villages, and only boasting of a few scattered cottages. In 1898 grass had grown far up on the sides of the road, and the fine and wide coaching highway of our forefathers had become little better than a country lane. To-day all this is being altered, the road being vastly improved, and motor cars bound for the South and West of England pass along by day and night, imprinting on the surface the peculiar marks of the motorist, those tracks and holes which denote the passage of many motor cars following each other in the same track. Parenthetically I may remark that this habit of every car pursuing exactly the same track is responsible for most of the damage attributed to motor-car traffic, for only a small strip of roadway, not exceeding one twentieth, has thus to bear the whole wear and tear.

The main road mileage for England and Wales is to-day 27,556 miles, and it is safe to say there is not a mile which is not costing surveyors uneasiness owing to the increasing traffic of all kinds which is to-day using them. The cost of the upkeep on the main roads repaired by County Councils has risen from 55*l.* in 1896 to 73*l.* per mile in 1906. I give some figures which amply demonstrate the increase in main road expenditure which has to be faced by County Councils with regard to roads which they repair themselves.

MAIN ROADS REPAIRED BY COUNTY COUNCILS THEMSELVES.

Statement (compiled from the Annual Local Taxation Returns) showing for the years 1896 to 1906 the total amount expended (otherwise than out of loans) by County Councils in England and Wales on the maintenance, repair, and improvement of those main roads which were repaired by the County Councils themselves, the mileage of those roads, and the average amount so expended per mile :—

Year.			Gross Expenditure on Maintenance, Repair, and Improvement.	Mileage.	Average Amount of such Expenditure per Mile.
			£		£
1896-97	841,598	15,238	55
1898-99	875,992	15,140	58
1900-01	980,601	15,986	61
1902-03	1,153,289	16,580	70
1904-05	1,225,697	16,970	72
1905-06	1,272,646	17,468	73

In addition, Rural District Councils and Urban authorities repair another 10,088 miles of main road on which the cost has also risen.

Near towns, villages, and scattered cottages, the demand for the use of dustless material is becoming so insistent that surveyors and highway committees are being forced to study, seemingly for the first time since the railway era, 70 years ago, the problem of how to make good roads of durable and withal dustless material. It is only a few weeks ago, when talking to a district surveyor—whose system of road making is apparently more dustless and durable than any other system as yet tried—that he expressed his gratitude to motorists for having exposed the weakness of the antiquated system of road making which had existed for so many years, and regretted that road surveyors in general had to admit that up to now the problem of making a hard and dustless road had not been solved. There can be no doubt that roads are gradually reassuming the importance which they possessed in the pre-railway days. Telford and Macadam were enabled by State help and private enterprise to construct new roads upon their then new systems. The Holyhead road and the Great North road are the last two important arteries for traffic which were constructed in this country. Though made some seventy years ago, they serve—and will serve for centuries to come—as lasting monuments to those two great civil engineers. I can conceive no finer memorial of a man than the building of a fine road useful to mankind for all time. It is truly *are perennius*. It is a national disgrace, however, that since then no new highway has been constructed, although this country's population and wealth have increased enormously, and though the traffic on the roads is a hundredfold greater than at the time of Telford and Macadam. The motor car, in sober fact, has reminded the nation of the importance of its roadways, which it had forgotten since the use of the railway had become so general and unchallenged. Now, however, road questions will gain more and more every year in importance, new and wide exits from large cities, such as London, must be made, and motor roadways between centres of population will probably in a few years be constructed to relieve the congestion of the present highways, which will then be left for the

slower traffic, and for those who for various reasons will continue to use horse carriages instead of mechanically propelled vehicles. That there were two Bills before the Houses of Parliament at the beginning of the session to provide a western exit from London, in itself shows the trend of events; and one only need add that every civil engineer of importance, every student of traffic problems, every government official whose department is connected with the subject, in addition to at least three royal commissions, and one departmental committee, have reported in favour of a new national road policy, including the making of fresh exits from towns to relieve the congestion of population and the congestion of traffic alike. One of the most important questions of the day is the establishment of a central highway board to superintend the maintenance of main roads. Their management to-day is in the hands of thousands of local authorities, who are nearly all working on different systems of road making—entailing waste and inefficiency. If the motor car compels the re-organisation of our highway system on a national basis, it will on that ground alone be worthy of the gratitude of posterity.

I must now pass on to some other changes for which the modern motor car is responsible.

Its political and social effects are many and important. Politically, I am glad to say that neither of the great parties in the State can claim the general body of motorists as being particularly attached to it, though perhaps the present Chancellor of the Exchequer, looking upon motorists as being a rich class, and therefore his special prey, and believing that they will bear further taxation without meetings in Trafalgar Square or invasions of the House of Commons in motor omnibuses, may perhaps alienate the loyalty of Liberal motorists. The majority of motor cars, however, belong to people possessed of moderate means, and it would be just as fair to assert that a man was to be specially heavily taxed because he possessed a horse-carriage, as to presume him to be exceedingly wealthy because he is in possession of a motor car. Of course, a few millionaires possess some very expensive motor cars, but the majority of motor-car owners are neither richer nor poorer than the majority of horse-carriage owners.

There are seasons, however, in the political world when the motor car owner is extremely popular with the chiefs of all political parties, that is at election times. It is then that cabinet ministers write flattering personal notes to well-known motorists asking them to lend cars for the coming bye or general elections, and the most bitter of anti-motorists of both sexes, Conservative or Radical, may be seen penning gushing notes to their friends, asking them if they can obtain the use of motor cars to help her or his friend to victory at the polls. And the voter himself is becoming so convinced of the superiority of the motor car as a vehicle for election purposes that in many cases he will not only not walk to the poll half a mile away, but he refuses to go in the most luxurious horse-drawn sociable or

broughams, preferring to wait for the more up-to-date motor car. Immediately an election is over, however, we are again told that the motor car is a terror to the rural population, that it is frightening them off the roads, poisoning their houses with dust, and that it is destroying the otherwise happy and arcadian life of the cottager.

The motor car also enables a member for an agricultural constituency to go to two or three, or even four meetings a night in out of the way districts, and to speak at small hamlets, where, until rapid locomotion was possible, there was always a difficulty in paying a visit, except very occasionally during the stress of a general election. There are few agricultural constituencies nowadays in Great Britain which a member cannot thus cover in a day. In fact, the possession of a motor car by a country member is now almost a necessity. It is, perhaps, due to this fact more than to any other that one hears but comparatively little abuse by M.P.'s of the motor car in the House of Commons, except, perhaps, from representatives of far away Celtic districts in which there are few roads and in which the automobile is still looked upon as the special and latest invention of Satan. A few Socialist M.P.'s also occasionally endeavour to gain cheap popularity by attacking motorists on the assumption that they are always rich men, whereas the majority, as I have pointed out, belong to what are called the professional classes.

The social effects of automobilism are becoming more marked every year. As I have already pointed out, it is decentralising the towns and filling up the suburbs and the country. In Mayfair and Belgravia there have never been so many houses to let, while in the suburbs, situated on high ground to the north or south of London, houses are in great request. Residents at Wimbledon and Hampstead are now only a matter of some twenty minutes away from the central parts of London, and better air and absence of noise are preferred to the rumble, dust, and smells of central London. High rents in the West End, as in the East End, depend upon the number of people wanting to live in a certain locality, close to their work or their play. Now that people can live further afield and get to their work without undue loss of time, the pressure upon these central localities is not so great, and down therefore have come the rents. In course of time rateable values must of course drop also, a contingency which should make our L.C.C. pause before committing London to more expensive schemes in the future.

The motor bus is also a decentraliser, uncongesting—if I may use such a word—many of the overcrowded working-class areas. In consequence there is nothing like the same demand upon accommodation at the back of the Strand, in Soho, or in the regions north of Oxford Street, and on both sides of Tottenham Court Road as there used to be. In fact, the landlords of working-class houses are already deploring the fact that greater transportation facilities are depressing their rents. One may, I think, assert without fear of con-

tradition that cheaper and swifter locomotion is the best cure yet known for congested areas, and that if the money spent in erecting workmen's dwellings on expensive sites had been spent on widening the main arteries of traffic, it would probably have been more wisely spent and more calculated to bring about the objects which the generous donors of money had in mind. To-day the problem is not only one of cheaper locomotion, but of more adequate roadways. Motor buses could convey millions more every year much more quickly and cheaply were the main streets wider and the congestion of traffic therefore less. During the last year, 1905-6, for which we have statistics, motor buses carried 184 million passengers as against the 180 million carried by the vast network of the L.C.C. tramlines, figures which in themselves show how great is the use made of the new form of locomotion, which, be it remembered, has only been in existence for a period of three years. These figures also demonstrate how necessary to the ebb and flow of the working population these vehicles have become.

In London the mechanical carriage already outnumbers the horse-drawn in many streets. On the Embankment between 9.45 and 10.30 the preponderance of motor cars is very marked, as shown by the following table, the figures of which were quite recently taken by a friend of mine :—

TABLE OF TRAFFIC IN LONDON ON THE THAMES EMBANKMENT, SHOWING THE NUMBER OF VEHICLES OF EACH KIND BETWEEN 9.45 AND 10.30.

				Motor Cars	Private Carriages
Saturday, February 29		106	47
"	March 26	137	73
"	"	27	...	168	61
"	"	28	...	86	31
"	"	30	...	137	65
"	"	31	(9.45 to 10.30 and 10.30 to 12) ...	147	66
				80	50

These figures were specially taken to demonstrate that at the time of going to business the preponderance of motor cars is greater than at other times. They also completely explode the idea that motor vehicles are a luxury of the idle classes. It is the same in all busy towns at the hour of going to business.

EDINBURGH.

Times taken on ten days at the same time of day :—

Motor Cars	Private Carriages
69	31

LONDON.

The following table shows the number of vehicles of each kind met in walking or driving about in the streets of London on the days in question :—

1908	Private Vehicles		Public Vehicles	
	Motor Cars	Carriages	Taxicabs	Hansoms
Saturday, February 29	218	102	213	103
Sunday, March 1 ...	115	15	175	101
Saturday, March 28 ...	153	61	237	181
Sunday, March 29 ...	110	29	127	55

On country main roads the traffic is about half and half divided between mechanically and horse drawn. The proportion of motor cars to other traffic is, moreover, increasing every year.

While the motor car is lowering the value of town houses and their rents and rates, it is increasing the value of country houses by adding to their accessibility, especially where they are at some distance from stations. The great increase in the week-end habit to some extent may be attributed to the increase in the use of motor cars. Good railway services have existed for some time past in many directions, but the difficulty lay in getting from the station to the country house, possibly some six or eight miles away, and the fact that the best expresses, stopping only at a few important stations, were of no assistance to many dwellers in the intermediate country. The motor car is now altering these conditions, for at important stations on main lines every Friday and Saturday, will be seen motor cars waiting to take their owners and their guests not merely four or six miles to their homes, but often anything between ten and thirty miles, saving sometimes over an hour from door to door which used to be absorbed by changing into a slow train which had to stop at all intermediate stations. Increased train facilities are thus available by enabling the motorist to use the more important centres where expresses are frequent and fast. Many people are consequently laying out money on purchasing sites or building on long leases in localities where, until now, they would never have dreamt of settling. Country life is also becoming less dull, for the neighbourhood—using that word in its proper sense—instead of being confined to a six-mile radius is now widened to a sixty-mile circle. Some recluses may think this a drawback, but on the whole it makes for more friendships and greater sociability. It is no uncommon thing now to ask friends to come over to lunch from thirty or forty miles away—a matter of one

and a half hour or two hours—and visits to the county town, or to the seaside or mountains in the neighbourhood, are now possible.

Easy communication also encourages good manners. A lady can now call upon her neighbours with greater ease, and social life in the country is fast reviving, and is becoming pleasanter in consequence, for distance is disappearing as locomotion is becoming easier.

Against these favourable tendencies must be put the one disadvantage, although it is temporary in character, namely, that houses and cottages on or near the main roads during the summer are suffering greatly from dust. But the removal of this regrettable feature is only a question of time, and I must express my firm belief that the concentration of so many able minds upon this problem will shortly result in the invention of a durable and dustless road system.

Somewhat closely connected with the subject of the development of the country by motor cars must be mentioned the fashion of touring which is now so much in vogue, both at home and abroad. Up to five or six years ago people only knew the immediate locality in which they lived, and except they were ardent hunting men or keen cyclists nothing above ten or twelve miles away was, as a rule, visited. But now the more distant counties, the landscapes of the west of England, the mountains of Wales, the lakes of Cumberland, the dales of Yorkshire, the beauties of the Western Highlands, which were only dimly visible from a train or occasionally through the medium of cumbersome horse vehicles, are within the reach of thousands. The true tourist, intent upon picturesque scenery, and not upon speed, can feast his eyes and regale his senses in a month or two of touring in any locality which he chooses to select. In consequence, the old roadside inns of England are beginning to revive, though as yet they are still behind the standard of similar accommodation in France. But the demand will, in course of time, create the supply, especially if the better hostelries are taken in hand by careful management combined with enterprise. The wayside hotel in future should prove profitable to the owner also. As a Frenchman once said to me, "You have the most beautiful scenery in England, but you have no accommodation!" He was right. The facilities which the motor car provides for touring in one's own country (without touching upon the question of touring abroad) are also providing railway chairmen and boards with much food for thought. This new and sensible fashion of making journeys is leading to a great increase in road travelling. Railway companies are also endeavouring in many instances to give motor-bus facilities to localities distant from stations, and such services have been widely established. This, indeed, is a feature of modern railway development. The Great Western, one of the most progressive of our systems, has been especially courageous in this direction, and has now over 100 motor buses running in connection with its excellent railway services. These have now so firmly fixed themselves in public favour that when the local authorities refuse to

make the roadways up to a standard good enough for these vehicles to run upon, the railway companies have only to threaten to withdraw their services, and a drastic change is at once made by the electors in the constitution of the local highway authority, and the immediate mending of the road takes place.

I think I may here say a word as to the effect of motor touring from an international point of view. Every French motorist who lands here, and every British motorist who goes abroad, learns to know the other nation better, to understand its manners and habits, and to enter, to a certain extent, into its political and social life. There is no doubt that the friendly feeling between English and French motorists has largely helped to foster and assist the "entente cordiale" which has had so great an effect upon European politics. Generally speaking, the governments of European countries are trying to encourage the visits of touring motorists to their countries, and in some cases are laying themselves out to give every information by the establishment of agencies and bureaux in London and elsewhere. The motor car is thus not only teaching geography and appreciation of scenery, but fostering international sympathy. The warm welcome also which is, as a rule, given to one brother motorist by another even of different nationalities, proves that one touch of mechanics makes the whole world kin.

The importance of the world-wide increase in motoring is well illustrated by the following figures:—

ESTIMATED NUMBER OF MOTOR VEHICLES THROUGHOUT THE WORLD,
IN THE VARIOUS COUNTRIES AT CHRISTMAS 1907.

America	130,000
United Kingdom	125,320
France	32,530
Germany	22,000
Austria Hungary	9,000
Italy	6,500
Spain and Portugal	4,250
Other countries of Europe, including Russia, Greece, Turkey, Roumania, etc.	6,000
India and Burma	2,500
Egypt, Canada, Australia, Cape Colony, Transvaal, and other British Colonies	3,000
Central American States	1,000
China, Japan, Eastern Asia, and Malay States	1,250
South America, including Brazil, Argentina, Peru, Bolivia, etc.	3,000
	<hr/> 346,350

Estimate for Great Britain based on—

65,800 motor cars.

4,520 heavy motor vehicles for trade, etc.

55,000 motor bicycles.

Total 125,320

Finally, I would remind you all that the motor car is still in its infancy, although it is now beyond the stage in which it was only considered to be the toy of a few wealthy or enthusiastic persons. It is now used very largely by the professional classes, by doctors, agents, commercial travellers, judges on circuit, tradespeople, engineers, and even archbishops and bishops are benefiting by its use. The democracy, although its vehicles are perhaps not yet very comfortable, still owe to it already a measurable amount of relief in reduced rent and in facilities for living in better air, further afield from its work, and in securing more varied holidays. It is the swiftest thing on earth, having accomplished a speed of 127 miles an hour, or well over 2 miles in one minute. The world is gaining by a faster means of locomotion, for time means money in more ways than one. It is stimulating the road-maker and the engineer. Sanitation is the better for a diminution in the horse droppings in the street, which when dried are euphemistically termed dust. Cities will gain also in the welcome diminution in numbers of the house-fly, which scientists tell us is now one of the most active and potent agents in spreading diseases. It will thus increase health as well as comfort; and, in conclusion, I assert that the motor car has not only come to stay, but has come to improve our country, to make our lives more comfortable, to assist all classes, and to help in the steady forward progress of mankind.

[M. OF B.]

GENERAL MONTHLY MEETING,

Monday, April 6, 1908.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President. in the Chair.

William Patrick Byrne, Esq., C.B.

Greville Douglas, Esq.

Miss I. Mary Forbes,

Mrs. M. M. Hamilton,

William Alfred Johnstone, Esq.

Henry Wilson McConnel, Esq., M.A. M.B. M.R.C.S.

Archibald Keightley Nicholson, Esq.

John Oswald Esq., J.P.

John Edward Stead, Esq., J.P. F.R.S. F.C.S. F.I.C.

were elected Members of the Royal Institution.

The following After Easter Lecture Arrangements were announced :—

GERALD STONEY, Esq., B.E. M.Inst.C.E. Two Lectures on THE DEVELOPMENT OF THE MODERN TURBINE AND ITS APPLICATION. On *Tuesdays*, April 28, May 5.

PROFESSOR F. T. TROUTON, M.A. D.Sc. F.R.S. *M.R.I.* Two Lectures on (I.) WHY LIGHT IS BELIEVED TO BE A VIBRATION, (II.) WHAT IT IS WHICH VIBRATES. On *Tuesdays*, May 12, 19.

PROFESSOR WILLIAM STIRLING, M.D. LL.D. D.Sc., Fullerian Professor of Physiology, *R.I.* Two Lectures on ANIMAL HEAT AND ALLIED PHENOMENA. On *Tuesdays*, May 26, June 2.

WILLIAM BATESON, Esq., M.A. F.R.S. Three Lectures on MENDELIAN HEREDITY. (The Tyndall Lectures.) On *Thursdays*, April 30, May 7, 14.

ALEXANDER SCOTT, Esq., D.Sc. F.R.S. Treas. C.S. *M.R.I.* Three Lectures on THE CHEMISTRY OF PHOTOGRAPHY. On *Thursdays*, May 21, 28, June 4.

G. F. SCOTT ELLIOT, Esq., M.A. F.R.G.S. F.L.S. Two Lectures on CHILE AND THE CHILIANS. On *Saturdays*, May 2, 9.

LAURENCE BINYON, Esq. Two Lectures on JAPANESE PRINTS. On *Saturdays*, May 16, 23.

HENRY WALFORD DAVIES, Esq., Mus.Doc. LL.D. Two Lectures on THE ART OF BACH AND FUTURE DEVELOPMENTS. On *Saturdays*, May 30, June 6.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

British Museum Trustees—Catalogue of the Thomason Tracts. 2 vol. 8vo. 1908.

Catalogue of Additions to the Manuscripts, 1900–5. 8vo. 1907.

Catalogue of Drawings by British Artists, Vol. IV. S-Z. 8vo. 1907.

Catalogue of Finger Rings. 8vo. 1907.

- Lords of the Admiralty*—Nautical Almanac, 1911. 8vo. 1907.
- The Secretary of State for India*—Kodaikanal Observatory Bulletin, No. XII. 4to. 1908.
- Memoirs of the Geological Survey of India, Vol. XXXVI. Part 2. 8vo. 1907.
- Memoirs of Department of Agriculture, Botanical Series, Vol. II. No. 4. 8vo. 1907.
- Accademia dei Lincei, Reale, Roma*—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XVII. 1^o Semestre, Fasc. 3-5. 8vo. 1908.
- Agricultural Society, Royal*—Journal, Vol. LXVIII. 8vo. 1908.
- American Academy of Arts and Sciences*—Proceedings, Vol. XLIII. Nos. 13-14. 8vo. 1908.
- American Geographical Society*—Bulletin, Vol. XL. No. 2. 8vo. 1908.
- Asiatic Society, Royal (Bombay Branch)*—Journal, Vol. XXII. 8vo. 1908.
- Astronomical Society, Royal*—Monthly Notices, Vol. LXVIII. No. 4. 8vo. 1908.
- Automobile Club*—Journal for March, 1908. 8vo.
- Bankers Institute*—Journal, Vol. XXIX. Parts 3-4. 8vo. 1908.
- Batavia, Royal Magnetical and Meteorological Observatory*—Observations, Vol. XXVIII. 1905. 4to. 1907.
- Regenwaarnemingen in Nederlandsch-Indie, 1906. 8vo. 1907.
- Belgium, Royal Academy of Sciences*—Annuaire, 1908. 8vo.
- Mémoires, Collection in 4to, 2^e Serie, Tome I. Fasc 5. 4to. 1907.
- Birmingham and Midland Institute*—Meteorological Observations, 1907. 8vo. 1908.
- Boston Public Library*—Monthly Bulletin for March, 1908. 8vo.
- British Architects, Royal Institute of*—Journal, Third Series, Vol. XV. Nos. 9-11. 4to. 1908.
- British Association*—Report of the Seventy-seventh Meeting (Leicester), 1907. 8vo. 1908.
- British Astronomical Association*—Journal, Vol. XVIII. No. 5. 8vo. 1908.
- Cambridge Philosophical Society*—Proceedings, Vol. XIV. Part 4. 8vo. 1908.
- Chemical Industry, Society of*—Journal, Vol. XXVII. No. 4-6. 8vo. 1908.
- List of Members, 1908. 8vo.
- Chemical Society*—Journal for March, 1908. 8vo.
- Proceedings, Vol. XXIV. Nos. 338-339. 8vo. 1908.
- Cracovie, Académie des Sciences*—Bulletin, Philologie, 1907: Nos. 8-10; 1908, Nos. 1-2; Sciences Mathématiques, 1907, Nos. 9-10; 1908, Nos. 1-2. 8vo. 1907-8.
- Dax, Société de Borda*—Bulletin, 1907, Nos. 2-3. 8vo.
- Editors*—Agricultural Economist for April, 1908. 4to.
- American Journal of Science for March, 1908. 8vo.
- Analyst for March, 1908. 8vo.
- Astrophysical Journal for March, 1908. 8vo.
- Athenæum for March, 1908. 4to.
- Author for March-April, 1908. 8vo.
- British Homœopathic Review for March-April, 1908. 8vo.
- Chemical News for March, 1908. 4to.
- Chemist and Druggist for March, 1908. 8vo.
- Concrete for March, 1908. 8vo.
- Dioptric Review for March, 1908. 8vo.
- Dyer and Calico Printer for March, 1908. 4to.
- Electrical Contractor for March, 1908. 8vo.
- Electrical Engineer for March, 1908. 4to.
- Electrical Engineering for March, 1908. 4to.
- Electrical Industries for March, 1908. 4to.
- Electrical Review for March, 1908. 4to.
- Electrical Times for March, 1908. 4to.

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- Electricity for March, 1908. 8vo.
 Engineer for March, 1908. fol.
 Engineer-in-Charge for March, 1908. 8vo.
 Engineering for March, 1908. fol.
 Horological Journal for March, 1908. 8vo.
 Illuminating Engineer for March-April, 1908. 8vo.
 Journal of the British Dental Association for March, 1908. 8vo.
 Journal of Physical Chemistry for February-March, 1908. 8vo.
 Journal of State Medicine for March, 1908. 8vo.
 Law Journal for March, 1908. 8vo.
 London University Gazette for March, 1908. 4to.
 Model Engineer for March, 1908. 8vo.
 Motor Car Journal for March, 1908. 4to.
 Musical Times for March, 1908. 8vo.
 Nature for March, 1908. 4to.
 New Church Magazine for April, 1908. 8vo.
 Nuovo Cimento for January, 1908. 8vo.
 Page's Weekly for March, 1908. 8vo.
 Photographic News for March, 1908. 8vo.
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 Revue d'Electrochimie for January, 1908. 8vo.
 Science Abstracts for February-March, 1908. 8vo.
 Terrestrial Magnetism for December, 1907. 8vo.
 Zoophilist for March-April, 1908. 4to.
Electrical Engineers, Institution of—Journal, Vol. XL. No. 187. 8vo. 1908.
Faraday Society—Transactions, Vol. III. Part 3. 8vo. 1908.
Florence Biblioteca Nazionale—Bulletin for February-March, 1908. 8vo.
Franklin Institute—Journal, Vol. CLXV. No. 3. 8vo. 1908.
Geographical Society, Royal—Journal, Vol. XXXI. Nos. 3-4. 8vo. 1908.
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Quarterly Journal, Vol. LXIV. Part 1. 8vo. 1908.
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Haarlem, Société Hollandaise des Sciences—Verhandlingen, Deel VI. Parts 3-4. 4to. 1907.
Harvard College Observatory—Sixty-second Annual Report. 8vo. 1908.
Horticultural Society, Royal—Journal, Vol. XXXIII. Part 1. 8vo. 1908.
Johns Hopkins University, Baltimore—Studies: Series XXV. Nos. 8-12. 8vo. 1907.
 Circulars, 1907, No. 9; 1908, No. 1. 8vo. 1907-8.
Legge, F., Esq. M.R.I.—An Introduction to Electricity. By B. Kolbe. 8vo. 1908.
London County Council—Gazette for March, 1908. 4to.
Madrid, Royal Academy of Sciences—Anuario, 1908. 32mo.
 Revista, Tom. VI. Nos. 5-8. 8vo. 1907-8.
Monaco, L'Institut Océanographique—Bulletin, Nos. 111-114. 8vo. 1907-8.
Munich, Royal Bavarian Academy of Sciences—Abhandlungen, Mat.-Phys. Klasse, Band XXIII. Ab. 2; Band XXIV. Ab. 1. 4to. 1907.
National Church League—Church Gazette for March, 1908. 8vo.
Navy League—Navy League Journal for March-April, 1908. 8vo.
New York Academy of Sciences—Annales, Vol. XVII. Part 3; Vol. XVIII. Part 1. 8vo. 1907-8.
Paris, Comité International des Poids et Mesures—Procès-Verbaux des Séances, 2^e Série, Tome IV. 8vo. 1907.
 Travaux et Mémoires, Tome XIII. 4to. 1907.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for Feb. 1908. 4to.

- Pennsylvania University*—Catalogue, 1907-8. 8vo. 1908.
- Peru, Cuerpo de Ingenieros de Minas*—Boletin, No. 53. 8vo. 1907.
- Pharmaceutical Society of Great Britain*—Journal for March, 1908. 8vo.
- Photographic Society, Royal*—Journal, Vol. XLVIII. No. 3. 8vo. 1908.
- Röntgen Society*—Journal, Nos. 14-15, Vol. IV. 8vo. 1908.
- Royal Engineers' Institute*—Journal, Vol. VII. No. 4. 8vo. 1908.
- Royal Irish Academy*—Proceedings, Vol. XXVI. Section A, No. 9; Vol. XXVII. Section A, No. 8; Section C, No. 4. 8vo. 1908.
- Royal Society of Arts*—Journal for March, 1908. 8vo.
- Royal Society of London*—Philosophical Transactions, A, Vol. CCVIII. Nos. 426-429; B, Vol. CXCIX. No. 260. 4to. 1908.
- Proceedings, Vol. LXXX. Series A, No. 537; Series B, No. 537. 8vo. 1908.
- Year Book, 1908. 8vo.
- St. Bartholomew's Hospital*—Reports, Vol. XLIII. 8vo. 1908.
- St. Petersburg, Imperial Academy of Sciences*—Bulletin, VI^e Serie. 1908, Nos. 4-5. 4to. 1908.
- Sanitary Institute, Royal*—Journal, Vol. XXIX. Nos. 2-3. 8vo. 1908.
- Selborne Society*—Nature Notes for March-April. 8vo. 1908.
- Smith, B. Leigh, Esq., M.R.I.*—The Scottish Geographical Magazine, Vol. XXIV. Nos. 3-4. 8vo. 1908.
- Societa degli Spettroscopisti Italiani*—Memorie, Vol. XXXVIII. Disp. 2. 4to. 1908.
- Stonyhurst College Observatory*—Meteorological and Magnetical Observations, 1907. 8vo. 1908.
- United Service Institution, Royal*—Journal for March, 1908. 8vo.
- United States Department of Agriculture*—Experiment Station Record, Vol. XIX. No. 5. 8vo. 1908.
- United States Department of Commerce and Labour*—Bulletin of the Bureau of Standards, Vol. IV. No. 3. 8vo. 1908.
- Report on Terrestrial Magnetism for 1907. 8vo. 1908.
- United States, Library of Congress*—Report of the Librarian, 1907. 8vo.
- United States Patent Office*—Official Gazette, Vol. CXXXII. No. 8; Vol. CXXXIII. Nos. 1-4. 8vo. 1908.
- Verein zur Beförderung des Gewerbfleisses in Preussen*—Verhandlungen, 1908, Heft 3. 4to.
- Vienna Imperial Geological Institute*—Verhandlungen, 1908, No. 1. 8vo.
- Abhandlungen, Band XVI. Heft 2. 4to. 1907.
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- Monthly Statistical Abstract for January, 1908. 4to.

WEEKLY EVENING MEETING,

Friday, April 10, 1908.

THE RIGHT HON. LORD RAYLEIGH, O.M. P.C. D.C.L.
LL.D. F.R.S., in the Chair.

PROFESSOR SIR J. J. THOMSON, M.A. LL.D. D.Sc. F.R.S. *M.R.I.* ;
Professor of Natural Philosophy, Royal Institution.

The Carriers of Positive Electricity.

THOUGH the ordinary cathode rays are the most conspicuous of the rays spreading out from the cathode in a vacuum-tube, there are other rays mixed with them, which as Goldstein* and the writer† showed long ago are not appreciably deflected by weak magnetic fields. The very complete study of the region near the cathode made by Goldstein, the results of which are described in a paper read before the Physical Society of Berlin,‡ has led him to distinguish five kinds of rays besides the cathode rays. Recent experiments made by Villard§ and the author|| have shown that some of these rays are deflected in electric and strong magnetic fields, and the direction of the deflexion indicates that they carry a positive charge of electricity. The fact that these positively charged rays travel with high velocities away from the cathode, and thus against the direction of the electric force, makes the investigation of their properties a very interesting problem, and I have lately made a series of experiments with the object of obtaining some information as to their nature and origin.

The tube used in the first series of experiments is represented in Fig. 1. A is a perforated electrode through which rays can pass on their way to the phosphorescent screen S covered with Willemite; the rays on their journey to the screen traverse strong electric and magnetic fields, the former produced by charging the plates LM to different potentials and the latter by placing the tube between the poles of a powerful electromagnet. From the deflexions which these produce on the rays, the velocities and values of e/m for the rays can be determined in the usual way. B is a flat electrode at the other end of the tube; this electrode is carried by a stopper working in a ground-glass joint, and can be rotated about a vertical axis. C is an auxiliary electrode at the side of the tube. D is a side tube in which

* Verhandl. d. Deutsch. physik. Gesellsch. iv. p. 228, 1902.

† Proc. Camb. Phil. Soc. ix. p. 243. ‡ Republished Phil. Mag. Mar. 1908.

§ Comptes Rendus, cxliii. p. 673, 1906. || Phil. Mag. xiv. p. 359, 1907.

a metallic obstacle is placed at the end of a rod, this end is fastened to a closed glass vessel containing a piece of iron. By moving this vessel along D by means of a magnet, the obstacle can either be inserted in the line of fire of the rays coming from B and passing down the hole in A, or withdrawn into the tube; the obstacle is in metallic

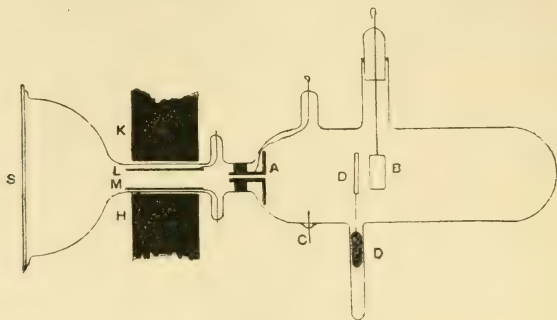


FIG. 1.

connexion with a wire leading out of the tube, so that it can be used as a cathode if required. The discharge through the tube was produced in some cases by a large induction-coil, in others by a Wims-hurst machine.

If the stopper carrying the electrode B were turned so that the normal to the electrode coincided with the axis of the tube A, or made only a small angle with it, and if B were made cathode and a discharge sent through the tube, then in addition to the cathode rays other rays passed through the tube in A and excited phosphorescence on the screen. The direction of the deflexions of the phosphorescence under electric and magnetic forces showed that these rays were charged with positive electricity.

If A were made cathode, the ordinary Canalstrahlen produced bright phosphorescence on the screen. The first point investigated was to make sure that the positive rays observed when A was anode, were not due to reversals of the induction-coil making A at times a cathode and sending ordinary Canalstrahlen down the tube. Very simple observations, however, showed that this could not be the explanation. In the first place, the positive rays still passed down the tube when A was disconnected from the coil and the auxiliary electrode C used as an anode; secondly, A being connected with the coil, the rays down the tube disappeared when B was twisted round, so that the normal to its plane made a considerable angle with the axis of the tube; and thirdly, the rays down the tube were stopped when the obstacle in the side tube was pushed forward so as to be in the line between the cathode and the aperture in the anode.

The next point investigated was to see whether the effect might

not be due to A acting at times as a cathode in virtue of the negative charge given up to it by the cathode rays starting from B. That this is not the explanation is proved by the following experiments. When the cathode rays were diverted by a magnet so that they no longer fell upon A, the brightness of the phosphorescent patch due to the positive rays going down the tube in A was not appreciably diminished, although the tendency for A to become cathode must have been almost entirely removed. The conclusions drawn from this experiment were confirmed by the results obtained when the obstacle in the side tube was used as a cathode instead of B. When the obstacle was not pushed far enough across the tube for its normal to pass down the tube in A, no positive rays passed down the tube, but as soon as the obstacle had advanced into such a position that its normal went down the aperture in A, the phosphorescence on the screen due to the positive rays appeared. The contrast between the brightness of the phosphorescence when the normal to the obstacle went down the hole in A and when it did not was very sharp, though there was very little variation in the number of cathode rays striking against the anode as a whole. These experiments show that the positive rays under discussion are not due to reversals of the induction-coil nor to the negative electrification of A by the bombardment of cathode rays, but that they originate at the cathode and travel away from it down the tube.

By means of the rotating cathode B we can determine whether the positive rays coming from the cathode are emitted normally to its surface, or whether, like some of the rays observed by Goldstein, they come off in all directions. When the normal to the cathode went down the tube in A, a plentiful supply of positive rays went down the tube. When the cathode was rotated, the phosphorescence due to the positive rays did not immediately disappear, although it became very much fainter; it could, however, be detected until the normal to the cathode made an angle of about 15° with the axis of the tube. The positive rays under discussion appear to follow much the same path as the cathode rays, for it was found that the angle of rotation required to prevent these getting down the tube was much the same as that required to extinguish the phosphorescence due to the positive rays.

Properties of these Positive Rays.

These rays get exceedingly faint at very low pressures, and cease to be observable at pressures when the Canalstrahlen are still quite bright. It is probably due to this that I have never been able to observe the resolution of the phosphorescence, under the action of electric and magnetic forces, into separate patches as in the case of Canalstrahlen when the pressure is low.* The spot of phosphorescence

* Phil. Mag. xiii. p. 561, 1907.

due to the positive rays coming out in front of the cathode is, under the action of electric and magnetic deflexion of the rays, drawn out into a continuous band, even when the pressure is such that the phosphorescence due to the Canalstrahlen shows well-developed patches. In the case of the Canalstrahlen, there are some rays whose deflexion shows that they are negatively charged and have a mass comparable with that of the positive rays. We find, too, in the case of the rays travelling in the opposite direction to the Canalstrahlen, that a considerable number of them are negatively charged particles, indeed I think the proportion of the negative to the positive is greater in this case than in that of the Canalstrahlen. I have observed cases in which the phosphorescence due to the negatively charged particles was little, if at all, less than that due to the positively charged ones.

A large number of determinations were made, by the method described in my first paper,* of the velocity and values of e/m for these rays; in consequence of the spot of phosphorescence being drawn out into a band, the values of e/m ranged continuously from 0 to a maximum value.

Two tubes were used for this purpose, in one of them (fig. 1) the electrode B was connected with a stopper ground into the tube, one side of the electrode was aluminium, the other calcium; by turning the stopper either side could be made to face the tube A, down which the rays passed. L M are parallel plates, which can be connected with a battery of storage-cells; when this is done, rays passing between the plates are acted on by a strong electric field. S is the screen at the end of the tube: H, K the pole-pieces of the electro-magnets. C is a wire fused into the tube, to serve as an electrode, thus allowing A to be insulated or connected with one or other of the electrodes at will. The dimensions of the parts of the tube which affect the deflexions of the phosphorescence are as follows:—

Distance between the plates L and M = 0.45 cm.

Length of these plates = 3.8 cm.

Distance between the screen and the
end of the plates = 4.0 cm.

If V is the potential in volts between the plates, e the charge, m the mass, and v the velocity of the rays, D the deflexion due to the electrostatic field, we can easily prove that

$$\frac{e}{mv^2} = \frac{D}{5 \times 10^9 V}.$$

While if d is the deflexion due to the magnetic field when a current of 1 ampere is passing through the coils, it was found by the method described † that

$$\frac{e}{mv} = \frac{d}{2.8 \times 10^4}.$$

* Phil. Mag. xiii. p. 561, 1907.

† Loc. cit.

From these equations the following determinations of e/m and v were made ; the value of V was 420 volts.

Tube filled with Air.			
D.	d.	e/m .	v .
·5	1·4	10^4	2×10^8 cm./sec.
·4	1·1	$\cdot 78 \times 10^4$	2×10^8 "
·5	1·4	10^4	2×10^8 "
·45	1·3	$\cdot 96 \times 10^4$	$2\cdot 1 \times 10^8$ "
·4	1·4	$1\cdot 25 \times 10^4$	$2\cdot 5 \times 10^8$ "
·4	1·3	$1\cdot 08 \times 10^4$	$2\cdot 3 \times 10^8$ "
·3	1·1	$1\cdot 04 \times 10^4$	$2\cdot 6 \times 10^8$ "
Mean . .		$1\cdot 01 \times 10^4$	
Tube filled with Hydrogen.			
·7	1·7	$1\cdot 05 \times 10^4$	$1\cdot 8 \times 10^8$ cm./sec.
·4	1·3	$1\cdot 08 \times 10^4$	$2\cdot 3 \times 10^8$ "
·6	1·5	$\cdot 96 \times 10^4$	$1\cdot 7 \times 10^8$ "
·4	1·3	$1\cdot 08 \times 10^4$	$2\cdot 3 \times 10^8$ "
Mean . .		$1\cdot 04 \times 10^4$	

The other type of tube (Fig. 2), in which there were no auxiliary electrodes, had the following dimensions :—

Distance between the plates L and M . . . = 0·4 cm.

Length of plates = 4·0 cm.

Distance between screen and plate . . . = 3·5 cm.

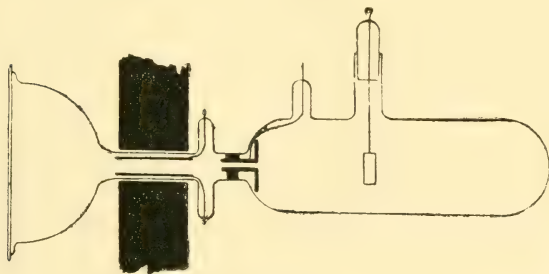


FIG. 2.

Hence if V is the potential-difference in volts between the plates, and D , as before, the deflexion in the electrostatic field,

$$\frac{e}{mv^2} = \frac{D}{5\cdot 5 \times 10^9 V}.$$

The magnet had been readjusted and furnished with new pole-pieces. If d was the magnetic deflexion, it was found that when 1 ampere was the current through the coils

$$\frac{e}{mv} = \frac{d}{4.5 \times 10^4}$$

With this tube the following values of e/m and v were obtained for the positive rays proceeding from the cathode in the opposite direction to the Canalstrahlen. The value of V was 400 volts.

Tube filled with Air.			
D.	d .	e/m .	v .
·4	1·7	$\cdot 83 \times 10^4$	$2 \cdot 2 \times 10^8$ cm./sec.
·3	1·6	$\cdot 9 \times 10^4$	$2 \cdot 6 \times 10^8$ „
·5	1·9	$\cdot 72 \times 10^4$	$1 \cdot 7 \times 10^8$ „
·5	2·0	$\cdot 9 \times 10^4$	$2 \cdot 0 \times 10^8$ „
·4	1·8	$\cdot 9 \times 10^4$	$2 \cdot 2 \times 10^8$ „
·2	1·3	$\cdot 9 \times 10^4$	$3 \cdot 2 \times 10^8$ „
·4	1·7	$\cdot 83 \times 10^4$	$2 \cdot 2 \times 10^8$ „
Mean . .		$\cdot 85 \times 10^4$	
Tube filled with Helium.			
·5	20	$\cdot 9 \times 10^4$	$2 \cdot 0 \times 10^8$ cm./sec.
·5	22	$1 \cdot 08 \times 10^4$	$2 \cdot 2 \times 10^8$ „
·5	21	1×10^4	$2 \cdot 1 \times 10^8$ „
Mean . .		$\cdot 99 \times 10^4$	

The deflexions of the Canalstrahlen in the same tube were also observed: this could easily be done by making the tube A the cathode. It was found that the structure of the Canalstrahlen was more complicated than that of the retrograde positive rays, the former showing two types of rays, characterised by values of e/m in the ratio of 2 to 1: the latter showed only one type of ray, this type coinciding, however, both as to value of e/m and the velocity with the type of Canalstrahlen rays having the maximum value of e/m . Some of the retrograde rays, as well as some of the Canalstrahlen, are deflected in a direction which shows that they carry a negative charge, and that their mass is comparable with that of the positively charged particles. I have determined the value of e/m and the velocity of these negative constituents of the retrograde rays, and find that the

value of e/m for the negative particles is numerically equal to that of e/m for the positive ones; while the velocity of the negative ones is slightly, but only slightly (about 15 per cent. in my experiments), less than that of the positive ones.

Experiments with Goldstein's Double Cathodes.

Goldstein* found that when the cathode consists of two parallel plates in metallic connexion, rays other than the cathode rays proceed from the space between the plates. If the plates are triangles, Goldstein found that a pencil of easily deflected cathode rays starts from the middle points of the sides, while pencils of undeflected rays start from the corners of the triangle. The difference in the character of the rays can be strikingly shown by using helium in the discharge-tube, when the rays from the corners are red and those from the sides blue. I have examined the electric and magnetic deflexion of these rays, using for the purpose a tube such as that shown in Fig. 3.

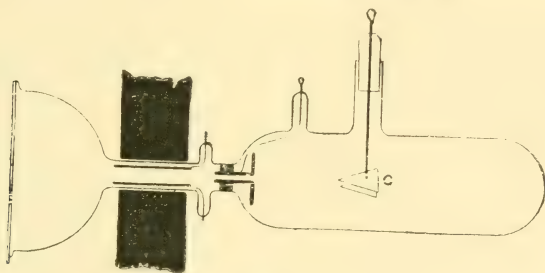


FIG. 3.

The cathode consists of two parallel triangles, and is carried by a stopper working in a ground-glass tube. By turning the stopper the different parts of the triangular cathode could be brought opposite to the opening in the tube and the distribution of the rays round the triangle studied. I found that this distribution depended a good deal upon the pressure of the gas in the discharge-tube. At all the pressures I tried, I found that the maximum emission of ordinary cathode rays was along the line starting from the middle points of the sides; at the higher pressures, this was the only direction in which the cathode rays could be detected; at very low pressures, however, rays could be detected starting from the corners of the triangle, as well as from the middle points of the sides; few, if any, however, were given out in any intermediate position. With regard to the positive rays, I found at all the pressures I tried that these

* Loc. cit.

streamed off from both the corners and the middle points of the sides, there were but few in any intermediate position ; the most abundant stream came, as was the case for cathode rays, from the middle points of the sides, but the disproportion between the streams from the corners and from the sides was nothing like so great for the positive as it was for the cathode rays ; so that the ratio of the quantity of the positive rays to the quantity of cathode rays was greatest at the corners of the triangle.

I also measured the velocity and the value of e/m for the positive rays. I found that this was the same whether the rays came from the corners or from the sides, and the same as that of one type of Canalstrahlen (the type for which $e/m = 10^4$), which went down the tube when A was made cathode. In the case of the positive rays coming from the triangular cathode, often only a small fraction were at all affected by electric and magnetic forces, by far the larger portions were quite undeflected by these forces ; so that the phosphorescence of the screen when the magnetic force was applied to the tube presented the appearance of a bright central undeflected patch with a faintly luminous tail.

We can, I think, explain this distribution of the rays from the triangular cathode on the view that there is a reciprocity between the cathode rays and the positive rays of the following kind. The corpuscles in the cathode rays are due to the impact of positive ions at or near the cathode, while the positive rays are due to the impact of the corpuscles at some distance from the cathode. Let us consider now what happens when the lines of electric force in the neighbourhood

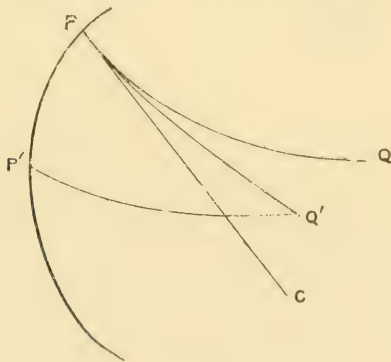


FIG. 4.

of the cathode are curved as in Fig. 4. A corpuscle starting along the normal from P would on account of its inertia not follow the line of force PQ but some path PQ' between PC and PQ, PC being the normal to the cathode. If, now, the corpuscle produces a posi-

tive ion at Q' , this ion as it moves up to the cathode will not follow the path $Q'P$ but some such path as $Q'P'$, striking the cathode at P' , and producing a cathode ray at P' . Thus the positive particle, if it strikes the cathode at all, will not give rise to a cathode ray to replace the one which produced it, but a ray starting from some other region. If, however, the line of force starting from P were a straight line, the positive particle produced by the ray at P would strike the cathode at P . When the discharge is in a steady state the number of corpuscles coming from *any region* must be proportional to the number of positive particles falling on that region. Now this will be the case when and only (except perhaps in very special cases) when the positive rays which strike the region are those produced by the corpuscles coming from it. For this to happen, the lines of force from that region must be straight lines. In the case of the triangular cathode there are six regions where the lines of force are straight, the middle points of the sides and the corners: and it is therefore from these regions that we should expect the discharge to be concentrated; and inasmuch as the region over which the lines are approximately straight is much greater at the middle points of the sides than at the corners, we should expect the maximum discharge to come from the middle point of the sides.

Magneto-Cathodic Rays.

Among the rays which sometimes occur near the cathode are some observed by Villard,* and called by him magneto-cathodic rays. These rays occur when the discharge-tube is placed in a strong magnetic field; they are in the direction of the lines of magnetic force, and when subject to an external electric field they are displaced in a direction at right angles to the electric and also to the magnetic force. Ions moving through a medium in which they are under the action of electric and magnetic forces, and which resists their motion with a force proportional to their velocity, would behave exactly in this manner. For, just as a stone falling through a resisting medium moves at first with nearly uniform acceleration but after a time settles down into a state where the velocity is constant and equal to what is known as the limiting velocity, so ions, when exposed to electric and magnetic forces and to a resistance proportional to their velocity, will after a time settle down to a state where the velocity is uniform. The time required to reach this state is inversely proportional to the resistance when the velocity is unity and directly proportional to the mass of the ion. I have shown in my "Conduction of Electricity through Gases"† that when the ions have reached this state, and the magnetic

* Comptes Rendus, cix. p. 42, 1905.

† Second edition, p. 106.

force H is so large that Hu_0 is large compared with unity, u_0 being the velocity acquired by an ion when unit force acts upon it, and inversely proportional to the pressure, then the ions move nearly along the lines of magnetic force, but have a small component in the direction at right angles to both the electric and magnetic forces. In these respects they resemble Villard's magneto-cathodic rays; the ions, however, would carry electric charges, while Villard could detect no charge when the magneto-cathodic rays entered a Faraday cylinder. It is perhaps possible that the absence of charge may have been due to the rays making the gas round the Faraday cylinder so good a conductor of electricity, that no appreciable charge could accumulate in the cylinder.

On the Method by which the Retrograde Rays acquire their Velocity.

We have seen that the velocity of those positive rays which move away from the cathode, and which are, when under observation, free from the influence of the electric field in the tube, is practically the same as that of the Canalstrahlen which have moved up to and then passed through the cathode. This result is remarkable, for in the latter case the action of the electric field which produces the discharge in the tube would increase the velocity of a positively charged particle, while in the former it would diminish it. The fact that the velocities in the two cases are very much the same suggests, at first, the suspicion that the electric field may not be accountable for any considerable portion of the velocity in either case, and that perhaps the particles forming these rays may, like the α particles given out by radioactive substances, start with a high initial velocity, much higher than they could acquire under the electric field. If we refer to the table of velocities, we shall see that there is not, in the different experiments, much variation in the velocity of the particles; these are always 2×10^3 cm./sec., and could be generated by a fall through a potential difference of about 20,000 volts. We must not, however, attach too much importance to the constancy of the velocity, for the range of pressure over which we can make accurate observations on the retrograde rays is very limited. For when the pressure gets very low, and the discharge requires a high potential-difference to send it through the tube, the rays are not bright enough to be observed; while if the pressure is more than a small fraction of a millimetre, the rays either do not reach the screen, or when they do reach it are so diffuse that the phosphorescent patch is not definite enough for its position to be measured with accuracy. And although even rough measurements show that at these high pressures the velocity of the particles is less than when the pressure is low, we should not be justified without further evidence in concluding

that the initial velocity was less, for before reaching the screen the rays have had to make a long journey through the gas, and if the pressure is high, they will in their journey lose more of their velocity than when the pressure is low.

To test whether or not the velocity of the rays was due to the electric field, I tried the following experiments:—

In the first experiment, a piece of wire gauze was placed about 2 mm. in front of the perforated electrode of the tube represented in Fig. 2 and well insulated from it; the gauze was used as the cathode, and measurements of the velocity of the particles were made (1) when the perforated electrode was connected with the gauze, (2) when it was connected with the anode. In the second case, a particle if it retained its charge during the whole of its path, would in its journey between the gauze and the perforated electrode be at least as much retarded as it had been accelerated before reaching the gauze, and any velocity it possessed after passing through the perforated electrode must have been acquired from sources independent of the electric field; while in the first case its velocity would be measured by the electric field in the tube. On trying the experiment, it was found that though the Canalstrahlen were not nearly as bright in the second case as in the first, they were still quite perceptible, and that the velocity of those which got through was the same as the velocity of those reaching the screen in the first case. The fact that a large proportion of the rays are stopped by connecting the perforated electrode with the anode, while those which get through are not affected, shows that the velocity of the majority of them is not great enough to travel against the potential-difference between the electrodes; while the fact that some get through without diminution of velocity, indicates that when they are passing between the gauze and the perforated electrode, they are for the moment electrically neutral and without charge, and that they re-acquire, by losing a corpuscle, a positive charge after passing through the opening in the electrode by collision with the molecules of the gas. The following experiment shows in perhaps a simpler way than the preceding one that some of the Canalstrahlen are uncharged during a portion of their path. The perforated cathode (Fig. 5) was wedge-shaped, the angle of the wedge being about 27° , the diameter of the cathode was 2 cm., the aperture through which the Canalstrahlen passed was about 5 mm. from the sharp end, the length of the path of the rays from one side of the cathode to another was about 2.5 mm. A flat piece of wire gauze was fixed parallel to the upper face of the cathode (the face most remote from the anode), and insulated from it: the distance of the gauze from the cathode was about 3 mm. The Canalstrahlen travel at right angles to the lower face A B of the cathode. If the wire gauze is connected with the anode, and if the particles in the Canalstrahlen are charged, they will after passing through the cathode be acted on by a force which has a component at

right angles to their direction of projection : thus if they are positively charged they will be bent to the right, if they are negatively charged to the left, while if they are uncharged they will be undeflected. The path of the rays when the pressure of the gas is not too low, can

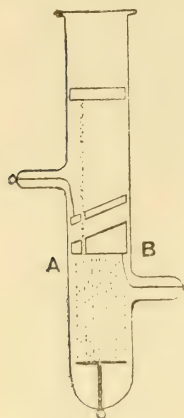


FIG. 5.

readily be traced by the luminosity they produce in the gas as they pass through it. On trying the experiment, it was found that when the gauze was connected with the anode the path of the few rays which got through the gauze was a straight line, coinciding in direction with their path before passing through the cathode. An easy way of seeing this is to connect by means of a key the gauze in quick succession with the anode and the cathode, when it is easily seen that though the Canalstrahlen are much more numerous in the latter case than in the former, the paths of those which do get through are identical, so that even when the gauze is connected with the anode some of the rays get through the space between *cd* and *cf* without suffering any deflexion, showing that they must be uncharged as they pass through this region. It is thus evident that a considerable number of the positively charged Canalstrahlen lose their

positive charge by attracting when in the neighbourhood of the cathode a negatively electrified corpuscle : the mass of the corpuscle is so small in comparison with that of the particles forming the Canalstrahlen, that the addition of the corpuscle will not materially reduce the velocity of the Canalstrahlen. These rapidly moving uncharged particles will soon get ionized by collision, and by losing a negatively electrified corpuscle again become positively charged.

In my first paper on the positive rays,* I showed that at not too low pressures the appearance presented by the phosphorescence on the screen indicated that many of the particles in the Canalstrahlen were positively charged for only a portion of their path ; the experiments just described are very direct evidence of this effect.

Again, at not too low pressures the Canalstrahlen are accompanied by negatively electrified particles having masses and velocities equal to those of the positive particles ; the negatively electrified particles in my experiments always being less numerous than the positive ones, in most cases very much so, in others the difference was not very great. We should expect the negatively electrified particles to be less numerous than the positive ones, since they would more readily lose their charges by collision.

I think that the positive rays which travel away from the cathode

* Phil. Mag. xiii. p. 561.

arise in the same way as the negative rays which accompany the Canalstrahlen. Let us consider what happens to the Canalstrahlen as they approach the cathode. When they reach the cathode some of them, as we have seen, get neutralized there, some will go further than this, and by gathering another corpuscle will become negatively electrified; those negatively electrified ones will, however, be repelled from the cathode, and under the action of the electric field will acquire a velocity of the same order as that acquired by the positive particles in their approach to the cathode. The rapidly moving electrified particles will in their course through the gas soon lose corpuscles by collision and thus become positively electrified, forming the positive rays which come from the cathode. Such rays, however, on the view just given start their journey with a negative charge.

The Canalstrahlen and the positive retrograde rays are not found with all types of discharge; thus, in the type of discharge sometimes called the flash discharge, which occurs when a condenser of large capacity is earthed through the discharge-tube, the discharge passes as a column of uniform luminosity stretching from one electrode to the other, and there is no dark space in the neighbourhood of the cathode. In this case I have never been able to detect positive rays of any kind, either in front of or behind the cathode.

It is important to distinguish between the positive ions to be found in a gas, ionized by Röntgen rays and not exposed to electric fields strong enough to give to them very high velocities; and the positive ions which, like those in the Canalstrahlen, have very great kinetic energy. For between the positive charges and the molecules there are forces comparable in intensity to those which exist between the atoms of different elements having the greatest chemical affinity for each other. Thus, unless the positive ions possess more than certain amount of kinetic energy, combination will go on with great rapidity and positively charged aggregates will be formed. If, however, the positive ions are moving with great rapidity, they will be in a state analogous to a gas at a very high temperature, and at these very high temperatures chemical combination does not take place. We have seen that the particles in the Canalstrahlen have a velocity of the order of 2×10^8 cm./sec. With a velocity such as this their kinetic energy would be equal to the mean kinetic energy of the molecules of a gas at a temperature of many hundred thousand degrees absolute; and though, as I have shown,* a positive ion might be expected to combine with a corpuscle if its velocity were but a little less than this, it would not be likely to do so with an uncharged molecule where the attraction would be very much less. If we take the case of a positive ion of mass m projected with a velocity V at right angles to the line joining it with a molecule of

* Conduction of Electricity through gases, 2nd edit. p. 360.

mass M , and at a given distance from it, the condition that the ion should be able to get away from the molecule is that

$$mV^2 > \frac{M}{m + M}$$

should be greater than a certain quantity depending on the force between the molecules and the ion and apsidal distance between them. Now if the force were independent of the mass of the molecule, we see that it would require a greater value of mV^2 to separate the ion from the molecule, when M is small compared with m , than when it is large. Thus if the kinetic energy of the ions were gradually to diminish say by collisions with the molecules, then if there were molecules of different masses in the gas through which the Canalstrahlen are moving, combination would occur first with the molecules of smallest mass, while the heaviest molecules would be the last to combine. The lightest molecules would thus have the first pick of the ions, which would therefore tend to be absorbed by the lightest gases. The force between an ion and a molecule is proportional to the volume of the molecule; and if the volume of a molecule were to increase as rapidly as its mass, the preceding considerations would not be valid. We have every reason, however, to believe that the changes in the volume of the molecules are not comparable with the changes in the masses: that, for example, the volume of a molecule of oxygen, instead of being sixteen times that of a molecule of hydrogen, is hardly more than twice, so that the increase in the forces exerted by the heavier molecules is not sufficient to counteract the influence of the increased mass.

*The Nature of the Positive Ions in different Gases when
the Ionization has settled into a steady state.*

The Canalstrahlen are formed in very intense electric fields, and the kinetic energy which they possess tends to prevent them combining with the molecules and corpuscles around them; they are thus under quite different conditions from the ordinary ions produced in a region where the electric force is small or absent, for these have time before being removed from the field to enter into combination with the molecules, the system of molecules and ions getting into a steady state if the source of ionization is constant. The difference between this case and that of the Canalstrahlen may be compared with the difference between the state of a mixture of different chemical substances after they have entered into combination and settled into a state of equilibrium and when they were first mixed. I thought that it would be interesting to determine the values of e/m for the ordinary ions simultaneously with the determination of e/m for the Canalstrahlen in the same discharge-tube. The method used to

determine e/m for the ordinary ions was as follows. A tube represented in Fig. 6 was sealed on to the tubes such as have already been described in connexion with the determination of e/m for the Canalstrahlen. B is an ionization chamber, the gas in it being ionized by cathode rays coming down through the tube C which was connected with the earth. The cathode was at D in front of the tube, the anode in a side tube F. Three parallel electrodes, L, M, N, were put in the ionization chamber. The first, L, was a plate at the top of the tube; the second, M, near the bottom. Of the tube was a piece of wire gauze about a millimetre above N, which was the top of an

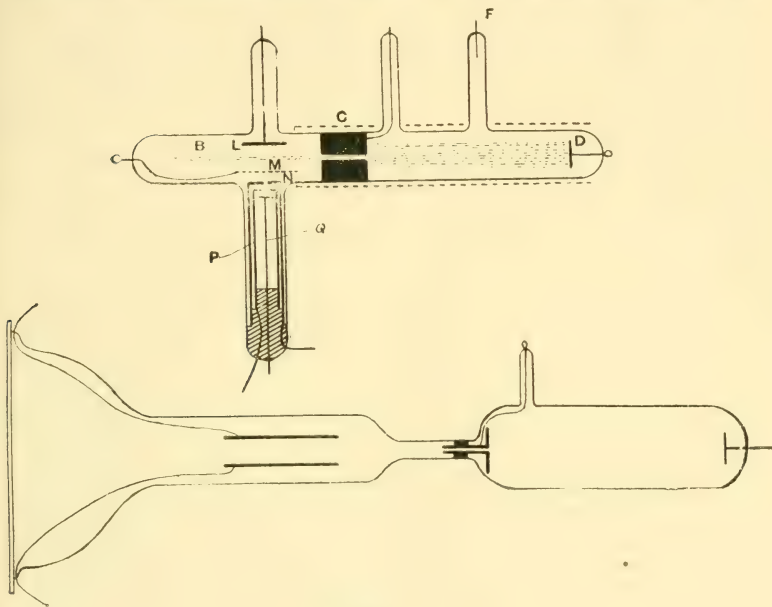


FIG. 6.

earth-connected cylinder, with a small hole 0.9 mm. in diameter bored through the centre, the thickness of this plate was 1.6 mm. By means of these electrodes ions could be collected and some of them sent through the hole with a definite and known velocity. Suppose for example we wish to send a stream of positive ions through the hole. A small difference of potential (in our experiments generally that due to two Leclanché cells) was maintained between the plates L M, L being at the higher potential. The electric field produced in this way caused a stream of slowly moving positive ions to pass downwards through the gauze; by means of a potential divider any potential-difference between 10 and 800 volts could be established

between the gauze and the top of the cylinder, the gauze being positive to the cylinder. The ions collected by the upper plates thus entered into a much stronger field which gave to them a velocity much greater than that with which they entered it, so that when they passed through the hole they were all moving with practically the same velocity.

Beneath the top of the box there was an insulated Faraday cylinder (P) connected with a Wilson electroscope. The distance between the top of the cylinder and the bottom of the plate was 1 mm. in one piece of apparatus, 0.5 mm. in another; the diameter of the hole in the Faraday cylinder was 2.3 mm. Beneath this hole there was a metal disk insulated and connected with another Wilson electroscope; the plane of the disk was parallel to N and thus at right angles to the undeviated path of the ions, the axis of the hole in N passed through the centre of the disk. The part of the tube below N was placed between the poles of a powerful electromagnet, the lines of magnetic force being at right angles to the undeviated path of the ions, and parallel to the direction of the cathode rays. To protect the cathode rays coming from D from the magnetic field, a deep cutting was made in one of the poles of the electromagnet and the portion from C to D of the tube placed in this and then covered over with layers of soft iron. This tube was sealed on to the tube T of the kind already described, for the determination of e/m for the Canalstrahlen.

If the ions travelled without deviation parallel to the axis of the tunnel in N, they would all strike against the disk Q, and the Faraday cylinder would not receive any charge. If, on the other hand, they were very much deflected by the magnetic field, they would all strike against the Faraday cylinder and the disk would not receive any charge. If we measure the charges received by the disk and the Faraday cylinder during any time, the ratio of the charges will be the ratio of the number of ions which strike against the disk to the number striking against the cylinder. The readings of the electroscopes do not give us directly the charges received by the systems to which they are attached, but the potentials to which these systems are raised. We can, however, if we know the ratio of the potentials easily deduce that of the charges. For let E_1, V_1 be the charge and potential of the Faraday cylinder, E_2, V_2 the corresponding quantities for the disk.

Thus if the q 's represent coefficients of capacity, we have

$$\begin{aligned} E_1 &= q_1 V_1 + q_{12} V_2 \\ E_2 &= q_{12} V_1 + q_2 V_2 \end{aligned}$$

so that

$$\frac{E_1}{E_2} = \frac{q_1 V_1 + q_{12} V_2}{q_{12} V_1 + q_2 V_2} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (1)$$

To determine the q 's by experiment we proceed as follows. Given a charge to the disk, the cylinder being insulated and uncharged, then if V_1' and V_2' are respectively the potentials of the cylinder and disk as determined by their electroscopes,

$$q_1 V_1' + q_{12} V_2' = 0, \quad \text{since } E_1 = 0.$$

Thus if α is the ratio of the potential of the cylinder to that of the disk when the cylinder is uncharged,

$$q_1 = -q_{12} \frac{V_2'}{V_1'} = -\frac{q_{12}}{\alpha}.$$

Similarly, if β is the ratio of the potential of the disk to that of the cylinder when the disk is uncharged, we have

$$q_2 = -\frac{q_{12}}{\beta}.$$

Substituting in equation (1) we have

$$\frac{E_1}{E_2} = \frac{\beta (V_1 - \alpha V_2)}{\alpha V_2 - \beta V_1}.$$

The quantities α and β are very easily determined, and from this equation we can deduce the ratio of the charges when we know that of the potentials. By using two electroscopes and determining by means of them the *ratio* of the charges received by the cylinder and disk, we eliminate any irregularities that might arise from variations in the working of the coil used to produce the cathode rays which ionize the gas in the ionization chamber.

If an ion is projected through the tunnel in N along the axis of the tunnel, it will, if there is no magnetic field acting upon it, travel along a straight line and hit the disk. If there is a magnetic field its path, after getting through the hole, will be a circle, since if it is free when once it has got through the hole from any electric force, it will, however, continue to hit the disk until the radius of this circle is less than the radius of the circle passing through the hole, the edge of the disk, and touching at the hole the axis of the tunnel. If d is the distance of the disk below the hole, a the radius of the disk, r the radius of this circle is equal to $\frac{d^2 + a^2}{2a}$. When the radius of the path of the ion is less than this, the ion will give up its charge to the Faraday cylinder; when it is greater than this it will give up its charge to the disk.

If H is the magnetic force acting on the ion, e its charge, m its mass, and r the radius of its circular orbit,

$$H e r = m v,$$

if V is the potential-difference between the gauze and the top of the box

$$V e = \frac{1}{2} m v^2;$$

thus

$$H^2 r^2 \frac{e}{m} = 2 V.$$

Thus when H increases through the value given by the equation

$$H^2 \left(\frac{d^2 + a^2}{2a} \right) \frac{e}{m} = 2 V, \quad . \quad . \quad . \quad . \quad (2)$$

there ought to be a large increase in the ratio of the charge on the Faraday cylinder to that on the disk. If the pencil of ions coming through the hole were indefinitely thin, and if all the ions travelled with the same velocity in the same direction, the transference of the charge from the disk to the cylinder would be quite abrupt. With a magnetic force less than a certain value, all the charge would be on the disk, while with a force greater than this all the charge would be on the cylinder. In my experiments the diameter of the hole, 0.9 mm., was a considerable fraction of the length 1.6 mm. of the tunnel, so that there was a considerable latitude in the direction of propagation of ions through the hole. This has the effect of making the ratio of the charges on the cylinder and disk change much less abruptly than if they were all projected in the same direction, since those ions which are projected towards the side of the cylinder to which they are bent by the magnetic field, will be carried to the side by a smaller magnetic force than those which are projected at right angles to the disk. When the hole is very small, the charge carried by the ions passing through it in a given time is also very small, and the potentials of the disk and cylinder change very slowly. The purpose for which these experiments were made was not so much to get accurate values of e/m for the ions as to find out whether these had masses comparable with the mass of an atom of hydrogen, or of oxygen, etc. The arrangement used was adequate for doing this, and had the advantage of giving a supply of ions which could produce measurable effects in a minute or so, thus avoiding many difficulties as to insulation which crop up when the experiments have to be extended over very much longer periods. Experiments with very much smaller holes are, however, in progress.

The strength of the magnetic field between the poles was determined by comparing the currents induced in a small coil when suddenly withdrawn from the magnetic field with the current obtained by turning an earth inductor through 180° in the earth's magnetic field. When the pole-pieces were 1.15 cm. apart, the magnetic forces H for different currents through the coils of the electromagnet were as follows :—

Current through Electromagnet
in Amperes.

	H.
0.5	1330
1	2570
2.5	4000
2	4900
2.5	5600
3	6000
3.5	6400
4	6660

When, as in our Faraday cylinder, $d = 5$ mm. $e = 2$ mm., the radius of the critical circle $\frac{d^2 + a^2}{2a} = 0.725$ mm., we see by the application of equation (2) that if $e/m = 10^4$, the potential-difference V required to reduce the radius of the orbit to the critical value would, when the currents through the electro-magnets were 1, 2, 3, 4 amperes, be respectively 170, 620, 900, 1100 volts. These are the potential-differences between the gauze and the top of the cylinder N required to send the ions to the plate. The following table gives the charge acquired by the disk when the sum of the charges on the disk and Faraday cylinder was 100 for different strengths of magnetic fields; assuming that all the ions carry the same charge, these numbers represent the percentage of the ions passing through the hole which reach the disk. In the table V is the potential-difference between the gauze and N in volts, i the current through the electro-magnet in amperes, and n the percentage of ions which reach the disk. The gas in the tube was hydrogen.

	$V = 10.$	$V = 20.$	$V = 30.$	$V = 40.$	$V = 50.$	$V = 60.$	$V = 80.$
i	n	n	n	n	n	n	n
1 . .	17	18.4	18	18	18	19	22.8
2 . .	18	23	23	22	22	20	19
3 . .	11	16	22	23	25	26	21
4 . .	8	9	20	22.5	25	27	26

	$V = 100.$	$V = 120.$	$V = 140.$	$V = 160.$	$V = 180.$	$V = 420.$
1 . .	23	25	24	28	31	50.5
2 . .	16	17	22	14	12	12
3 . .	21	19	19	16	9	9
4 . .	25	23	21	18	15	8

On looking at the numbers we see that until the voltage exceeds 160 volts there is no appreciable difference between the number of ions going to the disk when the magnetic field is due to a current of 1 ampere, and when it is due to 2 amperes. We saw, from the preceding calculation, that a voltage of 170 volts would carry ions for which $e/m = 10^4$ to the disk against the electric current, while

it would require about 700 volts to drive them across when the current was 2 amperes, thus the difference which sets in between the results when $\iota = 1$ and $\iota = 2$ at 160 volts indicates the presence of a considerable number of ions for which $e/m = 10^4$.

For voltages between 30 and 160 there is no appreciable difference with currents ranging from 1 to 4 amperes, while for voltages less than 30 there is an appreciable diminution in the number which get to the disk when the current through the electromagnet is raised from 1 to 4 amperes. This indicates that there are some ions which, under a voltage of say 25 volts, are stopped when the magnetic field is that due to 4 amperes, but can get across when it is due to 2 amperes. Since 1100 volts would just drive particles for which $e/m = 10^4$ across the field due to 4 amperes, 25 volts will drive particles for which $e/m = 10^4/(1100/25) = 10^4/44$ across this field. For the field due to 2 amperes 620 volts are required to drive particles for which $e/m = 10^4$, 25 volts will drive particles for which $e/m = 10^4/(620/25) = 10^4/25$. From the preceding results we infer the presence of ions for which e/m is between $10^4/44$ and $10^4/25$. Such ions might be molecules of nitrogen or oxygen due to traces of air in the discharge-tube; these, however, are only a small fraction of the whole number of ions. The pressure of the hydrogen in this case was about 0.003 mm. It is necessary to work at pressures so low that the mean free path of the ion is large compared with the distance d .

Let us now compare the results obtained when the apparatus had been repeatedly filled with oxygen obtained by heating permanganate of potash in a tube fused on to the discharge-tube, running the coil with the gas at the pressure when the discharge passes most easily, and then filling and repumping; the oxygen on its way from the permanganate to the discharge-tube went through a worm immersed in liquid air to free it from any traces of water vapour given off from the permanganate. No hydrogen lines could be detected in the spectrum. The Faraday cylinder had been taken down between this experiment and the preceding one, and slightly altered so that the radius of the critical circle in this case when $d = 4$, $a = 2.5$, is 0.47 cm., hence the potentials required to force ions for which $e/m = 10^4$ across to the disk when currents 1, 2, 3, 4 amperes flow through the coils of the electromagnet are respectively 72, 270, 400, and 480 volts. The pressure of the oxygen was 0.009 mm.

The following (see Table on next page) are the results of the experiments, n as before being the percentage of ions which reach the disk.

The figures in this case are quite different from those for hydrogen. We see that for voltages over 100 the charge on the disk is not appreciably diminished when the current through the electromagnet is raised from 1 up to 4 amperes; this shows that the number of ions with masses comparable with those of a hydrogen atom is too

	V = 10.	V = 15.	V = 20.	V = 25.	V = 30.
ι	n	n	n	n	n
1 . .	75	82	81	81	80
2 . .	56	62	68	72	72
3 . .	36	43	50	58	61
4 . .	28	27	43	50	56

	V = 40.	N = 100.	V = 200.	V = 300.	V = 400.
1 . .	82	81	81	81	81
2 . .	78	80	80	80	81
3 . .	68	78	76	79	80
4 . .	63½	76	79	80	80

small to be detected, for such ions under a field of 100 volts would have been able to make their way to the disk against a magnetic field due to 1 ampere, but not that due to 2 amperes or more; thus if these had been present in any considerable number, the number reaching the disk when $\iota = 1$ would have been appreciably greater than when $\iota = 2$.

The fact that 20 per cent. under these voltages reach the Faraday cylinder is due, I think, to the obliquity of the ions as they come through the hole, and to the diffusion they suffer in passing through the gas. Under the smaller voltages the effect of the magnitude of the magnetic field is very apparent; thus until the voltage is above 20, the majority of them are stopped by a field of 4 amperes, indicating that the mass of the majority of the ions is not greater than $480/20$ or 24 times the mass of an atom of hydrogen. In fact that the majority of the ions have masses comparable with that of the molecule of oxygen, and are not aggregates of several molecules.

Though the preceding list shows that the number of ordinary hydrogen ions in this gas was too small to be detected, yet when the Canalstrahlen produced in a tube in direct connexion with the one in which the ionization occurred were investigated they were found to be well developed, and to give exactly the same value for e/m as when the apparatus had been filled with hydrogen, as in the experiments already discussed.

By measuring the relative numbers of ions carried to the cylinder and the disk by different voltages against a constant magnetic field we can readily estimate the relative amounts of heavy and light ions in the gas. Indeed I think that by using a very small hole in the plate N a very fair analysis of the gas in the ionizing chamber might be made. Thus suppose the magnetic field were that due to 3 amperes through the coils of the electromagnet; then with apparatus of the dimensions used in one of the experiments the ions for which $e/m = 10^4$ would not reach the disk until $V = 900$, while those for

which $e/m = \frac{1}{2}10^4$ would reach it when $V = 450$, those for which $e/m = \frac{1}{16}10^4$ when $V = 56$, those for which $V = \frac{1}{32}10^4$ when $V = 28$. Thus for ions of different atomic weights the stages are well separated, and the relative numbers of the ions of the different kinds could be determined. With the comparatively large hole used in the experiments described above it was quite easy to observe the gradual diminution in the number of the lighter ions, as each dose of oxygen was supplied to the tube and then pumped out. This method of analysis is applicable at pressures far below those at which even spectrum analysis is available.

By reversing the potentials in the ionization chamber we can collect and send through the opening in the plate negative ions and corpuscles which are present in large numbers in the gas. The corpuscles, on account of their small mass, are prevented from reaching either the Faraday cylinder or the disk by a comparatively small magnetic force, and then only negative ions get through to the conductor. The proportion of these reaching the disk and cylinder with changes in the electric and magnetic fields show variations of a similar character to those observed for the positive ions.

The relative rates at which the cylinder or disk charged up according as positive or negative ions were supplied to them from the ionization chamber was determined. When the magnetic field was weak the rate of charging was much more rapid with negative than with positive ions; this was due to the excess of corpuscles in the ionization chamber; when, however, the magnetic field was strong enough to stop the corpuscles, the rate of charging under potential-differences of the order of about 200 volts was about the same for the negative as for the positive ions, while with smaller differences of potential, say 25 volts, the rate of charging with negative ions was only about $\frac{1}{6}$ of that with positive. The positive ions seem, in the ionization chamber, to be moving more rapidly than the negative, for with no electric field in that chamber both cylinder and disk acquire a positive charge when the magnetic field is strong.

With the apparatus we have been describing we can measure simultaneously the values of e/m for the ions and the Canalstrahlen in the same vessel, and the experiments we have described show that we can get a complete change in the character of the ions without any change in the nature of the Canalstrahlen; this is, I think, strong evidence that the particles composing the Canalstrahlen are the same from whatever source they may be derived.

It might, however, be urged that although the tube might be cleared of hydrogen to begin with, this gas might be driven by the discharge out of the cathode and that this might be the source of the Canalstrahlen, and I have noticed a phenomenon which at first sight suggests this view. I have observed that under some conditions there is a lag amounting in some cases to half a minute or so between starting the discharge and the appearance of the phosphor-

escence due to the canal-rays ; this might be explained by supposing that it takes time to liberate sufficient hydrogen to produce appreciable Canalstrahlen. I have made many experiments on this lag and these show that it has no special connexion with hydrogen, but is due to an alteration in the pressure of the gas produced by the discharge. It is well known that the Canalstrahlen are only well developed when the pressure in the tube is between certain limits. It is only when the initial pressure is near to, but outside, one of these limits that the lag occurs, and then the alteration in pressure which occurs when the discharge passes may accumulate until the pressure is brought within the required limits. That this, and not the introduction of hydrogen rather than any other gas, is the explanation of the lag is I think proved in the following experiments. If the presence of hydrogen were all that is wanted for the Canalstrahlen, then the lag should not occur when the tube is filled with hydrogen : we find that the lag occurs when the tube is filled with hydrogen, as well as when great precautions have been taken to remove this gas from the tube. Again, in a tube from which hydrogen has been removed and the lag is well developed, the admission of a small quantity of dry air will remove the lag just as effectively as the admission of hydrogen. When once the lag has been got rid of it is necessary to give the tube a long rest from the discharge before it returns. The fact that the lag may be destroyed by admitting a small quantity of gas shows that it is due to the alteration in pressure and not to a change produced by the discharge in the surface of the electrode. This can also be proved in the following way : two discharge-tubes A and B are connected together and with the pump, and the pressure is adjusted so that both A and B show the lag ; then if the discharge is sent through A until the lag disappears from that tube, it will be found to have simultaneously disappeared from B, though no discharge has been running through this tube.

It is somewhat remarkable that we do not, when the tube is filled with oxygen, get any trace in the Canalstrahlen of particles having masses comparable with those of the ions in oxygen. For though such ions would not be formed in very intense electric fields, there are places in the discharge-tube where the electric field is weak, as, for example, outside the cathode dark space ; we might expect positive ions to be formed in these regions, and then dragged by the electric field up to and through a perforated cathode mingling with the Canalstrahlen. The reason that we get no evidence of these oxygen ions in the Canalstrahlen is, I think, as follows : Let A be a positive ion, B a corpuscle, and let the relative velocity of A and B at the instant under consideration be at right angles to AB and equal to V . Then it is easy to show that A and B will not part company if $\frac{mV^2}{2}$

is less than $\frac{e^2}{AB}$, where m is the mass and e the charge of the cor-

pulse, M the mass of the ion being supposed very large compared with m . Thus if the relative velocity falls below a certain value the ion and the corpuscle will form a neutral doublet and will cease to be a possible constituent of the Canalstrahlen. If the ion is moving much more rapidly than the corpuscle, then V will be the velocity of the ion, and we see that the smaller this velocity the more likely is it to have its charge neutralized. M being the mass of the ion $\frac{1}{2} M V^2 = P e$, where P is the potential-difference moved through by the ion, thus the ion will be neutralized unless $P > \frac{M}{m} \cdot \frac{e}{A B}$. Thus

to protect a heavy ion, for which M_1 is large, from being neutralized it must be subject to a much stronger electric field than would be necessary for a light ion; thus, if there were a mixture of different gases in the discharge-tube, the ions formed from the lighter gases would persist longer than those formed from the heavier ones.

An illustration of this result is furnished by the fact that, as I showed in the paper,* the only ions besides those of hydrogen which can be observed in the Canalstrahlen are those of the next highest gas helium, which, when the discharge passes through helium, can be observed in the Canalstrahlen without difficulty.

The places where the neutralization of the positive ions by the corpuscles takes place will be either quite close to the cathode or when the cathode is perforated in the region behind the cathode; for in front of the cathode where the positive ions are produced, though the velocity of these ions will be small, since they are in a feeble electric field, yet the corpuscles which have come from the cathode will have passed through a great potential-difference and will have a very high velocity; thus the relative velocity of the positive ions and the corpuscles will be very large. Quite close to the cathode the velocity of the corpuscles will be very small, and though the velocity of the ion will be much greater than in the former case, yet since the mass of the ion is so much greater than that of the corpuscle, the velocity acquired by the ion under the same potential-difference will be small compared with that acquired by the corpuscle, so that the relative velocity of the two close to the cathode will be much less than at a greater distance in front of it, so that combination is much more likely to occur near to the cathode, or if the cathode is perforated behind it.

If the forces between a small positive ion and an uncharged molecule are independent of the atomic weight of the molecule, or only increase slowly as the atomic weight increases, then such an ion is more likely to attach itself to a light molecule than to a heavy one; for we can show that the condition that the ion and the molecule should separate is that

$$\frac{M M' V^2}{M + M'}$$

* Phil. Mag. ser. 5, xiii. p. 561.

should be greater than a certain quantity depending only on the force between the systems and their distance apart when nearest together. M is the mass of the ion, M' that of the molecule, and V the relative velocity of the two at the absidal distance. If M' is very large compared with M the condition is that Mv^2 should be greater, while if M' were equal to M , the condition is that $\frac{1}{2}Mv^2$ should be greater than the same quantity; thus in the second case the ion would take twice as much energy to get free as in the first, and so would be more likely to combine with the molecule.

Nature of Ionization by Cathode Rays.

If, as seems most probable, the positively charged particles are produced from the ionization of the gas by cathode rays, the study of the processes by which this ionization is accomplished may be expected to throw considerable light on the nature of the positive rays. When a gas is ionized by cathode rays, secondary cathode rays are generated, and the author has recently shown* that the maximum velocity of the secondary rays is independent of the velocity of the primary rays. A comparison of the velocity of the secondary rays from gases, as determined in my experiments, with those from metals, measured by Füchtbauer† shows that there is not much difference between the two. The velocity of the rays from gases was that due to a potential-difference of 40 volts, of those from metals that due to a potential-difference of 33 volts; the difference between these results is not greater than could be explained by errors of experiment. Thus, as far as our present knowledge goes, the velocity of a secondary cathode ray is independent both of the velocity of the primary ray and varies but little with the nature of the molecule from which the secondary ray is projected. The first result shows that the energy of the secondary ray is not acquired by a corpuscle in the primary rays striking against one in a molecule of a gas and imparting to it sufficient energy to force it out of the molecule, for if this were the case we should expect the energy of the secondary ray to vary quickly with that of the primary. Neither does it seem likely that the energy in the secondary ray is due to a general explosion of the molecule of the gas produced by a gradual accumulation of energy in the molecule from impacts with the primary rays, for then we should expect the energy in the secondary rays to depend largely on the chemical nature of the molecules.

As a working hypothesis to account for these very striking properties of the secondary rays, I would suggest that perhaps the first stage is ionization by cathode rays, may be the separation from the molecule, not of a single corpuscle, but of an electrically neutral

* Proc. Camb. Phil. Soc. xiv. p. 540, 1908.

† Phys. Zeits. vii. p. 748, 1908.

doublet consisting of a negatively electrified corpuscle in rapid rotation round a much more massive particle with a positive charge, and that these doublets may be the same from whatever molecule they may be ejected. The secondary cathode rays are due to the subsequent breaking up of this doublet, their energy being the kinetic energy possessed by the corpuscle when rotating round the positive charge. This hypothesis would also explain the constancy of e/m in the Canalstrahlen produced from different gases.

There are many ways in which the doublet might get broken up after it had escaped from the molecule. Thus, for example, if another corpuscle, which we shall call for brevity a comet, were under the attraction of the positive particle to describe an orbit such as that shown in Fig. 7, then when the comet was in the

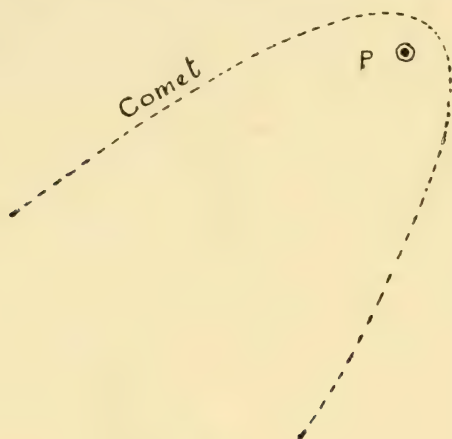


FIG. 7.

immediate neighbourhood of the positive particle, it would neutralize the attraction of this particle on the corpuscle in the doublet; thus this will move off with undiminished velocity along a straight line, and when the comet has left the system, will, if not free, be at any rate further from the positive particle than it was before, and still possessed of its original kinetic energy; if it did not get free under the influence of the first comet, a repetition of the process by other comets might liberate it from the doublet. The same effect might be produced if the positive part of the doublet came close to a gaseous molecule, which behaved like a conductor of electricity; the negative charges induced on the conductor would balance the attraction of the positive particle on the corpuscle in the doublet, and just as in the previous case, the corpuscle would be able to get off with undiminished velocity.

The questions now arise, can we get any experimental evidence of the existence of these doublets, and is it possible that such systems, if they existed, could have escaped the careful scrutiny which has been given? The second question is more easily answered than the first, for these doublets being uncharged would not possess the properties which make the positive rays or the cathode rays so noticeable; thus they would not be deflected by uniform magnetic or electric fields, and the absence of the charge might involve also a loss of the power of producing luminosity when they pass through a gas, and thus render them invisible. With regard to the first question I have made some preliminary experiments, the results of which suggest the existence in the neighbourhood of cathode of neutral systems, such as the doublets which dissociate into corpuscles and positive ions. The arrangement used in these experiments is represented in Fig. 8.

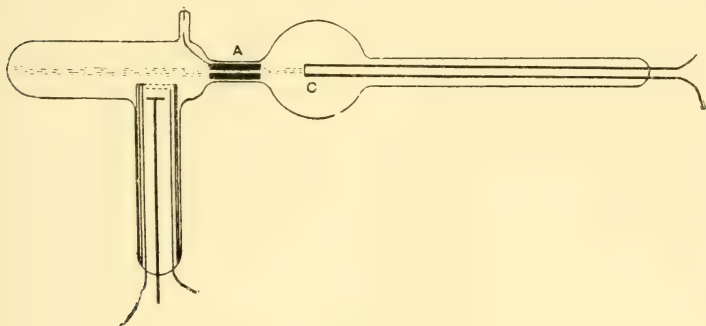


FIG. 8.

The idea of the experiment was as follows. If the secondary cathode rays are produced from the primary without the intervention of the neutral doublet stage, then, as the secondary ionization is due to the secondary cathode rays, a strong electric field, arranged so as to stop the negative corpuscles forming the secondary cathode rays, ought to act as a complete screen against this ionization. If, on the other hand, there is an intermediate stage between the primary and secondary rays, and if this stage consists of neutral doublets, then some of these ought to be able to get through the strong electric field, if this is quite close to the primary rays, because it is only those secondary rays which are produced from the doublets whilst the latter are passing through the field which would be stopped; the doublets themselves will not be stopped, and if they last long enough to get through the field they ought to give rise to ionization on the other side. To test this view the apparatus represented in Fig. 8 was used. A copious supply of slowly moving primary cathode rays

was produced from the hot Wehnelt cathode C, these passed through a hole in the anode A, the anode was earthed, the primary rays passed over the top of the side tube, T; across the top of the tube were stretched two parallel pieces of wire gauze about a millimetre apart: the upper gauze was earthed, the other could be charged negatively by connecting it with the negative terminal of a battery of small storage-cells, the positive terminal of which was earthed. When the lower gauze was earthed as well as the upper, the tube was filled with the glow due to the secondary rays. When the lower gauze was charged to a negative potential of about 40 volts, this glow became exceedingly faint; but that the gas below the gauze was ionized was shown by the fact that when the negative potential of the lower gauze was increased to about 200 volts, a potential quite insufficient to produce luminosity in an unionized gas, the tube again became full of luminous glow. Thus something capable of ionizing the gas was able to traverse the strong electric field. There are two sources of ionization which have to be eliminated before we can assign this ionization to the existence of neutral systems traversing the electric field—the ultra-violet light coming from the luminous discharge in the main tube, and soft Röntgen rays produced by the slowly moving primary cathode rays. To test whether it was due to ultra-violet, a thin plate of quartz was placed over the top of the upper gauze: with this arrangement no luminosity could be detected in the side tube under the conditions as to potential and so on which gave bright luminosity in the tube when the quartz was absent. Hence I conclude that the luminosity was not due to ultra-violet light. To test whether it was due to soft Röntgen rays, taking the quartz away, I got a bright luminosity in the side tube with the primary rays passing horizontally down the tube, then by means of a magnet I bent the primary rays so that they struck the glass of the tube just above the side tube, the path of the rays being represented by the dotted line of the figure. This made the rays themselves further from the side tube, but brought the places where they struck the glass, the sources of the Röntgen rays, much nearer to that tube: so that if the ionization in the side tube were due to Röntgen rays it should be increased by the introduction of the magnet, while if it were due to the neutral doublets it would be diminished. As a matter of fact the luminosity in the side tube almost disappeared when the rays were deflected in this way, showing that it was not due to Röntgen rays, while the effect is what we should expect if the ionization were due to uncharged systems.

In the preceding experiments there is the possibility that the ionization might arise in some such way as the following. The secondary cathode rays would have to penetrate some way between the two pieces of gauze before they were stopped, and if they collided against the molecules of the gas they might ionize it: the positive ions so produced would, under the action of the electric field between

the pieces of gauze, acquire considerable kinetic energy when they reached the lower gauze, they would travel some distance after passing through before they were stopped and brought back to the gauze, and would thus have an opportunity of ionizing the gas below the gauze by collision. The negative corpuscles produced in this way would be repelled from the lower gauze and might acquire sufficient energy to produce fresh ions by collisions, and thus give rise to the luminosity observed below the gauze. To eliminate this source of ionization, a strong magnetic field was used to prevent any of the secondary cathode rays from straying into a region where they could affect the ionization in the region under observation. Two arrangements were used for this purpose. In the first, the tube with the hot line cathode (Fig. 8) was used. The primary cathode rays were coiled up into a small bunch by means of a strong electromagnet placed just under the tube, from which the cathode rays emerge, the cathode rays in the early part of their path were screened from the effect of the magnetic force by thick iron plates. The magnetic force was strong enough to prevent the primary cathode rays, which were produced under a potential-difference of about 250 volts, from travelling more than 2 or 3 millimetres across the lines of force. The path of the rays when not under the influence of magnetic force never approached within this distance of the two pieces of gauze, and the deflexion of these rays by the magnet was away from the gauze. No luminosity could be seen close to the gauze next to the discharge-tube. Nevertheless, when the lower gauze N was at a potential of about 200 volts, the upper gauze being earthed, there was a perceptible luminous discharge in the side tube, showing that in spite of the strong magnetic field something must have passed across the gauze and ionized the gas in the side tube. A modification of this experiment was tried, in which the two pieces of gauze were connected together and with the earth, and an insulated plate connected with a charged electroscope was placed in the side tube at some distance from the gauze; the ionization in the side tube produced a leak of the electroscope. It was found that even when the primary cathode particles were coiled up by the strong magnetic field into a small bundle at the mouth of the tube from which they emerge, there was a rapid leak of the electroscope showing that the gas in the side tube was ionized. The leak was more rapid when the electroscope was positively than when it was negatively charged.

A somewhat similar experiment was also tried with the apparatus represented in Fig. 6. A magnetic field of 1200 was established between the pole-pieces, and the plates L, M, N connected with the earth, so that there was no electric field in the ionizing vessel. Under these circumstances neither the cylinder nor the disk received any electric charge when the electric discharge passed through the upper tube. The Faraday cylinder was then disconnected from the

electroscope and charged up positively to about 40 volts; the disk now acquired a positive charge, when the cylinder was charged to - 40 volts the disk got a negative charge. This shows that the gas between the cylinder and disk was ionized, though the magnetic field prevented any negative corpuscles from entering this region.

Though we have given reasons for thinking that the Röntgen rays are not the cause of the ionization in the side tube when this is exposed to strong magnetic fields, soft Röntgen rays are produced by the impact of the primary cathode rays against the molecules of the gas in the tube. This was proved by covering the end of the side tube (Fig. 8) with thin aluminium foil and placing in the side tube behind the foil an insulated metal plate connected with a charged electroscope. The escape of electricity from this plate could not be ascribed to ionized gas making its way from the main tube into the side one, for the only channel of communication was through a long stretch of glass tubing from the main tube to the pump, and then through another long tube from the pump to the side tube; since the opening between the main tube and the side tube was closed, it was necessary to exhaust them separately. When the primary and secondary cathode rays were well developed and the main tube filled with a bright glow, the charge from the electroscope rapidly leaked away whether it were positive or negative. The gas in the side tube is thus ionized by rays which have passed through the thin aluminium foil. The leak was, however, completely stopped when, by means of a strong magnetic field, the primary and secondary cathode rays were rolled up into a small bundle at the mouth of the tube, from which they emerge just above the aluminium foil. In this case the length of the path of the rays after coming through the tube was only 2 or 3 mm., and there was hardly any luminosity in the tube. The aluminium foil prevents the ionization in the side tube in this case, for if the foil is removed the gas, as we have already stated, is ionized.

The preceding experiments are in harmony with the view that neutral doublets are one of the stages in the process of ionization; they must, however, be regarded as only preliminary. More extended experiments are necessary before we can be certain that the effects are not due to some very easily absorbed kind of radiation or to the diffusion of very slowly moving ions.

We have hitherto considered the case when the primary ionization was due to cathode rays, but there are reasons for thinking that similar doublets are produced when the ionization is produced by positive rays. Thus Füchtbauer* found that the velocity of the secondary cathode rays from metals was the same whether they were produced by cathode rays or Canalstrahlen. It is sometimes argued that the much greater difficulty experienced in saturating a gas

* Loc. cit.

ionized by α particles than one ionized in any other way, shows that results of ionization are different in the two cases: this result is, however, exactly what we should expect if there were no such difference. For when a gas is ionized by α rays, each α particle produces an enormous number of ions, but there are comparatively few particles, and these are widely separated. Thus in a gas ionized by α rays we have intense ionization in some localities and very weak ionization in others, while other methods give much more uniform ionization. Now the electric force required to saturate a gas depends upon the maximum density of ionization as well as upon the average; thus it will require a more intense field to saturate a gas ionized by α particles than a gas where the total ionization is the same but the ionization is uniformly distributed through the gas. The researches of Moulin* on the ionization by α rays, show that the differences between this kind of ionization and others can be explained as arising from the want of uniformity in the distribution of the α ionization.

I have much pleasure in thanking Mr. E. Everett and Mr. G. W. C. Kaye for the assistance they have given me in this investigation.

[J. J. T.]

* *Le Radium*, t. v. March 1908.

ANNUAL MEETING,

Friday, May 1, 1908.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the chair. :

The Annual Report of the Committee of Visitors for the year 1907, testifying to the continued prosperity and efficient management of the Institution, was read and adopted, and the Report on the Davy Faraday Research Laboratory of the Royal Institution, which accompanied it, was also read.

Forty-one new Members were elected in 1907.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1907.

The Books and Pamphlets presented in 1907 amounted to about 203 volumes, making, with 693 volumes (including Periodicals bound) purchased by the Managers, a total of 896 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year :—

PRESIDENT—The Duke of Northumberland, K.G., P.C., D.C.L. F.R.S.

TREASURER—Sir James Crichton-Browne, M.D. LL.D. F.R.S.

SECRETARY—Sir William Crookes, D.Sc. F.R.S.

MANAGERS.

Sir Thomas Barlow, Bart., K.C.V.O.
M.D. LL.D. D.Sc.

Sir George Darwin, K.C.B. M.A. LL.D.
D.Sc. F.R.S.

The Right Hon. The Earl of Halsbury,
P.C. M.A. D.C.L. F.R.S.

W. A. B. Burdett-Coutts, Esq., M.P. M.A.

Charles Hawksley, Esq., M.Inst.C.E.

Donald William Charles Hood, M.D. C.V.O.
F.R.C.P.

Rudolph Messel, Esq., Ph.D. F.C.S.

Henry Francis Makins, Esq., F.R.G.S.

George Matthey, Esq., F.R.S.

Ludwig Mond, Esq., Ph.D. D.Sc. F.R.S.

The Right Hon. Sir John Fletcher Moulton,
P.C. M.A. F.R.S.

Alexander Siemens, Esq. M.Inst.C.E.

The Right Hon. Sir James Stirling, P.C.
M.A. LL.D. F.R.S.

The Right Hon. The Earl of Rosse, K.P.
D.C.L. LL.D. D.Sc. F.R.S.

MANAGERS—continued.

Sir William H. White, K.C.B. LL.D. D.Sc.
F.R.S.

VISITORS.

Arthur N. Butt, Esq., F.R.Hist.S.

Dugald Clerk, Esq., M.Inst.C.E. F.C.S.

Charles A. Ballance, Esq., M.V.O. M.S.
F.R.C.S.

John B. Broun-Morison, Esq., J.P. D.L.
F.S.A.

Edward Dent, Esq., M.A.

John Cameron Graham, Esq., K.C. J.P.

James Dundas Grant, Esq., M.D. M.A.
F.R.C.S.

Charles Edward Groves, Esq. F.R.S.

Sir Henry Harben.

Major E. H. Hills, C.M.G. R.E. F.C.S.

John List, Esq., M.Inst.C.E.

Robert Mond, Esq., M.A. F.R.S.E.

Francis Lys Smith, Esq.

James Swinburne, Esq., F.R.S.

Lieut.-Colonel Sir Frederick Nathan, R.A.

WEEKLY EVENING MEETING,

Friday, May 1, 1908.

The RIGHT HON. LORD RAYLEIGH, O.M. P.C. D.C.L. LL.D.
D.Sc. Pres.R.S., in the Chair.

PROFESSOR JOSEPH LARMOR, M.A. D.C.L. LL.D. D.Sc. Sec.R.S.

The Scientific Work of William Thomson, Lord Kelvin.

[THE material which constituted this discourse was subsequently worked up into the obituary notice of Lord Kelvin, published by the Royal Society ('Proceedings,' No. A 543, vol. 81 A, pp. i.-lxxvi.). From that source the greater part of the present report is taken textually.

The lecture was illustrated by a very complete historical collection of Lord Kelvin's scientific apparatus, utilised in part for experiments, and in part as an exhibition in the Library. This was rendered possible through the cordial co-operation of the University of Glasgow and Prof. A. Gray, F.R.S., of the firm of Messrs. Kelvin and James White, Ltd., and Dr. J. T. Bottomley, F.R.S., of the authorities of the South Kensington Museum, and other contributors.]

It would be impossible in a review of ordinary length to convey any idea of the many-sided activity by which Lord Kelvin was continually transforming physical knowledge, through more than two generations, more especially in the earlier period before practical engineering engrossed much of his attention in importunate problems which only he could solve. It is not until one tries to arrange his scattered work into the different years and periods, that the intensity of his creative force is fully realised, and some notion is acquired of what a happy strenuous career his must have been in early days, with new discoveries and new aspects of knowledge crowding in upon him faster than he could express them to the world.

The general impression left on one's mind by a connected survey of his work is overwhelming. The instinct of his own country and of the civilised world in assigning to him a unique place among the intellectual forces of the last century, was not mistaken. Other men have been as great in some special department of physical science: no one since Newton—hardly even Faraday, whose limitation was in a sense his strength—has exerted such a masterful influence over its whole domain. He might have been a more learned mathematician or an expert chemist; but he would then probably have been a less

effective discoverer. His power lay more in the direct scrutiny of physical activity, the immediate grasp of connecting principles and relations; each subject that he tackled was transformed by direct hints and analogies, brought to bear from profound contemplation of the related domains of knowledge. In the first half of his life, fundamental results arrived in such volume as often to leave behind all chance of effective development. In the midst of such accumulations he became a bad expositor; it is only by tracing his activity up and down through its fragmentary published records, and thus obtaining a consecutive view of his occupation, that a just idea of the vistas continually opening upon him may be reached. Nowhere is the supremacy of intellect more impressively illustrated. One is at times almost tempted to wish that the electric cabling of the Atlantic, his popularly best known achievement, as it was one of the most strenuous, had never been undertaken by him; nor even, perhaps, the practical settlement of electric units and instruments and methods to which it led on, thus leaving the ground largely prepared for the modern refined electric transformation of general engineering. In the absence of such pressing and absorbing distractions, what might the world not have received during the years of his prime in new discoveries and explorations among the inner processes of nature?

His scientific papers, mostly mere fragments, which overflowed from his mind, as has been said, into the nearest channel of publication, have been collected by himself up to the year 1860, in somewhat desultory manner, in four substantial volumes. In addition there are three volumes of Popular Lectures and Addresses, which are more finished products, perhaps equalled in weight and scope only by those of Helmholtz. His fertility, especially in the first dozen years from 1845 to 1856, seems to be almost without precedent. Owing to the want of systematic exposition, much of this progress was grasped only imperfectly by contemporaries, and even long afterwards; but the close attention of a few master minds, including Clerk Maxwell, and in a less degree Helmholtz, and in certain respects that of the school of scientific electrical engineers that was rising into confident power under his own inspiration, made up partially for this failure. In the writings on Thermodynamics and the Theory of Available Energy, this lack of consecutive arrangement has remained until the present time a serious obstacle. In the notice* of the first two volumes of the 'Collected Papers,' which was contributed to 'Nature' in 1885 by Helmholtz, the writer was so engrossed by this interesting episode as to devote nearly the whole review to its consideration; but even he has missed recognising that Thomson's "dissipation of energy" was in 1855 determined quantitatively just as much as Clausius' "entropy" was in the same month of the same year, and was, moreover, even then as wide in scope, making due allowance for the almost total absence

* *Nature*, xxxii. (1885), pp. 25-7; Helmholtz's Papers, iii. p. 593.

of numerically exact physico-chemical data on which to develop it, as it had again become twenty years later in Helmholtz's own hands in 1882, or in those of Willard Gibbs in 1876-8.

Probably the severest ordeal to which a mass of occasional writings, evolving an entirely new range of thought, could be subjected, is that of republication after the lapse of years. The fragmentary character of the production of Thomson's papers, in scattered Journals and Transactions, naturally suggested ideas of obscurity to the workers who had time only to skim the contents of separate papers without absorbing them as a connected whole; but it will probably be granted to be a most remarkable circumstance, and irrefragable proof of sureness of construction in a subject so difficult and entangled, that the papers on Thermodynamics, which also founded the modern general Theory of Energy, were capable of being reprinted in full with but slight occasional erasures, and those mainly of unessential character. Here one is, of course, leaving out of account the preliminary struggle to reconcile the apparently conflicting principles of Carnot and Joule, which forms one of the most instructive and fascinating episodes in scientific history.

We may be permitted to surmise that it was in the keen insight of these early years that his mental habitudes became fixed. His most striking characteristic all through life was insatiable thirst for knowledge, unwearied inquiry and investigation at all times, in season and out of season, combined with sympathetic interest and charming deference and encouragement to any person, however junior, who was honestly bent on the same pursuits. It is not surprising that, with new and profound views breaking in upon him from all sides, it should have grown into settled permanent habit that no mode of occupation of his time was to be allowed to interfere with the claims of scientific investigation.

Already when he took his degree at Cambridge in the Mathematical Tripos in January 1845, it appears that many subjects closely connected with fundamental advances of the ensuing time were fermenting in his mind. It was only a few months afterwards that he at length, after years of search, discovered for the scientific world Green's 'Essay on Electricity' of 1828, ever since one of the classics of mathematical physics; he obtained, in fact by accident, a copy from his previous mathematical tutor, W. Hopkins, when he recognised how much of it he had anticipated by his own more intuitive results when still a boy. Soon afterwards he went to Paris to learn physical manipulation in the laboratory of Regnault—a fact which seems to have been forgotten when he recalled, in graceful terms, his obligations to the French science of his youth, in an address in connection with the celebration of the centenary of the Institute of France, of which the echoes vibrated through Paris. He has put on record that already even at that time, he went about among the Paris booksellers inquiring for a copy of another work of genius, which he was himself

to enroll among the few supreme classics of scientific knowledge, Sadi Carnot's small tract of 1824, '*Réflexions sur la Puissance Motrice du Feu*'; he found in 1845 that it was quite forgotten, though they knew in the book-shops of the social and political writings of his brother Hippolyte Carnot, ultimately his editor and biographer (1878) in later years.

If one had to specify a single department of activity to justify Lord Kelvin's fame, it would probably be his work in connection with the establishment of the science of Energy, in the widest sense in which it is the most far-reaching construction of the last century in physical science. This doctrine has not only furnished a standard of industrial values which has enabled mechanical power in all its ramifications, however recondite its sources may be, to be measured with scientific precision as a commercial asset; it has also, in its other aspect of the continual dissipation of available energy, created the doctrine of inorganic evolution and changed our conceptions of the material universe. A sketch of the early history of this doctrine will illustrate the innate power and independence of Lord Kelvin's thought, as well as in some degree his relations to his great predecessors and contemporaries.

The initial difficulty of the subject lay in the feature, entirely novel to physical science, that in the inorganic world what we call dissipation or scattering of energy is loss only in a subjective sense; it concerns only the energy "*available to man*, for the production of mechanical effect," to use Thomson's own phrase of 1852.* We can produce organised mechanical effect from diffuse energy such as heat, which consists in the unregulated motion of a crowd of jostling molecules, only by judicious guiding of its innate effort towards an equilibrium, just as we can get power from a turbulent waterfall by guiding the stream against a mill-wheel or turbine. But when the average of the molecular motions has come to a steady equilibrium throughout all parts of the material system, of which uniformity of temperature is the criterion, all chance of arranging or guiding part of its molecular energy into co-ordinated power available for our operations on finite bodies has passed away. This is, roughly, the *rationale* of the principle of Carnot. Yet the energy has not disappeared; it is still there, but it is uniformly diffused and so not recoverable into the organised form of mechanical power. This absolute conservation of the total energy is the principle of Joule, which is the main experimental support of the presumption that all energy is ultimately of the dynamical type. In a complete view of physical transformations the two principles, of Carnot and of Joule, have both to find their places. Here a fundamental perplexity confronted and detained Lord Kelvin for some three years, 1847-50.

* Math. and Phys. Papers, i. p. 505.

When heat is allowed to flow away to a lower temperature without passing through an engine, its capacity for doing work has been dissipated. The opportunity for obtaining mechanical power from it has vanished beyond recall. Can then heat be correctly measurable as mechanical energy if some of the mechanical energy is lost irrecoverably every time that the heat diffuses to a lower temperature? Thomson, ever attracted by the engineering side of things, was dominated by Carnot's principle, as we have seen, even when as a youth in 1845 he went to Paris to Regnault's laboratory. Thus he at once set himself to explore its practical content by the aid of the mass of exact data on gases acquired by Regnault, as soon as these results appeared, in 1847, as the first instalment of the famous series of experimental researches, which had been subsidised by the French Government with a view to obtaining all the data that could be pertinent towards the improvement of knowledge of the principles of steam and gas engines. In Thomson's first paper towards this end, entitled 'On an Absolute Thermometric Scale founded on Carnot's Theory of the Motive Power of Heat, and calculated from Regnault's Observations,' he clears the ground for exact physical reasoning by elevating the idea of temperature from a mere featureless record of comparison of thermometers into a general principle of physical nature, making it a measure of the dynamical potentiality of heat, which is, on Carnot's principles, an intrinsic measure, i.e. quite independent of the substances in which the heat happens to be contained. But he cannot get rid of the impression that heat is something different from energy, which may produce energy in falling to a lower level of temperature, or on the other hand may diffuse passively so that this opportunity of creating energy is irrecoverably wasted. Such a view would tend towards the caloric theory which held that heat is somehow substantial; in terms of it Carnot, in fact, formulated his arguments. It has been remarked on this by Helmholtz that if Carnot had then possessed completer knowledge he would possibly never have hit upon his principle; on the other hand, his rough manuscripts, published many years after, have revealed that during the remaining six years of his short life he was inclining strongly towards the correct view on the nature of heat. In a footnote, Thomson gives expression to his own doubt. The experiments of "Mr. Joule, of Manchester," seem "to indicate an actual conversion of mechanical effect into caloric. No experiment, however, is adduced in which the converse operation is exhibited; but it must be confessed that as yet much is involved in mystery with reference to these fundamental questions of Natural Philosophy." And in a fuller account, soon after, of Carnot's Theory,* as further developed numerically by aid of the data given by Regnault's experiments on steam, he adheres substantially to this position, "although this, and

* Trans. R. S. Edinburgh, January 2, 1849.

with it every other branch of the Theory of Heat, may ultimately require to be reconstructed upon another foundation when our experimental data are more complete." He returns, in a note, stimulated by a remark of Joule, to the problem of what becomes of the mechanical effect that appears to be lost when heat diffuses; but he cannot admit the suggestion of Joule to cut the knot by abandoning Carnot's principle, and he appeals to further experiment "either for a verification of Carnot's axiom, and an explanation of the difficulty we have been considering: or for an entirely new basis for the Theory of Heat." Still harassed by these doubts, he returns yet again to test the experimental verification of Carnot's principle (which he finds adequate) in an Appendix; * for, as he says, "Nothing in the whole range of Natural Philosophy is more remarkable than the establishment of general laws by such a process of reasoning" as is that principle in its wider ramifications.

We have here found Thomson actually hesitating as to whether heat is to be classified as energy, on the ground that the fall of heat to a lower temperature can occur without developing any mechanical work. Yet it is true, as Lord Rayleigh has expressed it, † that most great authorities, especially in England, including Newton, Cavendish, Rumford, Young, Davy, etc., have always been in favour of the doctrine that heat is a mode of motion. The fact is, as we have seen, that Thomson knew too much to allow him to rest in such a partial view of things; he saw also a totally different side of the subject, which not even his close connection with Joule and appreciation of his work, could allow him merely to ignore.

Just a year before Thomson's first paper on Carnot's principle, Helmholtz, then a young army surgeon, had stepped (1847) into the first rank of physicists (though recognition came later, the memoir, e.g., becoming known to Thomson only in 1852) by the publication of the '*Erhaltung der Kraft*,' which asserted the universality of the conservation of total energy, and developed with convincing terseness and lucidity the ramifications of that principle throughout nature. To establish the transformation of heat into work he is already able to appeal to the classical experiments of Joule, published three years previously (1844)—not yet mentioned by Thomson, whether it was from want of knowledge or from some fancied mode of evading their force in the light of his insistence on Carnot's principle. These experiments proved definitely that expansion of a gas working against the pressure of the atmosphere absorbs an equivalent of heat, whereas expansion into a vacuum absorbs none. It was, in fact, in this paper that Joule rather summarily condemned Carnot's principle as above mentioned, on account of its supposed discrepancy with his own established results. And Helmholtz had naturally to consider this

* April 30, 1849.

† The Scientific Work of Tyndall, Roy. Inst. Proc., Vol. xiv. p. 218.

point. He seems to have had access then only to Clapeyron's account of Carnot, of date 1843, from which, however, he expounds the argument succinctly and correctly. He admits the probability of the truth of Clapeyron's deductions for gases, but falls back on the suggestion that they may also be obtainable otherwise on more certain principles; while he characterises as very unlikely the (correct) inference that compression of water between its point of maximum density and the freezing-point would absorb heat. Thus Helmholtz,* contrary to Thomson, saves the conservation of total energy by abandoning and ignoring the ideas belonging to the principle of Carnot.

The brilliant and suggestive writings of J. R. Mayer on the conservation of total energy were at that time unknown to Helmholtz: they seem to have been first brought to general notice† by Joule himself in the classical memoir on the Mechanical Equivalent of Heat presented by Faraday to the Royal Society in 1849. The sketch above given will have shown how little such theoretical considerations as those of Mayer, however illuminating and acute within their own range, were calculated to remove the profounder perplexities of Thomson, so long as there remained the apparently essential contradiction on which these doubts had their foundation. His insistence in class lectures on the absolute necessity for Joule's experimental work is still recalled by his students.

The credit of being the first to resolve these difficulties belongs to Clausius. In his memoir 'On the Motive Power of Heat and the Laws of Heat which may be deduced therefrom,' communicated to the Berlin Academy in February 1850, he quotes the title of Carnot's tract (Paris, 1824) in a footnote at the beginning‡ of the paper, which proceeds as follows: "I have not been able to procure a copy of this work: I know it solely through the writings of Clapeyron and Thomson, from which latter are taken the passages hereafter cited." Then, in the introductory section, after referring to the difficulties above discussed, and the work of Holtzmann, Mayer and Joule, he continues:—

"The difference between the two ways of regarding the subject has been seized with much greater clearness by W. Thomson, who has applied the recent investigations of Regnault, on the tension and latent heat of steam, to the completing of the memoir of Carnot.§ Thomson mentions distinctly the obstacles which lie in the way of an unconditional acceptance of Carnot's theory, referring particularly to the investigations of Joule, and dwelling on one principal objection to which the theory is liable. If it be even granted that the produc-

* *Wissenschaftliche Abhandlungen*, 1. p. 38.

† Osborne Reynolds, *Life of J. P. Joule* (Manchester Memoirs, vi.), p. 133.

‡ The quotations are here printed from Hirst's translation, in which this memoir occupies pp. 14-68.

§ *Trans. R. S. Edinburgh*, xvi.

tion of work, where the body in action remains in the same state after the production as before, is in all cases accompanied by a transmission of heat from a warm body to a cold one, it does not follow that by every such transmission work is produced, for the heat may be carried over by simple conduction ; and in all such cases, if the transmission alone were the true equivalent of the work performed, an absolute loss of mechanical force must take place in nature, which is hardly conceivable. Notwithstanding this, however, he arrives at the conclusion that in the present state of science the principle assumed by Carnot is the most probable foundation for an investigation on the moving force of heat. He says : ‘ If we forsake this principle, we stumble immediately on innumerable other difficulties, which, without further experimental investigations, and an entirely new erection of the theory of heat, are altogether insurmountable.’

“ I believe, nevertheless, that we ought not to suffer ourselves to be daunted by these difficulties ; but that, on the contrary, we must look steadfastly into this theory which calls heat a motion, as in this way alone can we arrive at the means of establishing it or refuting it. Besides this, I do not imagine that the difficulties are so great as Thomson considers them to be ; for although a certain alteration in our way of regarding the subject is necessary, still I find that this is in no case contradicted by *proved facts*. It is not even requisite to cast the theory of Carnot overboard ; a thing difficult to be resolved upon, inasmuch as experience to a certain extent has shown a surprising coincidence therewith. On a nearer view of the case, we find that the new theory is opposed, not to the real fundamental principle of Carnot but to the addition ‘ no heat is lost ’ ; for it is quite possible that in the production of work both may take place at the same time ; a certain portion of heat may be consumed, and a further portion transmitted from a warm body to a cold one ; and both portions may stand in a certain definite relation to the quantity of work produced. This will be made plainer as we proceed ; and it will be moreover shown that the inferences to be drawn from both assumptions may not only exist together, but that they mutually support each other.”

This memoir, as Willard Gibbs justly claims in his obituary notice (1889) of Clausius, laid securely the foundations of modern thermodynamics. But it seems equally true that this high merit lies mainly in the single remark at the end of the passage just quoted, which resolved the difficulties that had stopped Thomson ; after that the development, though luminously accomplished, would have been plain sailing to any first-class intellect. Thomson’s great memoir ‘ On the Dynamical Theory of Heat,’* in which he at once connects Clausius’ name with that of Carnot, appeared the following year. After giving a demonstration of the principle of “ Carnot and Clausius ” (§ 13), he proceeds (§ 14) to say that, about a year before,

* Trans. R. S. Edinburgh, March 1851.

he had adopted this principle in connection with Joule's principle, notwithstanding that he could not then resolve the apparent discrepancy, as the basis of a practical investigation of the motive power of heat in air and steam engines. "It was not until the commencement of the present year that I found the demonstration given above. . . . It is with no wish to claim priority that I make these statements, as the merit of first establishing the proposition upon correct principles is entirely due to Clausius, who published his demonstration of it in the month of May last year, in the second part of his paper on the motive power of heat. I may be allowed to add that I have given the demonstration exactly as it occurred to me before I knew that Clausius had either enunciated or demonstrated the proposition. . . . The reasoning in each demonstration is strictly analogous to that which Carnot originally gave."

Once Thomson gets thus under weigh, as we have seen, by his own unaided efforts though anticipated by Clausius, he develops rapidly the thermal aspects of the subject, concurrently with Clausius and Rankine, but with wider generality, in particular avoiding their hypotheses connected with perfect gases. So little was he prepared to trust to a permanent gas thermometer as giving practically the intrinsic dynamical scale of temperature, that the following year he had already begun with Joule their series of laborious joint experiments to determine exactly how much the gas thermometer differs from the absolute scale. Their procedure was to deduce the result sought from observation of the slight cooling or heating produced by driving the gas under high pressure through a porous partition; with a perfect gas the process would be isothermal. When we consider that the results were to lead straight into the very core of molecular dynamics, the investigation may well rank to this day as one of the most striking advances in the record of physical science. It is noteworthy that Thomson in his own work kept on with the symbol for the unknown Carnot's function, until the dynamical scale had thus been experimentally investigated; though a gas thermometer was doubtless adequate to give to Clausius and Rankine indications of absolute temperature, so far as required for their preliminary approximate investigations over limited range. We have only to think of the modern physical undertakings steadily pushed downward toward the absolute zero of temperature, to realise that, except on the basis of Thomson's dynamical scale of 1847 and his method conjointly with Joule of exactly realising it in 1852, there could be no such thing as temperature in a scientific sense, and low temperature research would be devoid of most of its significance. These essential foundations for the scientific treatment of Energy were laid firmly in 1852, in a way that has held good without substantial modification ever since.

Perhaps this point, the rigorous scientific generality of the foundations on which he built from the beginning, could not be enforced

more strongly than by recalling that it is just this Thomson-Joule intrinsic cooling effect of expansion without external work, very slight under ordinary conditions, due merely to mutual separation of the molecules of the gas, that is the essential feature in the modern continuous processes for liquefaction of even the most refractory gases, by the expenditure of mechanical power to abstract the heat, which have now become familiar. On the other hand, the great economy of the reversed Carnot gas-cycle for ordinary refrigeration was pointed out in 1852, and applied by his brother to the ventilation of Belfast College.

In their parallel developments of the subject, while Clausius kept mainly to the theory of heat engines, applications over the whole domain of physical science crowded on Thomson. Already in December 1851 he communicates to the Royal Society of Edinburgh his Theory of Thermo-electric Phenomena, including the classical prediction of the convection of heat by the electric current, the so-called Thomson effect, which in the theory of electrons has a literal title to its original name. The formulæ of the printed abstract* of this paper show that he must have been already in full command (December 1851) of Carnot's principle in its most generalised form—viz., as he expressed it in May 1854, but there introducing absolute temperature T , then recently determined by himself and Joule—that in a complete reversible cycle of change $\Sigma(H/T)$ vanishes, or in differential notation $\oint(dH/T) = 0$, a form which was independently given by Clausius in December 1854, and from which the transition to Clausius' entropy-function (1856) is but a step. These advances appeared in full in the memoir, 'Trans. R. S. Edin.,' 1854,† where, in the way customary with him, he passes on to a long digression on the thermo-electrics of crystalline matter, including, after Stokes, the full theory of rotational vector effects. This latter subject was brought again into prominence many years after, when times were riper for it, with reference back to the present exposition, on the announcement by E. H. Hall of the discovery of an influence of this kind in electric conduction in a powerful magnetic field. Here also shines forth in a notable example what was always a main feature of Thomson's theoretical activity, the utilisation to the utmost of models and images of physical phenomena. He absolutely refused to deny to matter, however continuous and uniform as to sense it might appear to be, the possession of any property which he could imitate in a lattice structure or other architectural model, however complex; clearly, in his view, one has no right to assign limits *a priori* to the possible physical complexity of molecular aggregation.

One type of such limits, indeed, the only ones *a priori*, he vindicated in one of his most refined theoretical advances, those, namely,

* Math. and Phys. Papers, i. pp. 316–323.

† Loc. cit., pp. 232–261.

which are imposed on reversible phenomena by the principle of the conservation of energy. The demonstration on these lines that there can be no rotational quality in either magnetic or dielectric excitation in continuous media afterwards became, in Maxwell's hands, one of the main confirmations in the general electric interpretation of optics, by leading at once to the validity of Fresnel's theory of double refraction.

But we must return from this digression. The cosmical aspect of Carnot's principle, in its reconciliation with that of Joule, had immediately arrested Thomson's attention, and the fundamental law of Dissipation of Energy in natural phenomena stood revealed in a brief note in April 1852, embodying the following momentous and carefully formulated conclusions :—*

"1. There is at present in the material world a universal tendency to the dissipation of mechanical energy.

"2. Any *restoration* of mechanical energy, without more than an equivalent dissipation, is impossible in inanimate material processes, and is probably never effected by means of organised matter, either endowed with vegetable life or subject to the will of an animated creature.

"3. Within a finite period of time past, the Earth must have been, and within a finite period of time to come the Earth must again be, unfit for the habitation of man as at present constituted, unless operations have been, or are to be, performed, which are impossible under the laws to which the known operations going on at present in the material world are subject."

It is of interest to contrast this principle of degradation, or diffusion, of energy towards a uniform equilibrium, with the other great principle, dominating the phenomena of the organic world, which took shape at about the same time. Just fifty years ago biological thought was startled with the idea of the gradual evolution of organic forms, by the persistence, through hereditary transmission, of such accidental modifications as are adapted to the surrounding conditions of life, to the existing environment. In inorganic phenomena the energy becomes distributed among merely passive molecules; in the organic world the unit of investigation is an organism which has apparently the active property of fixing and transmitting in its descendants any structural peculiarity that it may come by. But even here there is something in common; the automatic evolution towards improved adaptation, in this case with no limit or equilibrium yet in sight, is attained at the cost of compensating dissipation, namely, the destruction of the individuals that happen to be ill adapted even though in other respects superior.

We observe in passing that in Thomson's formulation, Clause 2 already implies Clausius' conception (1854) of compensating trans-

* Loc. cit., p. 514.

formations. What is perhaps now more interesting is that it expresses a decided opinion (which he still retained in 1892) on a question which Helmholtz* to the end preferred to leave open, namely, whether the refinements of minute structure and adaptation in vital organisms may permit departure from the law of dissipation, which is known to be inflexible in the inorganic world, by utilising to some extent diffuse thermal energy for the production of vital mechanical power. The development of Clause 3 led to the famous series of investigations and discussions regarding the beginnings and the ultimate fate of our universe, and the duration of geological time, which have formed a region of intimate contact, but not always of agreement, between dynamical and evolutionary science.

Earlier in the same note, and also more fully in 'Phil. Mag.,' February 1853, Thomson illustrated his early complete grasp of all matters relating to the availability of thermal energy and to compensating transformations, in calculating the dissipation which arises from throttling steam, and the work which can theoretically be gained from the thermal energy in an unequally heated space.

This history is, however, not yet complete. Examination of the 'Notes inédites' of Sadi Carnot, appended to the reprint of the 'Réflexions,' published with charming biographical detail by his brother in 1878, and welcomed enthusiastically by Lord Kelvin, leaves an impression that Carnot was already struggling with difficulties of the kind to which the insight of Thomson exposed him some twenty years later. He had analysed (p. 91), with sure instinct, the Gay-Lussac experiment concerning heat of expansion of gas by efflux, and afterwards developed it (p. 96) into a suggestion of the identical porous plug experiment of Joule and Thomson. He points out (p. 92) that the view that heat is "*le résultat d'un mouvement vibratoire des molécules*" conforms to our knowledge in a long list of the principal transformations of energy; "*mais il serait difficile de dire pourquoi, dans le développement de la puissance motrice par la chaleur, un corps froid est nécessaire, pourquoi, en consommant la chaleur d'un corps échauffé, on ne peut pas produire du mouvement.*" He seems to be trying (p. 94) to think out a definite distinction between this movement of the particles of bodies and the "*puissance motrice*" into which it cannot be changed back. "*Si les molécules des corps ne sont jamais en contact intime les unes avec les autres, quelles que soient les forces qui les séparent ou les attirent, il ne peut jamais y avoir ni production, ni perte, de puissance motrice dans la nature. Alors le rétablissement d'équilibre immédiat du calorique et son rétablissement avec production de puissance motrice seraient essentiellement différents l'un et l'autre.*" "*La chaleur n'est autre chose que la puissance motrice, ou plutôt que le mouvement qui a changé de*

* See an interesting passage in his *Lectures on Heat*, posthumously published.

forme. C'est un mouvement dans les particules des corps. Partout où il y a destruction de puissance motrice, il y a en même temps, production de chaleur," and reciprocally. Like Thomson at the later date, he intended to seek the guidance of further experiment, outlines of which he sketched. These extracts suggest the very problems which are still fundamental in the molecular theory of Energetics, about which much is yet to be learned, though Thomson's theory of dissipation of energy and its molecular interpretation by Maxwell and Thomson and Boltzmann has illuminated the whole field. Yet Carnot already saw (p. 93) that his negation of perpetual motion demands that when heat does work in falling to a lower temperature, if some heat is really absorbed in the process the amount so absorbed must be independent of the mechanism of the process, must, in fact, be an equivalent of the work; for if the other alternative were possible, "on pourrait créer de la puissance motrice sans consommation de combustible et par simple destruction de la chaleur des corps." Clausius and Thomson had nothing in 1850 to add to this reasoning of date earlier than 1832.

No apology is required for thus dwelling at length on this episode in the evolution of the principles of physical science, the development of the principle of energy into its wider aspect, in which it assumes its universal co-ordinating rôle as the principle of available energy—involving its complete available conversation only in the limited class of phenomena that satisfy the Carnot test of being reversible, and in other cases emphasising the partial dissipation into diffused unavailable molecular energy which is characteristic of the operations of physical nature. No passage in the history of modern physics can, perhaps, compare with it in interest. In the other outstanding advance of the last century, the unravelment of the function of the æther as the sole means of intercommunication between the molecules of matter so as to constitute a *cosmos*, as the seat of the activities of radiation and of electric and chemical change, the problem to be solved was of a different type. The questions have there been more precise; they have suggested, and their investigation has been directed by, definite adaptable trains of experiment. But the pioneers in the theory of available energy had to probe among the *arcana* of common experience, in a manner which takes us back to the beginnings of dynamical science and recalls the efforts of Archimedes and Galileo and Pascal in detecting controlling principles in the maze of everyday phenomena.

The original stimulus to all this wide grasp of the relations of inanimate nature had its origin in the progress of mechanical invention, in the successful construction and operation of thermal engines. Irrespective of the problem of their industrial improvement, the detection of the essential features of this mechanical value of heat would appeal strongly to an analytical mind like that of Carnot. But his compact informing principle, as its content was ultimately developed in Thomson's hands, far transcended the special thermal problem from

which it started; it now dominates the whole range of physical science. It is only on its validity that our confidence is based, that we can treat the interactions of the finite bodies of our experience by strict mathematical and dynamical reasoning, entirely leaving aside, as self-balanced and inoperative, those erratic though statistically regular motions of the molecules, forming a very considerable part of the total energy, which constitute heat in equilibrium.

This fundamental basis of our knowledge of inanimate nature, thus acquired from clues suggested by human industrial improvements, still retains an aspect essentially anthropomorphic; it is conditioned by the limitations of our outlook as determined by the coarseness of our senses, as Maxwell seems to have been the first definitely to perceive. For the case of an ultra-material sentient creature of bodily size so small as to be comparable with a single chemical atom, his own sensible physical universe would be controlled by some fundamental law possibly of quite different type, while the phenomena which are prominent to us would take on for him a cosmical character as regards both time and space. We can ourselves catch partial glimpses of such a transformed physical universe, not subject to ordinary laws of matter in bulk, in the phenomena of high vacua, where the gaseous molecules come nearly individually before our attention and can almost be counted, and in the recent cognate phenomena of radio-activity either spontaneous or electrically excited. The boundary of demarcation of this new world from the universe which is dominated by the principle of available energy is naturally ill-defined: its exploration sheds light on both, and is perhaps the most interesting of the present activities of theoretical and practical physics.

Here also Lord Kelvin has played a part. Already, in 1852, he had prefixed to one of his papers the title 'On the Sources available to *Man* for the Production of Mechanical Effect,' as if in anticipation of this anthropomorphic side of the subject, first broached apparently by Maxwell in 1871 at the end of his "Theory of Heat," where he points out that it is only man's inability to obstruct passively the individual molecules at will that prevents the whole of their energy from being available, and shows how sentient agents capable of doing this could reverse the otherwise irrevocable course of diffusion of the energy in a gaseous medium.

Perhaps Thomson's own most systematic pronouncement on the inner significance of these relations is a short paper in 'Proc. R. S. Edin.,'* of date February 1874. He points out that the changes contemplated in abstract dynamics are strictly reversible; while in actual physical phenomena the absence of reversibility is conspicuous, a fact which was already embedded in the principle of dissipation of energy in 1852. Now "the essence of Joule's discovery that heat is diffused energy is the subjection of physical phenomena to dynamical law."

* Also *Nature*, ix. 1874, pp. 421-424; *Phil. Mag.*, March 1892, pp. 291-299.

Yet if we could reverse all inanimate motion, inorganic nature would unwind again its previous evolution; and if the materialistic hypothesis of life were true, living creatures could grow backwards with conscious knowledge of the future but no memory of the past, and would again become unborn. But the real phenomena of life infinitely transcend human science, and speculation regarding consequences of their ultimate reversal is utterly unprofitable. Far otherwise, however, is it in respect to the reversal of the motions of matter uninfluenced by life, a very elementary consideration of which leads to the full explanation of this theory of dissipation of energy." * He then follows up the matter by illustrative applications of the theory of probability of a kind that in more recent times has led to a statistical definition of entropy rich in promise for applications to chemistry and to natural radiation.

The very interesting subject of the thermodynamics of radiation is only about twenty years old. Resting as it does fundamentally on the link with mechanical energy which is afforded by Maxwell's working pressure of radiation, Lord Kelvin would never admit its validity. The reason seems to be that he was never able to satisfy himself about any mechanical model of the relation of the atom to the æther that would give a mechanism for this pressural interaction between them. There are those who hold that the physical idea of an electron is sufficiently precise to make the *rationale* of light-pressure logical and secure. But Lord Kelvin would not consider it until he could visualise the whole process—see it in operation, as he used to say—to effect which completely would possibly go deeper than we may ever hope to penetrate; and this inability cut him off from what some consider to be the most refined and beautiful special development of the science which he founded.

The question naturally arises whether the establishment of the mathematical function that is fundamental for the theory of mechanical energy is not a subtler matter than this mere estimation of chances: in other modes of its investigation a powerful array of the dynamical properties of the medium is introduced. What becomes of them in the present aspect? The answer is, that the chance cannot be estimated aright until we know all the conditions, dynamical and other, to which the distribution of molecules is subjected. The dynamical relations find their place as conditions restricting the possibilities of random distribution. If through ignorance some of them are overlooked, the chances will be in error; each new condition that is discovered modifies to some extent the whole process, and thus amends our knowledge.

But this aspect of entropy is quite in keeping with the subjective character, so to speak, of available energy. Objectively, the dissipa-

* Cf. Helmholtz's review already quoted, *Nature*, xxxii., 1885, *Wiss. Abhandlungen*, iii. p. 594.

tion of energy is merely the progress towards an equilibrium. As regards the purposes of man, whole regions of available energy may exist, of which he is ignorant, because he does not happen to have learned how to use them. The amount of energy available at a given temperature in a lump of carbon is possibly not yet exactly known: the process of turning it into heat before utilising it of course wastes most of it. Once, however, any slow reversible method of combustion has been discovered, in a voltaic battery for instance, the determination will be possible and may be effected once for all. Or, following a hint thrown out by Lord Rayleigh in 1875, afterwards developed more fully by Gibbs, we may make a rough estimate by applying the Carnot-Clausius formula to a cycle of which the upper temperature is that of spontaneous dissociation of the materials. We can, in fact, ascertain available energies only for systems which we can reach from a standard one by processes reversible in Carnot's sense.

Very early in Joule's investigations (1841) on the quantitative equivalence of various kinds of energy, he attacked the problem of the voltaic cell, and found his expectation verified, that in many cases the electromotive force was proportional to the thermal value of the chemical action of one Faraday equivalent of the reagent materials—provided he employed* “galvanic arrangements adapted to allow the combinations to take place without any evolution of heat in their own localities.” He concluded that the condition thus laid down must be departed from in certain observed cases of discrepancy, and Thomson, in 1852,† conducted experiments to detect such local reversible heat. This principle of Joule was also stated quantitatively later, in a general way, by Helmholtz in the ‘*Erhaltung der Kraft*’ in 1847. It lies at the foundation of Thomson's memoir of December 1851, ‘On the Mechanical Theory of Electrolysis,’ whence the restriction above stated, the absence of local reversible heat, is quoted. *On this condition* the principle is exact; and the main point of Thomson's paper is the calculation, with a view to comparison with direct experiment, of the theoretical absolute value of the electromotive force of a Daniell's cell, from Joule's measurements of the heat developed by the combination of an electrochemical equivalent of its materials. The paper also developed the parallel between chemical energy and mechanical energy as sources of electromotive force, including the deduction by the principle of energy of the force induced by motion of a circuit across a permanent magnetic field. The further prosecution of the main subject, into cases where local reversible heat is developed (as evidenced by sensible change in electric conditions with

* Math. and Phys. Papers, i. p. 477.

† Loc. cit., p. 503; cf. also p. 496, where, in agreement with Joule, he ascribes the main loss to the work done by evolved gases in expanding against the atmospheric pressure.

temperature), remained for Gibbs and Helmholtz twenty-four years afterwards. In another paper of the same date, on absolute electric measurement, Thomson discusses Joule's thermal determination of absolute electric resistance of 1846, which afterwards proved to be more correct than the earlier values of the ohm.

Most interesting in connection with modern ideas is an abstract of February 5, 1852,* again mainly expounding Joule's inspiring results and views on the transformations of energy. Thomson estimates from Liebig's data that about one-thousandth part of the total solar radiation incident on forest land is absorbed usefully by the trees, that being the amount recoverable as heat by their combustion. An intention to discuss these matters in connection with Carnot's principle, dealing also with the wave-lengths of the radiation, does not appear to have been fulfilled. Passing on to animal work, he estimates, after Joule, that as much as one-sixth of the energy of the food consumed can go directly into mechanical power. Then, relying on Carnot's principle, and Joule's discoveries regarding the heat of electrolysis and of electromagnetism, he proceeds to argue that "it is nearly certain that when an animal works against resisting forces, there is not a *conversion of heat into external mechanical effect*, but the full thermal equivalent of the chemical force is *never produced*—in other words, that the animal body does not act as a *thermodynamic engine*; and very probable that the chemical forces produce the external mechanical effects through electrical means."

Here he is emerging from the narrower theory of heat to the general theory of available energy, where heat is not the intermediary towards mechanical power; and we shall see presently how quickly he progressed in it. When it is recalled that at the time all this was going on, or immediately after, he was also laying the dynamical foundations of the phenomena of induced electric currents, including for example, the calculation of the period of the vibrations produced by electric discharges, the activity may well seem unprecedented; adequate exposition of the results had to fall behind.

The next stage (1855) in this series of investigations, the development of the ideas expressed in the extract just quoted, seems to demand special attention, for it is surely nothing less than the laying down of the precise laws of the all-embracing modern science of Free or Available Energy. The evolution of this generalisation can, as it happens, be traced. The memoir on 'A Mathematical Theory of Magnetism' has been already alluded to. In it, as everywhere else in Thomson's dynamical writings, the conservation of the potential energy, used there in the manner of Lagrange and Green and MacCullagh and Helmholtz, in the sense of a potential of mechanical forces, is employed to determine the essential relations between

* Loc. cit., p. 505.

physical properties. This use of the law of energy as a connecting principle afterwards became the note of Thomson and Tait's 'Treatise on Natural Philosophy.' In revising for press a continuation of this magnetic memoir, 'Phil. Mag.,' April 1855, where he is engaged in deducing magnetic reciprocal relations in more elementary fashion by use of a work-cycle, a thought occurred to him and was embodied in a footnote under date March 26, which will be quoted in full.*

"It might be objected that perhaps the magnet, in the motion carried on as described, would absorb heat and convert it into mechanical effect, and therefore that there would be no absurdity in admitting the hypothesis of a continued development of energy. This objection, which has occurred to me since the present paper was written, is perfectly valid against the reason assigned in the text for rejecting that hypothesis: but the second law of the dynamical theory of heat (the principle discovered by Carnot and introduced by Clausius and myself into the dynamical theory, of which, after Joule's law, it completes the foundation) shows the true reason for rejecting it, and establishes the validity of the remainder of the reasoning in the text. In fact the only absurdity that would be involved in admitting the hypothesis that there is either more or less work spent in one part of the motion than lost in the other, would be the supposition that a thermodynamic engine could absorb heat from matter in its neighbourhood, and either convert it wholly into mechanical effect, or convert a part into mechanical effect and emit the remainder into a body at a higher temperature than that from which the supply is drawn. The investigation of a new branch of thermodynamics, which I intend shortly to communicate to the Royal Society of Edinburgh, shows that the magnet (if of magnetised steel) does really experience a cooling effect when its pole is carried from A to B , and would experience a heating effect if carried in the reverse direction. But the same investigation also shows that the magnet must absorb just as much heat to keep up its temperature during the motion of its pole *with* the force, along AB , as it must emit to keep from rising in temperature when its pole is carried *against* the force, along DC ."

The exposition of the new branch of thermodynamics here referred to appeared in fact in the same month, April 1855, in the first part of the first volume of the 'Quarterly Journal of Mathematics,' under the title 'On the Thermoelastic and Thermomagnetic

* Elec. and Mag., § 672. In a less definite way this principle had been effective already; cf. Mack, *Prinzipien der Wärmelehre hist.-krit. entwickelt*. Early in 1849 James Thomson explains that it was his brother's pointing out to him that, on Carnot's principle, water could be frozen isothermally without requiring mechanical work, which set him on to the train of thought that predicted the lowering of the freezing-point by pressure and calculated its amount. As freezing is accompanied by expansion, a cycle involving freezing at a high pressure and melting at a low pressure, in fact confronted him with a perpetual motion, which he had to evade.

Properties of Matter, Part I.,' which represents the contents of the latter part of the paper to which the more general introductory matter was probably added. This paper was reprinted in 'Phil. Mag.,' January 1878, with some additional notes.* The principles that we are now concerned with occupy the first few pages; the argument is expressed in terms of elastic strain, but that is obviously only for convenience of exposition. The total intrinsic energy e of a material system, measured from a standard initial configuration and temperature, is defined as a function of its actual configuration and temperature. It is established from Carnot's principle, as in the quotation above, that for transformations conducted entirely at the same definite temperature t , the mechanical forces applied to the system must be derivable from a work function w which represents, in fact, the potential energy acquired by the system in passing at that temperature from the standard configuration to the actual one. If ϵ denote the simultaneous increment of e , then $\epsilon - w$ must be the heat H taken in from outside during that change from the standard configuration, when conducted at the actual temperature.

It is to be observed that this simple consideration, which apparently here appears in science for the first time, carries the principle of potential energy in its mechanical application right back to Carnot's principle of 1824. In the previous writings on general potential energy, such as Helmholtz's 'Erhaltung der Kraft,' nothing of the kind is hinted at: while Clausius' treatment, being restricted to transformation of heat, is nowhere connected up with the general theory of energy. The first law of thermodynamics henceforth drops to more restricted scope, for it merely asserts that available energy when lost is changed into heat in equivalent amount. Yet it still suffices to maintain the presumption that all energy-processes have their source in—are consistent with—the ordinary Newtonian principles of dynamics as applied to ultimate molecules; considering the difficulty experienced by Thomson in reconciling Joule's law with his innate conviction of the validity of Carnot's principle, it is not surprising that this inference appealed to him with special force. Indeed, when the historical conflict between the two laws is kept in mind, the value of the first will not be disparaged. From this point of view the principle of Carnot appears in transformed aspect. Its chief interest is now transferred to the two creative ideas which it contains, the introduction into science (i) of the idea of a complete cycle of transformations, and (ii) of the criterion of absence of waste of power in any mechanical process, namely, that the process can be reversed, which includes the condition of temperature uniform throughout the system at each instant. The further development, including Carnot's function and the quantitative determination of the idea of temperature which it brings with it, is the thermal com-

* Math. and Phys. Papers, i. pp. 291-316.

pletion of these fundamental principles of the general science of Energetics. When the illustrious originator of these ideas died in 1832 at the age of 36 he was in possession of the material to complete the train of essential principles himself.

Thus far we have secured a work-function w (available energy) for the applied forces at each temperature t , of form determinable by direct experiment. If such a function were known for every temperature, knowledge of the mechanical energy relations of the system would be complete. Thomson accordingly proceeds to connect these functions for adjacent temperatures by means of a Carnot cycle. In fact, he shows how to construct w as a function of both the configuration and the temperature, so that the same function shall, for each constant temperature, represent the energy then available for work.

The two functions, total energy e and work of available energy w , on which the complete science of Energy is thus founded, are naturally to be compared with the two functions, energy U and entropy S , which were made fundamental by Clausius in the very same month, April 1855—the tendency of the entropy of a *self-contained system* to increase being his mode of exact expression by Thomson's principle of dissipation. In fact, the distinction between the two methods is that Thomson's function w refers primarily to a system fed with heat so as to remain at constant temperature, while Clausius' function S refers primarily to an isolated system.

The principal operations of chemistry and physics are performed at constant temperature; thus it is Thomson's function w that is fundamental in the modern science of Energy, having been re-introduced by Willard Gibbs as "the characteristic function at constant temperature, and by Helmholtz as "free energy." The entropy is simpler to describe, and also to work with, except when the operations are isothermal; on the other hand, the "free energy" is a direct physical conception connecting up heat-energy in line with all other types of available physical energy, and thus transforming thermodynamics into the universal science of the relations of the statical transformations of Energy, namely, Energetics.

The function entropy seems to have been never employed in Lord Kelvin's investigations. As may be inferred from the above, it did not lie directly in his line of thought, which concerned itself with the physical entities energy and work. The idea of entropy is so directly suggested by his principle of dissipation, and the early mastery of the Carnot-Clausius equation $\int (dH/T) = 0$ for a reversible cycle, in its widest form, which is shown in his theory of thermoelectric phenomena, that it could hardly have been strange to him; conceivably he never directly formulated it, because he had, in fact, developed a more directly physical scheme.

It is customary, after Thomson's own example, to call the relation

$\int (dH/T) = 0$, as above, the Carnot-Clausius equation. It would provide the necessary complement to this nomenclature if his own equation between w , e , and t , which is, in more usual notation, the equation of energy A available at constant temperature T ,

$$A = E + T \frac{\partial A}{\partial T},$$

and is now the fundamental principle in chemical physics through the far-reaching applications made by Gibbs, Helmholtz, van 't Hoff, Nernst, and other investigators, were known as the Thomson equation. His dominating position is indeed already widely, but not very definitely, recognised.

The question whether Thomson had prior knowledge of the entropy principle has been matter of some controversy between Clausius and Tait: on the view here taken it is relatively unimportant.

We may now recall in general terms the form of the principle developed into most varied applications by Willard Gibbs, with such power and invention as to constitute him the creator of a new science. The necessary increase of the entropy function S defines the trend of adiabatic transformation; the necessary decrease of the available energy function A defines the trend of isothermal transformation.

The two functions are immediately connected by noticing that the S in the given configuration exceeds S_0 , that in the standard configuration at the same temperature T , by $-\partial A/\partial T$. We can render an isothermal transformation adiabatic by including in the system an infinite reservoir of heat at its own temperature, in the manner favoured by Planck: the change of total entropy is that of $S - H/T$, so that this function must always increase in an isothermal system. The reverse transition from adiabatic to isothermal would not be so direct. In fact, the entropy S is the convenient analytical function to employ when the temperature is different in different parts of the system, as is illustrated by the complexity of the calculation (already conducted in February 1853, in terms of Carnot's function μ) of the energy available for mechanical effect in such a system when self-contained,* which is mainly of cosmical interest, and has probably drawn attention away from the principles of free energy, though the latter were again emphasised in Thomson and Tait's 'Natural Philosophy.'

This analysis of available energy by Thomson had not escaped the notice of Willard Gibbs (1876), though possibly only in its narrower connection with elasticity.† "Such a method is evidently preferable with regard to the directness with which the results are obtained. The method of this paper shows more distinctly the *role of energy* and

* Thomson, loc. cit., p. 554. The calculation of the final uniform temperature is in fact based (p. 556) implicitly on constancy of the entropy.

† Scientific Papers of J. Willard Gibbs, i. p. 204.

entropy in the theory of equilibrium, and can be extended more naturally to those dynamical problems in which motions take place under the condition of constancy of entropy of the elements of a solid, . . . just as the other method can be more naturally extended to dynamical problems in which the temperature is constant." Gibbs then refers back to a previous note explaining the wider generality of his own method : its most salient feature is, however, the far wider development, by its author, into the doctrine of the chemical potentials of the constituent substances.

As throwing light on the stage at which scientific thought had arrived at the time Thomson was thus formulating the general science of Energetics, the following quotation from Helmholtz's important lecture,* 'On the Interaction of Natural Forces'—delivered first at Königsberg, February 7, 1854, and in which he was the first to refer the replenishment of solar heat to gravitational shrinkage—is pertinent to our history. "These consequences of the law of Carnot are, of course, only valid provided that the law when sufficiently tested proves to be universally correct. In the meantime there is little prospect of the law being proved incorrect. At all events we must admire the sagacity of Thomson, who, in the letters of a long-known little mathematical formula which only speaks of the heat, volume and pressure of bodies, was able to discern consequences which threatened the universe, though certainly after an infinite period of time, with eternal death."

Later, in 1861, in writing of the constant surprises that arose in his work on acoustics, and the impression borne in upon him that new results develop themselves in the mind according to laws of their own, so that it seems to be hardly things essentially of his own invention that he is reporting, Helmholtz suggests that "Mr. Thomson must have found the same thing in his own work on the mechanical theory of heat."†

The end of this early period of pure scientific activity came when Thomson's enthusiastic encouragement of the costly practical enterprise of Atlantic submarine telegraphy entangled him in the solution of a whole class of practical problems, which only he could undertake, and which constituted one of the most strenuous tasks of his career. After the first cables had failed, in 1857–8, through wrong methods of working which entirely misconceived the situation, Thomson was given a free hand to make the most, under these expensive conditions, of the problem—in fact to get signals transmitted at profitable speed through a conductor, which, as he insisted, merely diffused electricity along it as heat travels along a bar, instead of conveying it in compact pulses or waves. Here he succeeded by following out the pre-

* English translation (by Tyndall), l. 1873, p. 172.

† Life, p. 205.

visions of pure reasoning: for there could be little opportunity for making tentative experiments, according to the usual trial methods of inventors, such as, for instance, have so rapidly and brilliantly improved the arrangements for telegraphing across space, after Hertz had taken the preliminary crucial step of discovering, or rather recognising, the electric waves that are concerned in it. The necessities of this cable problem led largely to the invention of the fundamental principles of delicate and exact practical electrical measurement; and though their embodiment in apparatus has naturally been subject to continuous improvement in detail, yet the principles remain largely those evolved in the early days by Thomson and his associates.

Among the most potent causes of the general improvement in physical modes of thought during the last third of the century, was the appearance, in 1867, of what then purported to be merely the first volume of the '*Treatise on Natural Philosophy*,' by W. Thomson and P. G. Tait, which has proved to be a turning point in the exposition and expression of physical science, at any rate in this country. The preparation of this book, which had gone on for some years, induced frequent visits by Thomson to his friend and disciple Tait, at Edinburgh. Among other things, this treatise revised the terminology of dynamics, which had been allowed to grow up, in many respects, in forms that retained only historical meaning; the impulse thus given, which had indeed already been operating less systematically in the previous years, and was largely due doubtless to his brother James Thomson, has led, in the hands of Maxwell, Heaviside and others elsewhere, to greater attention to the language of science, the introduction everywhere of expressive terms, which react powerfully in inducing clearness of ideas. Another of the benefits conferred by this work was that it served, in some degree, to focus the scattered fragments of Thomson's own investigations and those of his associates, and to exhibit his scientific method, as exemplified in the subjects covered in this first instalment, which contained general kinematics and dynamics, general theory of the potential, and theory of elasticity with extensive geodetic application.

A translation of this book into German, by Helmholtz and Wertheim, appeared in 1871-4. In a preface, Helmholtz pointed out how it satisfied, in very remarkable manner, a most urgent want in higher scientific literature. Previously there had been no resource but to go to original memoirs, difficult of access even if one knew where to find them; and on this account the recent progress of connected mathematical physical thought had been halting. Moreover, as he said, when a worker like Sir William Thomson admits us to participate in the very upbuilding of his ideas, exhibits to us the modes of intuition, the guiding threads, which have helped him, by bold combinations of thought, to control and arrange his refractory

and entangled materials, the world owes him its highest gratitude. Helmholtz goes on to contrast the universal outlook of such a book, involving unavoidable lacunæ and difficult transitions, with the beautiful precision of the best special treatise of the earlier period. But the reader who does not spare himself the necessary effort towards mastery reaps an ample reward; he will find himself trained and equipped for the task of appreciating and extending knowledge, to a degree that he could never have attained from mere passive assimilation of sharply cut formal demonstrations. Valuable to the same end is the constant endeavour of such a work to employ those mathematical methods that keep close to actuality, are amenable to detailed interpretation; though they are naturally much harder, especially at first, than a strictly ordered analytical calculus would be, there remains the permanent gain of direct insight into the processes and relations of nature. Finally, allusion is made to difficulties encountered by the translators, arising from the originality of the treatment, and the series of new scientific terms that the authors had, in consequence, introduced.

This appreciation, by the most competent living master, set out justly the advantages and defects of Thomson's method of work. He never had time to prepare complete formal memoirs. It was but rarely that his expositions were calculated to satisfy a reader whose interests were mainly logical; though they were almost always adapted to stimulate the scientific discontent and the further inquiry of students trained towards fresh outlook on the complex problem of reality, rather than to logical refinement and precision in knowledge already ascertained. Each step gained was thus a stimulus to further effort. This fluent character, and want of definite focus, has been a great obstacle to the appreciation of "Thomson and Tait," as it is still to Maxwell's "Electricity," for such readers as ask for demonstration but find only suggestion and exploration. There is perhaps nothing that would contribute more at present to progress in physical thought than a reversion, partial at any rate, from the sharp limitation and rigour of some modern expositions to the healthy atmosphere of enticing vistas which usually pervades the work of the leaders in physical discovery. With increased attention to the inspired original sources of knowledge the functions of a teacher would be more than ever necessary, to point to the paths of progress and to contrast the effectiveness of different routes, as well as to restore valuable aspects which drop away in formal abstracts; science would thus adhere to the form of a body of improving doctrine rather than a collection of complete facts.

The establishment of significant terminology in dynamics was made still more effective by material illustration of the principles thus connoted. For example, the law of conservation of rotational momentum, of which the germ was already in the *Principia*, had been developed by d'Alembert and Laplace into complex formulæ which

were interpreted cumbrously in terms of the idea of description of areas, introduced long before by Kepler and Newton for the simple case of the motion of the planets. Building on the introduction by Poinso^t of the idea of rotational effort or torque, Thomson set about making the rotational momentum possessed by a body an intuitive fact, like its mass or volume or density, instead of a mere complex mathematical expression. The subject was illustrated in his lectures by experiments intended to develop a direct sense of the elastic stiffness of balanced spinning masses. The gyroscope of Foucault, with its special gimbal suspension, became the gyrostat, in which the spinning body was enclosed and protected by a frame or box, and could thus be manipulated readily in many ways, e.g., either balancing itself on an edge, or on stilts, or being hung on to a pendulum with free suspension; it could thus assist towards realising varied knowledge of the properties conferred by this newly recognised type of permanent possession of matter—the intrinsic spin of a free body, which in the absence of friction of the bearings would be retained for ever—which has in fact more recently become a branch of experimental engineering.

This idea of spin, as a possible essential endowment of masses of matter, was constantly being extended in many directions. In particular it came to be the foundation of the first, and hitherto the only fairly successful, mechanical representation of the working of the æther of space. Though Thomson's early mathematical elucidations and demonstrations of Faraday's ideas formed a main part of the material which Maxwell welded into a connected theory of electricity, which afterwards extended itself into optics and general radiation, his own mode of investigation took on rather the reverse order. For he could imagine no direct mechanical model wide enough to include the various sensible modes of working of electricity, which he had himself elucidated in special domains from the doctrine of transformation of energy and the principles of Ampère and Faraday. Thus he turned aside to attack the more compact and definite problem of discovering the type of an æther that would satisfy merely vibratory requirements, those namely of light and radiation. Long-continued efforts to base its properties on elasticity arising simply from solidity, after the manner developed by Green, though evolving many fruitful aspects of elastic propagation in continuous physical media, had all ended in failure. Why not then try this elasticity of rotation instead of elasticity of form? The result was immediate success. But, as embodied in a model—only theoretical it is true, but one whose construction could be distinctly contemplated, and has since been in part made practically effective in the form of gyrostatic apparatus for steadying ships relatively to the vertical—the achievement carried wider consequences than this. It formed the objective justification of a purely analytical solution of the same problem which had been offered by MacCullagh, of Dublin, fifty

years before. That investigator had, in fact, shown that all the optical functions of the æther were consistent with Lagrange's abstract relations, in which the essential content of Newtonian dynamics had been concisely formulated free from the material accidents attaching to special systems and mechanisms. MacCullagh's abstract mathematical solution had, however, been widely repudiated, because it went beyond such conceptions as could properly belong to ordinary static material phenomena. But rotational momentum, as a possible endowment of matter, had been overlooked in this criticism : and Lord Kelvin's ideal model was thus the justification of MacCullagh's theory of light—which, once it became favourably appreciated, now extended itself most readily and naturally to include the whole field of electric phenomena, which appeared as non-vibratory modes of disturbance of the same æther—thus in fact exhibiting the Maxwellian theory developed in the reverse direction from optics to electrics.

All his life Lord Kelvin was keenly interested in such potentialities of intrinsic latent spinning masses, as a possible factor in the ultimate explanation of the sensible qualities of bodies. In ordinary physics, the elasticity of solids is merely taken as something existing, without attempt at explanation of its origin, but with full utilisation of the restrictions as to type which the principle of energy imposes. Hence the significance of the Royal Institution Lecture of 1881, with its copious and beautiful illustrations, entitled 'Elasticity viewed as possibly a Mode of Motion'—viewed in fact as possibly stiffness induced in matter, itself non-elastic, by latent intrinsic spin—and more fully developed in the British Association Address of 1884, 'Steps towards a Kinetic Theory of Matter.'*

It has been recalled that the elastic properties of the æther can be imitated by a model composed of connected rotating masses, too complex, of course, to be actually made, but which can quite definitely be imagined. The fascinating question whether all physical action can thus be ascribed to latent phenomena of motion was always to the fore in Thomson's thought. The refined experimental proofs by Joule that mechanical energy never vanishes, but when apparently lost is still traceable as the diffuse energy of heat, afforded to him the strongest presumption that all manifestations of energy are subject to the principles of dynamics as resolved into their essential elements by Newton. This conviction would be the stronger, from the fact that for two years or more he was at a loss to reconcile Joule's principle with the doctrine of Carnot, which had very early obtained a firm hold on his mind. The reconciliation came through the distinction drawn between total energy and available energy ; the deeper meaning of the principle of Carnot was simply (1874,

* Popular Lectures and Addresses, i,

following Maxwell) that the mechanism of physical energy is so minute in scale of magnitude as to be only partially under the control of man. Is it possible to reduce all potential energy, electric or otherwise, to the ultimate simplest terms, as interactions of latent cyclic motions, such as he was constantly occupying himself with, in connection with his gyrostats?

At one period, from 1867 onward, Thomson made a very determined effort to carry a special scheme of this kind as far as it could go, in the form of the theory of vortex atoms, to which he devoted some of his most original and powerful efforts, building on the famous hydrodynamic theorems of Helmholtz which have exerted so great an influence in modern physical thought. To Lord Kelvin, at this time,* prompted by a magnificent display of smoke rings recently witnessed in Tait's lecture room where they rebounded from one another as if made of rubber, the discoveries of Helmholtz inevitably suggested that vortex rings "are the only true atoms," as they evade the customary "monstrous assumption of infinitely strong and infinitely rigid pieces of matter," while they have permanent individuality and free periods of vibration, in that respect probably passing beyond any ideas present to the mind of Rankine, when in 1850 he treated of 'Molecular Vortices' in connection with thermodynamics. The vortex ring would also be "strong and durable," an unchanging element, in a way that a vibrating congeries of more elementary discrete ultimate atoms could not be.

We have also seen him engaged in the same task of illustration of elasticity and other properties of matter, by systems of rigid gyrostats connected by mechanism, in place of the flexible vortex rings affected only by the fluid medium in which they subsist. Thus, in 1856, he illustrated Faraday's magneto-optics by calculating the duplication of the period of oscillation of a pendulum, which is produced when a gyrostat is hung from it—a problem which, as Gray remarks, bears directly on the mode of explanation of the modern Zeeman effect, rather than of its converse the original magneto-optic influence of Faraday. The general treatment of these problems of latent steady motions, forming perhaps the most notable modern extension of physical dynamics, originated in the second edition of Thomson and Tait's 'Natural Philosophy,' published in 1879; the requisite analysis was, however, introduced in simpler and more compact form by Routh two years previously, in his essay on 'Stability of Motion,' where everything was expressed in terms of a modified Lagrangian function, described later by Helmholtz, whose writings expanded the theory in many directions, and made it more widely known, as the "kinetic potential."

The recreation of yachting, by which Thomson was wont to

* Phil. Mag., July 1867, pp. 15-24.

recruit his energies in summer, reacted naturally towards the improvement of nautical affairs. His dynamical instinct, and experience in the invention of delicate instruments, found a congenial field in placing the ship's compass on a scientific basis. The heavy cumbersome magnets, swinging on pivots under unsuitable conditions, were replaced by the well-known systems of needles, delicately suspended yet insensitive to shock, so small that the iron masses compensating for the magnetism of the ship could be effectively introduced in moderate size. Again, by the use of steel wire, he worked up the modern method of taking reliable soundings from a ship in motion, the depth being calculated from the compression of the air in a narrow glass tube attached to the sinker. But the most remarkable feat in this domain was the thorough practical mastery of the complicated phenomena of the tides, achieved mainly under his direction, and culminating in the invention, about 1876, of simple automatic mechanism for performing all the laborious calculations of tidal harmonic analysis, both direct and inverse. The tides are controlled by the Sun and Moon, and so repeat themselves very closely in periods of nineteen years. But there is another far more fundamental and instructive way of investigating them. To every periodic (simple harmonic) component in the motion of either Sun or Moon relative to the Earth, there corresponds a component of the same periodic time in the tide produced by them at any place, and there are no other components; yet to calculate their amounts directly with the existing irregular contours and depths of the ocean would be a problem practically impossible. The method of harmonic analysis, as first initiated in this subject on a much smaller scale by Laplace, allows us to deduce, from a tidal record for a sufficient length of time, the amplitudes and phases of these harmonic components of known periods; and when the more important ones have been thus determined, the prediction of future tides becomes a matter of merely summing up the harmonic constituents, no matter how complex the physical conditions at the place in question may be, so long as they are unchanging. All this and much more can now be done by the machines invented by Lord Kelvin and his brother,* though, owing to the preliminary imperfection of construction of the analysing machine, it is at present found to be safer and not very troublesome to determine the amplitudes of the components by calculation. This achievement—the complete mastery of the tides by means most simple but adequate—is perhaps the greatest triumph of the method of Fourier, which has always been one of the advances most admired by Lord Kelvin in modern physical mathematics. After this success it was natural to apply the same method of harmonic analysis to meteorological phenomena, including the atmospheric electricity which he had investigated many years before, which also are con-

* See Thomson and Tait's *Nat. Phil.*, ed. 2, Appendices.

trolled by Solar influence ; but here the problem has proved not to be so feasible, the definite periodic components being so mixed up with the erratic results of meteorological instabilities that not much has yet come out of the effort.

In later years Helmholtz paid many holiday visits at Largs, and enjoyed the yachting expeditions, which provided a refuge for him from the attacks of hay fever. In 1871, the two friends studied the theory of waves which Thomson "loved to treat as a kind of race between us." It was shortly before that Thomson had broken new ground suggested by observations from his becalmed yacht, on the theory of capillary ripples, and on the waves produced by wind and current, treated in two letters to Tait intended for the Royal Society of Edinburgh. In later years the latter subject was discussed in much more detail and developed in new directions by Helmholtz, with a view to meteorological atmospheric applications.*

On board the yacht, Helmholtz reports † that "It was all very friendly and unconstrained. Thomson presumed so much on his intimacy with them that he always carried his mathematical notebook about with him, and would begin to calculate in the midst of the company if anything occurred to him, which was treated with a certain awe by the party. How would it be, if I accustomed the Berliners to the same proceeding? . . ."

In 1884 Sir W. Thomson delivered the well-known course of lectures on 'Molecular Dynamics and the Wave Theory of Light,' at Johns Hopkins University, Baltimore, after attending the meeting of the British Association at Montreal. The papyrograph unrevised report, issued in December 1884, by Mr. A. S. Hathaway, may justly be said to have reawakened, or at any rate strongly intensified, interest in the ultimate form of the problem of æther and radiation, both in this country and abroad. It seems fair to say also that the interest and value of the lectures arose largely from the unpreparedness of their author. As his audience of American physicists fed him from day to day with the more recent experimental and theoretical results relating to selective absorption, which were largely new to him, they had before them the spectacle, on which Helmholtz had laid stress, of one of the great minds of the century struggling with fresh knowledge and trying to assimilate it into his scheme of physical explanation, calling up all his vivid store of imagery and analogy to aid. His auditors at the time, and his readers afterwards, thus must have considered the lacunæ and difficulties as their own personal problems in which they were assisting. Perhaps no exposition in physical science so vivid and tempting has ever been published ; and for many years afterwards scientific activity in these subjects was strongly tinged by the Balti-

* Cf. Baltimore Lectures, Appendix G, and Prof. Lamb's *Hydrodynamics*.

† *Life*, p. 287.

more lectures, which transformed optics for the time from an affair of abstract mathematical equations into a subject of direct physical contemplation, in close touch and analogy with the objective manifestations of ordinary dynamics.

In the preface to the authoritative edition of 1904, which in the twenty years' interval had grown to be a volume of some 700 pages octavo, Lord Kelvin in fact describes the object of the course of lectures as follows: "I chose as subject the Wave Theory of Light with the intention of accentuating its failures, rather than of setting forth to junior students the admirable success with which this beautiful theory had explained all that was known of light before the time of Fresnel and Thomas Young, and had produced floods of new knowledge, splendidly enriching the whole domain of physical science. My audience was to consist of professorial fellow-students in physical science; and from the beginning I felt that our meetings were to be conferences of coefficients* in endeavours to advance science, rather than teachings of my comrades by myself. I spoke with absolute freedom, and had never the slightest fear of undermining their perfect faith in ether and its light-giving waves; by anything I could tell them of the imperfections of our mathematics; of the insufficiency or faultiness of our views regarding the dynamical qualities of ether; and of the overwhelmingly great difficulty of finding a field of action for ether among the atoms of ponderable matter. We all felt that difficulties were to be faced, and not to be evaded; were to be taken to heart, *with the hope of solving them if possible*; but, at all events, with the certain assurance that there is an explanation of every difficulty though we may never succeed in finding it."

He goes on to say that he had now, in 1904, virtually got to the bottom of the difficulties of 1884. He thinks, too, that in the wider field of æthereal phenomena *everything non-magnetic* can be explained "without going beyond the elastic solid theory," but *nothing magnetic*. "The so-called electro-magnetic theory of light has not helped us hitherto; but the grand object is fully before us of finding comprehensive dynamics of ether, electricity, and ponderable matter, which shall include electrostatic force, magnetostatic force, electro-magnetism, electro-chemistry, and the wave theory of light."

His purely scientific activity from 1884 onwards hinged largely on the production of the definitive edition of these lectures, which, in terms of the remarks just quoted, had raised up in front of him all the difficulties in modern optical and general æthereal theory. The resulting volume, with its numerous insertions, including most of pp. 280-468, and the twelve Appendices occupying pp. 648-700, may take rank in fact as virtually Volume IV. of the 'Mathematical and Physical Papers.' Among the vast array of new and recent material collected into the volume there may be mentioned the following:

* In the literal sense of the term.

theory and observation on the opacity of air and gases, reflexion from diamond and from metals, his various attempts at elastic solid vibratory theories of the æther, rotation of the plane of polarisation combined with double refraction, waves on water and in dispersive media with the residual disturbance they leave behind, waves raised by wind or by ships, the total mass of the material universe, various theories of electrons or electrions as he preferred to call them; also much regarding molecular tactic of crystals and the resulting dynamics, this time on a Boscovichian foundation. The Royal Institution lecture of 1900, on 'Nineteenth Century Clouds over the Dynamical Theory of Heat and Light,' is also included; these difficulties he there reduces to two: the difficulty regarding the motion of matter through æther, which he thinks is "not wholly dissipated," and the difficulty about the frittering away of the energy of gaseous molecules among their numerous periods of free vibration, which he solves in what may possibly be held to be the natural way, by denying the proofs.

Little has been said here with regard to Lord Kelvin's masterful and most effective preoccupation with the development of modern electric engineering, which has now almost completed the transition from the age of steam to the age of electric power. In this new branch of applied science, his active perception of the essentials of progress assumed the form of generalship; most of the details of progress naturally came from others, but he was ready always to emphasise the salient problems, and to acclaim, early and enthusiastically, such nascent inventions as would be pertinent to their mastery. An example is afforded by the emphasis with which he hailed the invention of the original Faure storage cell or accumulator,* which promised to supply the improvements (including the subdivision of a large storage battery to play the part of a step-down transformer, not yet practically effective) then necessary for economical development of the electric generation of power. This subject came particularly to the front in his Presidential Address in 1881, at York, to the Physical Section at the Jubilee Meeting of the British Association, 'On the Sources of Energy in Nature available to Man for the Production of Mechanical Effect,' which almost repeats the title of his early paper of 1852, but is this time concerned with the practical utilisation of these sources, now rapidly ripening, whereas the earlier discussion related to their philosophical detection and estimation. In this Address, after referring to Siemens' suggestion, three years previously, of the electrical transmission at high potential of the power of Niagara Falls, itself resting, as he remarks, on Joule's early experimental discovery that in an electromagnetic engine as much as 90 per cent. of the energy of the driving current can be utilised, he proceeds to summarise his own conclusions regarding economy of trans-

* Brit. Assoc. Report, 1881, p. 526.

mission over long distances, as communicated in the form of evidence to a Parliamentary Committee two years before. The brief paper, now classical in electro-technics, then communicated,* ‘On the Economy of Metal in Conductors of Electricity,’ is an early notable instance of the blending of economics with exact physics: the solution of the problem “would be found by comparing the annual interest of the money value of the copper with the money value of the energy lost annually in the heat generated in it by the electric current. The money value of a stated amount of energy had not yet begun to appear in the city price lists.” He shows that the gauge to be chosen for the transmitting conductor does not depend on its length, but solely on the strength of the current to be employed. He was much concerned also in the early evolution of dynamos (the term had been introduced by him about this time as a contraction for dynamo-electric machine), the designing of which was to become entirely effective a few years later by means of the graphical methods introduced by Hopkinson. Perhaps the earliest domestic installation of electric lighting in this country was the experimental one which he established in his house at the University of Glasgow; while one of the early public installations was the one, still in operation, which he presented, in connection with the celebration of the six hundredth anniversary of the foundation of that most ancient house, to his College in Cambridge, which had been able, under new statutes, to re-elect him to the Fellowship that he had vacated long before on his marriage.

The introduction of heavy currents and voltages in engineering required the provision of suitable instruments of measurement. This was always a congenial task: his graded series of current-weighers or ampere-meters, and of volt-meters—embodying those theoretical principles of adequate support free from constraint or strain, in mechanical design, on which he always insisted, to the great improvement of general practice in such matters—have proved to be of fundamental service wherever exact measurement is essential.

His interests ramified into all departments of human activity: even his physical writings were often relieved by play of allusion to literature and history. In his later years he took an active and zealous part in political affairs, and attended regularly the sittings of the House of Lords. In his undergraduate days he was one of the founders of the Cambridge University Musical Society, playing the French horn at its opening concerts in 1843, and becoming president in due course. Later he published some observations† on the beats of imperfect harmonies of simple tones, tending to a conclusion different from that of Helmholtz which referred the beats to combination tones.

All this activity implied a robust constitution. As an undergraduate at Cambridge, he found time to take a keen interest in

* Brit. Assoc. Report, 1881, pp. 526–8.

† Roy. Soc. Edin. Proc., 1878.

manly sports, rowing in the Peterhouse boat, which had second place on the river, and winning the Colquhoun sculls, then, as now, one of the main objects of athletic ambition. Afterwards he was expert at curling, until a serious accident on the ice stopped the pursuit and left him slightly lame for life. His subsequent yachting and cable-laying experiences have been already referred to.

The general impression produced, at first sight, by the four volumes containing the collected scientific papers up to 1860, might well be a somewhat vague notion of desultory, though profound, occupation with the ideas that were afterwards to be welded by more systematic expositors into our modern theoretical knowledge of mechanical and electrical and optical philosophy. At first glance, the exposition in characteristically practical terminology might even suggest that these papers were concerned with the engineering achievements by which he is most widely known, as much as with new theoretical foundations for physical science. Closer attention has compelled the conclusion that the results of his activity in the early period from 1845 to 1856 are perhaps unique in modern scientific annals; at any rate there can have been few parallels since Newton and Huygens and their great predecessors. It is said that Lagrange qualified his profound admiration for the genius of Newton by the reflection that only once could it be given to a mortal to have a system of the stars to unravel. Somewhat in the same way one might imagine the reflection of a seer of the future, that it can hardly be given again to a man of genius to have, in his first dozen years of creative intellectual activity, the ideas and discoveries of a Carnot, a Faraday, and a Joule, to interpret and develop for mankind.

His only peer in general physics in those early days, as also later if we exclude his own disciples, was perhaps Helmholtz. They began their careers of investigation about the same time, but at first their paths did not lie much together. For in his early years Helmholtz's professional work was that of a physiologist, though in the essay on the 'Conservation of Energy' he revealed, in 1847, his true bent as a leader in the exploration of the underlying principles connecting the different departments of the fundamental science, general physics. By the time this famous essay came into Thomson's hands, in 1852, he had himself travelled, with Joule's assistance, as far as it reached, if we except some special applications: but much more, he had in fact already dug down, on the inspiration derived from Carnot, far into the true foundations of the doctrine of Energy as available and recognisable to man, evolving from it ideas now familiar, but then of revolutionary significance, as regards both dynamical science and cosmic evolution, of which no one up to that time had any definite notion. The saving virtue of physical or any other genuine science is, that the most essential discoveries of one generation become worked up so as to be obvious and almost axiomatic to the next. The charm

of the study of scientific history is thus to trace the beginnings of creative ideas, to see how slight sometimes was the obstacle that delayed the discovery of a new field of knowledge ; though here the temptation to read back our own refined knowledge into the past lays many snares. In no part of science is this interest greater than in the doctrine of Available Energy ; the generality of outlook, leading to recasting of the fundamental ideas regarding physical force and power, which was secured by Thomson away back in the fifties, is on the least favourable view a matter for wonder.

In the years following, the powers of Helmholtz were concentrated largely on his great task of the exploration of the physical foundations of the activity of the senses, a subject of fundamental importance because they supply our only outlook into the external world ; while Thomson's efforts were employed in the problem, then urgent and preparatory to Maxwell, of the dynamical interpretation of the ideas of Faraday, and in the creation of the fundamental science above referred to which constitutes Thermodynamics in its widest sense, the all-pervading doctrine of Available Physical Energy to which it seems appropriate that Rankine's name Energetics should belong. In later days of close friendship their fields of activity had much in common, Helmholtz apparently often brooding over, and developing into fuller and more varied aspects, fertile points of view, such as the influence of wind and surface-tension on waves, and the generalisation of dynamics by the inclusion of latent cyclic motions, that had been already thrown off in more summary fashion by his colleague. On the institution of the Helmholtz memorial medal, the first award was to Lord Kelvin.

In a letter to Tait in 1876,* who was preparing a biographical notice for 'Nature,' Helmholtz had given an estimate of the work of his friend at that period. "His peculiar merit, according to my own opinion, consists in his method of treating problems of mathematical physics. He has striven with great consistency to purify the mathematical theory from hypothetical assumptions that were not a pure expression of the facts. In this way he has done very much to destroy the old unnatural separation between experimental and mathematical physics, and to reduce the latter to a precise and pure expression of the laws of phenomena. He is an eminent mathematician, but the gift to translate real facts into mathematical equations, and *vice versa*, is by far more rare than that to find the solution of a given mathematical problem, and in this direction Sir William Thomson is most eminent and original. His electrical instruments and methods of observation, by which he has rendered, amongst other things, electrostatical phenomena as precisely measurable as magnetic or galvanic forces, give the most striking illustration how much can be gained for practical purposes by a clear insight into theoretical questions ; and the series of his papers on thermodynamics

* Nature, xiv. 1876, p. 388.

and the experimental confirmations of several most surprising conclusions deduced from Carnot's axiom, point in the same direction."

We have seen the hints and principles thrown out by Thomson in such profusion fructify in patient development by other great investigators, so that it would be difficult to name a branch of modern physical science in which his activity has not been fundamental. In one phase of his thought it becomes cosmical, and transcends experimental aids. All through life his ideas were wont to range over the immensities of the material universe, reaching back to its origin and onward to its ultimate fate. In his youth he established the cardinal principle of inanimate cosmic evolution, as effected through the degradation of energy, which determines the fate of worlds, and is the complement of the principle of evolution in organic life which came to light at about the same time. In another aspect of this principle, asserting that the trend of available energy must always be downwards, it has developed into the key to the course and the equilibrium of voltaic and chemical change, and to all other branches of physical knowledge in which the atomic nature of matter is the pervading influence. The greatness of the revolution thus effected in physical science, and in its industrial applications which are in strict relation to this available energy, requires no emphasis. The magnitude of the advance brought by the mere enunciation of the principle of dissipation is to be measured by the very inevitableness of this law to our present modes of thought; it is difficult now to recognise the limitations that must have belonged to the time when its formulation caused such surprise and wonder.

At the end of his strenuous career his thoughts reverted again to these problems of the origin and destiny of material things. Novel considerations were brought to bear, with intellectual vigour appropriate to youth, to demonstrate even the finiteness of the material universe—such, for example, as the darkness of the firmament and the moderate magnitude of the relative velocities of the most distant stars. In the last weeks, he pondered over the remote history of our own planet, and reasoned with striking force and lucidity, as may be read in a posthumous paper, on the antiquity of its continents and oceans, reaching back possibly to the time when the Moon separated from the Earth.

In this sketch the chief aim has been to set out a connected historical view of the course of Lord Kelvin's scientific activity and its relation to his contemporaries. No attempt has been made to describe the charm of his personality. That has been recognised long ago by the whole world; for many a year the ordinary restrictions of nationality have had little application to him; he has been venerated and acclaimed wherever scientific investigation is appreciated. No instance in his long career can be recalled in which he asserted for himself any claim of priority in intellectual achievement; rather his

attitude has always been to show how much he had learned from his colleagues, and how much he expected to derive from them in the future. In this regard it is fitting to interpolate an extract from the fine appreciation by Lord Rosebery, his successor in the Chancellorship of the University of Glasgow, delivered in his installation address* : "In my personal intercourse with Lord Kelvin, what most struck me was his tenacity, his laboriousness, his indefatigable humility. In him was visible none of the superciliousness or scorn which sometimes embarrass the strongest intellects. Without condescension, he placed himself at once on a level with his companion. That has seemed to me a characteristic of such great men of science as I have chanced to meet. They are always face to face with the transcendent mysteries of nature. . . . Such labours produce a sublime calm, and it was that which seemed always to pervade Lord Kelvin. Surely, in an age fertile in distinction, but not lavish of greatness, he was truly great. Individualism is out of fashion. . . . But great individualities, such as Lord Kelvin's, are independent of the pressure of circumstance and the wayward course of civilisation."

[129] It is unnecessary to attempt any list of the distinctions and awards which came to him in the course of years : it suffices to say that there was probably no honour open to a man of science that was not at his disposal. Abundant personal record is and will be available in appreciations by his colleagues, who were all his friends ; for example, in the masterly estimate by G. F. FitzGerald, contained in the memorial volume reporting the proceedings in celebration of the Jubilee of his Professorship at Glasgow in 1899. In deference to the strikingly unanimous desire of his countrymen of all classes, and amid touching tributes from his colleagues in other nations, he was laid finally to rest in historic ground, on December 23, 1907, alongside his great exemplar, Sir Isaac Newton, in Westminster Abbey.

[J. L.]

* The Times,⁷ June 13, 1908.

GENERAL MONTHLY MEETING,

Monday, May 4, 1908.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and
Vice-President, in the Chair.

J. O'Connor Donelan, Esq.
Harold Edward Donnithorne, Esq.
Miss Helen Douglas,
William Laurie Dunn, Esq.
The Rt. Hon. Earl Fitzwilliam, D.S.O.
Miss Minnie Sophia Farrer,
Hugh Erat Harrison, Esq.
R. C. B. Kerin, Esq.
Mrs. Carl Meyer,
Lady O'Hagan,
Harold Thomas Perkins, Esq.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to John Young Buchanan, Esq., M.A. F.R.S. *M.R.I.*, for his Donation of £100 to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Honorary Secretary announced the decease of the Rt. Hon. The Duke of Devonshire, K.G. G.C.V.O. P.C. M.A. D.C.L. LL.D. F.R.S., Chancellor of the University of Cambridge, Chancellor of the Victoria University of Manchester, on March 24, 1908; and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted:—

Resolved, The Managers of the Royal Institution of Great Britain desire to record their sense of the loss sustained by the Institution in the decease of the Duke of Devonshire, a Member of the Royal Institution and a late Manager.

The Managers feel the Institution has lost a most distinguished Member, and one whose family has long been associated with the Institution—the great Cavendish having been one of its first Managers.

The Managers appreciate the great services rendered by His Grace the Duke of Devonshire in the promotion of Science, more especially in its application to Industrial Development.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with the Duchess of Devonshire and the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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WEEKLY EVENING MEETING,

Friday, May 8, 1908.

SIR WILLIAM CROOKES, D.Sc. F.R.S., Honorary Secretary and
Vice-President, in the Chair.

JOHN YOUNG BUCHANAN, ESQ., M.A. F.R.S. *M.R.I.*

Ice and its Natural History.

IN a single lecture it will be impossible for me to do more than deal with those points in the natural history of ice which have come under my own notice, and which I have made the subject of special investigation.

The Nature of the Ice formed by Freezing Saline Solutions.—During the Antarctic cruise of the “Challenger,” the old question arose as to whether the salt which is always found in the water produced by melting sea ice, was present in the solid state as part of the crystalline ice, or in the liquid state as part of the adhering brine. It was one of considerable economical importance to whalers and other mariners who frequent Polar seas. It had been found that freshly frozen genuine sea-water ice was not drinkable. Genuine land ice, which is easily recognised, gave, of course, good water. The great mass, however, of the ice found floating in Polar seas is of mixed origin. Thus, in its first stage it may be the ice formed by the primary freezing of sea-water. On this falls snow. When there is wind the sea breaks on the edge of the floating ice and throws salt spray over it, which freezes in due time. Hence a piece of sea ice, picked up at random, is likely to be a very heterogeneous substance; and no two pieces can be expected to behave exactly alike. It is, therefore, not surprising that the reports of different navigators regarding the potability of the water formed by melting sea ice differed. Some maintained that it was undrinkable; others held that it could be drunk, if the first portions melted were rejected, and only the last fraction used.

In February 1874, when the “Challenger” reached her furthest south, much ice of all kinds was met with, and I took the opportunity to make a study of the floating sea ice. I collected many samples, observed their melting temperatures, and determined the percentage of chlorine in the water produced by their fusion. The results obtained showed that their melting temperature was very variable, and always below 0°C . It was further observed that this temperature was the lower the greater the percentage of chlorine found in the

melted ice. The advantage of fractional melting for the purpose of obtaining water of less salinity was confirmed; but it was found impossible, by any means, to produce pure water by melting the ice. This, combined with the fact that its melting-point was considerably lower than that of pure ice, was for me convincing evidence, at that date, that the salt was present in it in the solid state, and that, consequently, the crystalline body formed by freezing sea-water and similar saline solutions was not pure ice.

About ten years later Dr. Otto Pettersson's treatise* on the properties of water and ice came before me, and I studied it with great interest. In it he refers to my "Challenger" work, and rejects the view that "sea ice is itself wholly destitute of salts, and only mechanically encloses a certain quantity of unfrozen and concentrated sea-water." I was much gratified to find that he had, quite independently, arrived at the same conclusion as I had.

In the careful study which I made of his work, the following passage, p. 318, arrested my attention: "A thermometer immersed in a mixture of snow and sea-water which is constantly stirred, indicates $-1.8^{\circ}\text{C}.$ " If this statement was exact, it was clear that the evidence furnished by the melting temperature of the sea ice was not entitled to the weight which I had attached to it, and that the conclusion, at which we had both independently arrived, was open to doubt. On making the experiment, I was able to confirm his statement. I thereupon decided to proceed without delay to investigate the subject experimentally.

The question at issue concerned saline solutions generally, and, in the research, which I undertook in 1886, solutions of single salts were considered in the first line, and sea-water was included as a particular case of a composite saline solution.†

The principle which guided the research was the following: if the crystalline body which is formed when a non-saturated saline solution is partially frozen is pure ice, then pure ice of independent origin, such as snow, must, when mixed with the same saline solution, and heat is supplied, melt at the same temperature, when the concentration is the same.

The result of the research was definitively to establish the validity of this principle on experimental evidence.

* On the Properties of Water and Ice; by Dr. Otto Pettersson: Publications of the "Vega" expedition, 1883.

† The results of the research, which I began in the year 1886, were communicated to the Royal Society of Edinburgh in a paper on Ice and Brines, which was read on March 21, 1887, and was published in the Proceedings of the Society, xiv. pp. 129-149. A full account of it was also published in 'Nature,' 1887, xxxv. p. 608, and xxxvi. p. 9. The whole subject of the influence of dissolved salt on the state of aggregation of the substance H_2O at temperatures below its normal freezing and melting point, and above its normal boiling and condensing point was passed in review in my Chemical and Physical Notes in the 'Antarctic Manual, 1901,' pp. 73-108.

It was found that when a non-saturated saline solution is gradually frozen, certain crystals, which we call ice-crystals, separate out ; and, during this process, the temperature of the mixture gradually falls, while its concentration increases. When this mixture was warmed, the crystals gradually melted, the temperature rose, and the concentration of the solution diminished. When, in the process of cooling and freezing, the temperature had fallen to a certain point, say t and the salinity to s , it was found, when this process was reversed, and the same temperature t was reached during the process of warming and melting, that the solution had regained the same salinity s . Therefore, the substance which forms the ice-crystals which separate out at a temperature t during cooling, melts again at the same temperature during warming ; and the concentration of the solution at that temperature is the same whether the temperature is arrived at by cooling or warming.

When a solution of the same salt, having a higher concentration than that which was used in the previous experiment, was cooled to nearly 0°C ., and was then mixed with sufficient freshly fallen snow to form a sludge, and heat was supplied, there arrived a moment in the course of the melting when the temperature of the mixture had returned to t , the concentration of the brine was then found to be represented by the same salinity s , which had been found to correspond to the temperature t in the previous operation. Therefore, the snow melted in exactly the same way as did the ice-crystals which had been formed by the freezing of the solution itself, and at the same temperature for the same concentration.

But it was only a question whether the salt was in the ice or in the brine. There is no salt in snow ; yet it behaved in the saline solution in the same way as the crystals formed by freezing that solution ; **therefore, the crystals formed by freezing the saline solution must be equally free from salt with the snow.**

It was thus proved that the crystals formed in freezing a non-saturated saline solution are pure ice, and that the salt from which they cannot be freed does belong to the adhering brine.

After the main principle had been established, the determination of the temperature at which snow or comminuted ice melts in saline solutions was substituted for the determination of their freezing-points ; and in the case of many of the common salts and acids, even at high concentrations, yielded results which agreed with those previously determined by others by the more laborious freezing method.

The evidence on which Dr. Pettersson arrived at the conclusion that the ice, formed on freezing saline solutions, contains salt otherwise than as mechanically enclosed brine, was furnished by his analyses of sea-water and of melted sea ice, brought home from the Arctic regions. In these samples he found that the ratio $\text{Cl} : \text{SO}_3$ in the samples of sea-water was, as was to be expected, almost constant,

while, in those of the sea ice, it varied within very wide limits; and he justly observes that, if it is claimed that the ice of sea-water is salt only in virtue of adhering sea-water of slightly greater concentration, the saline contents of the water adhering to the ice crystals must have the same composition as that of the water before freezing. Having found that this was not the case, he was apparently justified in concluding that the act of freezing in sea-water has a selective or distributive effect on its saline ingredients. But, in freezing, the solution is separated only into two parts, the crystals and the mother-liquor. If there is found to be a deficiency of a saline ingredient in the mother-liquor, this can be due only to its transference to the crystals. Therefore, the crystals must contain salt solidly.

This reasoning is perfectly valid if we admit what is tacitly assumed, that the samples of ice analysed were produced by the *primary* freezing of sea-water of the composition of the samples of unaltered sea-water, which were analysed at the same time. But it was perfectly evident to me that this was not the case, especially as regarded the samples brought home by the "Vega" which were characteristic samples of the ice encountered by a ship having her Arctic experience. From the records of this expedition, which agree with those of all other Arctic voyages, primary sea ice is to be seen only at the beginning of winter, or the commencement of freezing. Almost before the ice is a day old secondary changes take place and become more and more pronounced as the season advances and the ice thickens. An excellent idea of the nature of these changes is furnished by Dr. Pettersson's investigations and by those of Dr. Karl Weyprecht.*

But the question of the nature of the ice formed by freezing non-saturated saline solutions can be solved only by the phenomena attending their primary congelation, where all secondary complications are excluded. I therefore made a number of experiments in which different samples of sea-water, in sufficient quantity (300 grm.) were frozen in a bath having a temperature between two and three degrees lower than the probable freezing-point of the sea-water. Freezing was continued until about one-third of the solution had passed into the crystalline state. The mother-liquor was drawn off, as perfectly as possible, before removing the beaker, in which the freezing had been effected, from the bath. The crystals were transferred quickly to a funnel, and drained by means of the jet pump. The chlorine and sulphuric acid were then determined in each fraction, and the ratio $\text{Cl} : \text{SO}_3$ calculated ($\text{Cl} = 100$). As an example, the following values of this ratio were obtained for water from the Firth of Forth: In the original water 11.83, in the mother-liquor 11.67, and in the crystals 11.62. In water from the Firth of Clyde the ratios were, in the original water 11.58, in the mother-liquor 11.57, and in the

* Die Metamorphosen des Polareises, Wien, 1879.

crystals 11.67. These ratios agree with each other as closely as the analytical possibilities permit.

Inasmuch as the chlorides and sulphates together make up more than 99 per cent. of the solid contents of sea-water, the constancy of these ratios makes it so probable as to be certain that the ice of sea-water is salt only in virtue of adhering sea-water of slightly increased concentration.

It follows, therefore, that the evidence furnished by the quantitative freezing of a composite saline solution, confirms the conclusion arrived at on the basis of the identity of the temperature at which a saline solution freezes, with that at which ice melts in it; namely, that the crystalline body formed by freezing a non-saturated saline solution is pure ice.

It was not until after this had been established, in 1887, that it became legitimate to say: "The freezing-point of *water* is lowered by the presence of salt dissolved in it," instead of saying: "The freezing-point of a *saline solution* is so much lower than that of pure water." The former of these statements expresses the fundamental principle of cryometric chemistry.

Distinction between the Melting-Point of a Substance and the Temperature at which it melts under given conditions.—An important consequence of this research was that the melting temperature of a body is not necessarily identical with the temperature at which it melts under particular circumstances.

I define the freezing-point of a substance to be: The temperature at which it, as a liquid, passes into itself as a solid; and its melting-point to be the temperature at which it, as a solid, passes into itself as a liquid.

Under "substance" I understand a single substance, completely defined as a chemical individual. A good example of what I mean is afforded by the substance, of which eighteen parts, by weight, consist of two parts of hydrogen and sixteen parts of oxygen, which have combined with the liberation of a quantity of heat sufficient to raise the temperature of the product by some 2000° C. In chemistry this substance is expressed by the symbol H_2O . In the solid state it is called ice, in the liquid state water, and in the gaseous state steam.

If we have a quantity of this substance, partly in the solid and partly in the liquid state, and in such conditions that, if ever so little heat be removed from the mixture, the quantity of ice is increased, and, if ever so little heat be added to the mixture, the quantity of ice is diminished and that of the water correspondingly increased, the temperature of the mixture is the freezing and melting temperature of the substance H_2O .

When ice is melting in a mixture of ice and water, immersed in a melting-bath, the temperature of the water must be a little higher than that of the ice, else there would be no inducement for heat to

pass from the water to the ice ; similarly, when ice is being formed in a mixture of ice and water, immersed in a freezing-bath, the temperature of the water must be a little lower than that of the ice produced. Therefore, the observed freezing temperature of water, and melting temperature of ice must always be different, if quite exactly determined. But it is found that, by supplying heat at as low a temperature as possible in the melting experiment, and removing it at as high a temperature as possible in the freezing experiment, the observed melting and freezing temperatures of the substance H_2O approach each other more and more closely. Therefore, as a matter of experiment alone, it is legitimate to conclude that the limiting values of these temperatures are identical. It is called 0° on the thermometric scales of Réaumur and Celsius, and 32° on that of Fahrenheit.

This temperature was for long held to be invariable ; indeed, it is little more than half a century since it was established that it is lowered by increase of pressure and raised by relief of the same, the quantitative proportion being that of $1^\circ C.$ to 135 atmospheres.

In the above definition of the freezing and melting temperatures of H_2O , that substance is specified as being present partly in the solid and partly in the liquid state. **The temperature at which ice begins to take form in water which is cooled when in contact only with itself, or with a solid other than ice, has not been determined, and is in fact uncertain.** The moment the smallest particle of ice is present, the water has the opportunity of **passing, as a liquid, into itself as a solid** : but not till then.

Evidence of the uncertainty which exists regarding the temperature at which ice begins to form in water, when it is cooled in contact only with a solid other than ice, is furnished by the wet-bulb thermometer when it is being prepared for use at temperatures below $0^\circ C.$, by freezing on it the quantity of water which is supported, against gravity, by the **perfectly clean** bulb. When this is rotated in air of -10° to $-20^\circ C.$ ice never begins to form until the temperature of the bulb of the thermometer has fallen to -2° or $-3^\circ C.$, and rarely before it has fallen to $-4^\circ C.$ In many cases I have observed it fall to temperatures as low as -8° or $-9^\circ C.$; and in such cases, when freezing begins, the whole drop of water is frozen without its being able, by the liberation of latent heat alone, to raise the temperature of the bulb of the thermometer to $0^\circ C.$

In our definition of the freezing and melting temperature of H_2O , no substance is specified except H_2O . But H_2O , that is, absolutely pure water, rarely, if ever, occurs in nature. Therefore our definition is not directly applicable to water as it occurs in nature. It has been found by experiment that when, in a mixture of ice and water, the water contains ever so little foreign, especially saline, matter in solution, the temperature at which it freezes, and that at which pure ice melts in it, is lowered. It is therefore probable that **in Nature, ice never melts and water never**

freezes exactly at 0°C . In fact the temperature at which ice melts in nature depends on the medium in which it melts as well as on the pressure to which it is subjected. If the pressure is constant, it varies with the nature of the medium; and, if the nature of the medium is constant, it varies with the pressure.

The effect produced by both these agencies is the same in kind: each, by its presence, induces the melting of ice and the freezing of water at a lower temperature than would be the case in its absence. In the case of dissolved salt this inducing power is active only at temperatures lying between 0°C . and the cryohydric temperature of the salt. Between these temperatures the solid salt, when exposed to an atmosphere which is not perfectly dry, is deliquescent ('Antarctic Manual' p. 81). Below the cryohydric temperature ice and salt are indifferent to each other. Of the two agencies the one which has the more potent influence on the natural history of ice is the nature of the medium in which it freezes or melts.

The Influence of Salt in inducing the Melting of Ice at Temperatures between 0°C . and its Cryohydric Point furnishes a quantitative explanation of observed Anomalies in its Physical Constants.—This influence furnishes a simple and natural explanation of the anomalies so often noticed in the physical behaviour of ice. Thus the belief that ice, at temperatures near 0°C ., does not contract but expands on being cooled, has been maintained by such experienced observers as Hugi and Petzold as well as Pettersson. It is impossible to arrive at this conclusion without close and accurate observation. The observations in each case were exact, but the interpretation of them was faulty.

When due weight is given to the influence of the medium, the anomaly disappears, and it is found that ice does not behave in the capricious way supposed, but conforms to the usual custom by expanding when warmed and contracting when cooled. The disturbing agency is the impurity which is present in even the purest water. This is excluded from the ice in the process of freezing, and remains in solution in the residual water, which becomes more and more concentrated as the freezing proceeds. The amount of such solution, which remains liquid at any time, depends on the temperature of the ice and liquid, and the concentration of the liquid or solution. When these are given, and the nature of the dissolved impurity is known, the amount of liquid present and the consequent contraction follow necessarily.

In order to illustrate this, it is necessary to select some substance as representative impurity. Chloride of sodium has been chosen, because it is the most widely disseminated and the best studied ingredient of natural waters.

The following extract from my paper on Ice and Brines, pp. 143-146, explains this in detail. "All natural waters, including rain-

water, contain some foreign and usually saline ingredients. If we take chloride of sodium as the type of such ingredients, and suppose a water to contain a quantity of this salt, equivalent to one part by weight of chlorine in a million parts of water, then we shall have a solution containing 0.0001 per cent. of chlorine, and it would begin to freeze and to deposit pure ice at a temperature of $-0.0001^{\circ}\text{C}.$; and it would continue to do so until, say, 999,000 parts of water had been deposited as ice. There would then remain 1000 parts of residual water, which would retain the salt, and would contain, therefore, 0.1 per cent. of chlorine, and would not freeze until the temperature had fallen to $-0.1^{\circ}\text{C}.$ This water would then deposit ice at temperatures becoming progressively lower, until when 900 more parts of ice had been deposited, we should have 100 parts residual water, or brine, as it may now be called, containing 1 per cent. of chlorine and remaining liquid at temperatures above $-1.0^{\circ}\text{C}.$ When 90 more parts of ice had been deposited we should have 10 parts of concentrated brine containing 10 per cent. of chlorine and remaining liquid as low as $-13^{\circ}\text{C}.$ In the case imagined we assume the saline contents to consist of NaCl only, and with further concentration the cryohydrate would no doubt separate out and the mass become really solid. On reversing the operation, that is, warming the ice just formed, we should, when the temperature had risen to about $-13^{\circ}\text{C}.$, have 999,990 parts of ice and 10 of brine containing 10 per cent. of chlorine. Now, owing to the remarkable fact that pure ice in contact with a saline solution melts at a temperature which depends on the nature and the amount of the salt in the solution, and is identical with the temperature at which ice separates from a solution of the same composition on cooling, the brine liquefies more and more ice at progressively rising temperatures, until, as before, when the temperature of the mass has risen to $-0.1^{\circ}\text{C}.$ it consists of 999,000 parts of ice and 1000 parts of liquid water containing 1 part of chlorine. The remainder of the ice will melt at a temperature gradually rising from -0.1 to $0.0^{\circ}\text{C}.$

“The consideration of this example furnishes an easy explanation of the anomalous behaviour of ice formed from anything but the very purest distilled water, in the neighbourhood of its melting-point. This subject has been studied with great care and thoroughness by Pettersson. The apparent expansion of all but the very purest ice, when cooled below $0^{\circ}\text{C}.$, is ascribed by him in part to solid saline contents of the ice, which exercise a disturbing and unexplained influence on its physical properties. Viewed in the light of the fact that the presence of even the smallest quantity of saline matter in solution prevents the formation of ice at $0^{\circ}\text{C}.$, and promotes its liquefaction at temperatures below $0^{\circ}\text{C}.$, we see that this apparent expansion of the ice on cooling is probably due to the fact that we are dealing, not with homogeneous solid ice, but with a mixture of ice and saline solutions. As the temperature falls this solution deposits more and

more ice, and its volume increases. But the increase of volume is due to the formation of ice out of water, and not to the expansion of a crystalline solid already formed.

"In Table IX. are given the volumes occupied by the ice (with enclosed brine) formed by freezing 100,000 c.c. (at 0° C.) of a water containing chloride of sodium equivalent to 7 grams chlorine in 1,000,000 c.c. (at 0° C.).

TABLE IX.—WATER CONTAINING 7 PARTS CHLORINE IN 1,000,000.

Temp. °C.	Water Frozen. c.c.	Ice Formed. c.c.	Brine remaining. c.c.	Ice and Brine. c.c.	Pettersson III. Vol. of Ice at T. c.c.	Differ- ence. c.c.
T	V ₁	v ₁	V ₂	v ₂	P	P - v ₂
-0·07	99,000	107,979	1000	108,979	108,980	1
-0·10	99,300	108,306	700	109,006	109,007	1
-0·15	99,533	108,561	467	109,028	109,038	10
-0·20	99,650	108,687	350	109,037	109,048	11
-0·40	99,825	108,879	175	109,054	109,057	3

"The volume (v_2) of the ice and brine formed on freezing this water is compared with (P) that observed by Pettersson in freezing a sample of the distilled water in ordinary use in the laboratory. It will be seen that the volumes observed agree very closely with those calculated for a water containing 7 parts of chlorine in a million, on the assumption that the saline matter is contained entirely in adhering liquid brine."

In the same paper (pp. 135, 136), the very low values found by Dr. Pettersson for the latent heat of fusion of different samples of ice prepared by freezing sea-water, were quantitatively explained on the basis of the above law; and the conclusion was experimentally confirmed by the thermal exchanges which were observed to take place in the freezing of a sample of sea-water, as the result of which chemical analysis showed that the ratio Cl : SO₃ was the same in the original water, in the ice formed, and in the brine remaining.

I give these quotations from my papers of 1887 at length because, though published so many years ago, they appear to have been but little read. I know of no manual of physical chemistry in which the above demonstration of the fundamental fact of cryoscopic chemistry is given, nor am I acquainted with any recent treatise on natural ice in which the influence of the nature of the medium, in which it melts, on the melting temperature of ice at constant pressure is even mentioned, still less taken into account.

For these reasons, in preparing the lecture, I have given the greatest space to this all-important subject, and I proceed with its development.

In Table I. we have, under v , the volume, in cubic centimetres, of ice, the melting of which is induced at the temperature t by the presence of 1.5105 gram chloride of sodium. The values of v are derived from determinations of the freezing-point of solutions of chloride of sodium. Under w ($= 0.9167 v$), we have the volume of water so produced, and under c ($= v - w$), the contraction due to the melting. The cryohydric temperature of solution of chloride of sodium is taken as -21.72 , and its concentration as 29.97 grams salt in 100 grams water.

The coefficient of cubic dilatation by heat of pure ice is taken as 0.00016, and it is assumed to be constant at the temperatures under consideration. The specific gravity of pure ice at t referred to that of water at the same temperature, is taken as 0.9167. The volume of the salt diffused through the ice is disregarded.

Using these constants, and those of Table I., we will apply the principle to the discussion of the apparent variations of volume of a block of ice, the volume of which at 0°C . is 1000 c.c. It contains diffused through it 1.5105 gram NaCl, which we assume to be provisionally in the *inert* state, in which it is deprived of the power to induce the melting of ice at temperatures between 0°C . and -21.72°C . Let the temperature of the block containing the inert NaCl be reduced to -25°C .; its volume will be reduced to 996.000 c.c., and as the temperature is below the cryohydric temperature, the salt is by nature inert; at such temperatures ice and common salt are indifferent to each other. Let the temperature of the block of ice be now raised to -22° ; the salt remains inert, and the volume of the ice increases to 996.48 c.c. If the temperature is further increased to -21.721 , the NaCl will still remain inert, and the volume of the ice will become 996.525 c.c.

If the heating is continued the temperature rises exactly to the cryohydric point, -21.72 , at which temperature the indifference of chloride of sodium to ice ceases, and induced melting at that temperature takes place. It will then be observed that the temperature remains constant for a time, while the volume of the block diminishes. When the temperature begins to rise, the volume of ice melted will be 5.498 c.c. As this produces 5.040 c.c. water, the diminution of volume is 0.458 c.c., and the apparent volume of the block is 996.067 c.c.

Let us now go back to the initial state, in which we have the block of 1000 c.c. ice, containing 1.5105 gram inert NaCl diffused through it, at the temperature 0°C . Let the temperature be reduced to -21°C ., the ice remaining inert. The volume of the ice will then be 996.64 c.c. Let the NaCl recover its activity, it will melt 5.629 c.c. ice, producing 5.160 c.c. water under a contraction of 0.469 c.c., so that the apparent volume of the ice at -21°C . is $996.64 - 0.469 = 996.171$ c.c.

By the aid of Table I. and the other constants we can calculate

the composition of a block of ice of any weight or volume, which contains, diffused through it, the constant quantity 1.5105 gram NaCl. The results of such a calculation are given in Table II. for a block of ice having the volume 1000 c.c. at 0°, the salt being supposed *inert*.

TABLE I.

t	v	w	$c = v - w$	t	v	w	$c = v - w$
°C.	c.c.	c.c.	c.c.	°C.	c.c.	c.c.	c.c.
-21.72	5.498	5.040	0.458	-10	10.437	9.567	0.870
-21	5.629	5.160	.469	-9	11.327	10.373	0.944
-20	5.825	5.340	.485	-8	12.532	11.488	1.044
-19	6.035	5.532	.503	-7	14.195	13.012	1.183
-18	6.284	5.760	.524	-6	16.406	15.039	1.367
-17	6.598	6.048	.550	-5	19.538	17.910	1.628
-16	6.978	6.396	.582	-4	23.946	21.951	1.995
-15	7.409	6.792	.617	-3	31.685	29.045	2.640
-14	7.858	7.203	.655	-2	48.352	44.323	4.029
-13	8.372	7.674	.698	-1	98.352	90.156	8.196
-12	8.964	8.217	.747	-0.5	198.350	181.821	16.529
-11	9.648	8.844	.804				

TABLE II.—1000 C.C. ICE + 1.5105 GRAM NaCl.

t	V	v	w	c	U	II
°C.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.
-25	996.000	996.000	996.000
-22	996.480	996.480	996.480
-21.721	996.525	996.520	996.520
-21.72	996.525	5.498	5.040	0.458	996.067	991.022
-21	996.640	5.629	5.160	0.469	996.171	991.011
-20	996.800	5.825	5.340	0.485	996.315	990.975
-15	997.600	7.409	6.792	0.617	996.983	990.191
-10	998.400	10.437	9.567	0.870	997.530	987.963
-8	998.720	12.532	11.488	1.044	997.676	986.188
-7.2	998.848	13.884	12.727	1.157	997.691	984.964
-7.0	998.880	14.195	13.012	1.183	997.697	984.685
-6.8	998.912	14.628	13.409	1.219	997.693	984.284
-6.0	999.040	16.406	15.039	1.367	997.673	982.634
-4	999.360	23.946	21.951	1.995	997.365	975.414
-2	999.680	48.352	44.323	4.029	995.651	951.328
-1	999.840	98.350	90.156	8.196	991.644	901.488
-0.1	999.984	998.350	915.154	83.196	916.788	1.634
0	1000	1000	916.7	83.3	916.7	0

Proceeding by steps in this way we obtain the numbers to be found under U in Table II. In it $V = V_0(1 + 0.00016 t)$ represents the volume of pure ice at t , the salt being inert; v , w and c have

the same signification as in Table I. $H (= V - v)$ represents the volume of pure ice included in U . It will be seen that H , the volume of real ice present at t , suffers a sudden diminution at $-21^{\circ}\cdot72$, and then continues to diminish, at first slowly, and then at an increasing rate, until liquefaction is complete.

In Fig. 1, curve B represents the variation of apparent volume U with temperature t , the salt being active; while the straight line Ba represents the variation of \bar{V} with t , the salt being inert. The line Ba represents the dilatation of pure ice, on the basis of a constant coefficient of dilatation $0\cdot00016$. As chloride of sodium is, by nature, inert at temperatures below the cryohydric point, the straight line bc is part of the curve B , as well as of Ba . Between -25° and $-21^{\circ}\cdot72$ the ice expands uniformly at the same rate, whether it is pure or contains salt. We have in c the first singular point of the curve, and it occurs in all the curves of expansion of ice contaminated by chloride of sodium. When the temperature of the ice rises to $-21^{\circ}\cdot72$, the inertness of the salt is exchanged for activity; and, if the requisite supply of heat is available, $5\cdot498$ c.c. of ice are melted, producing $5\cdot040$ c.c. water, under a contraction of $0\cdot458$ c.c. While this amount of ice is melting, the temperature remains constant, but the volume U contracts. Graphically, this is represented by a straight line (ce) parallel to the axis of volume. Therefore the curve of volume of the ice between -25° and the point where the temperature begins to rise above $-21^{\circ}\cdot72$ is represented by two straight lines bc and ce which meet each other in an acute angle at c . When the temperature rises above $-21^{\circ}\cdot72$, another, generally acute, angle is formed at e , so that this portion of the curve of volumes takes the form of the letter Z .

Between -25° and $-21^{\circ}\cdot721$ the coefficient of dilatation is $0\cdot00016$. At the point c , corresponding in Table II. to the temperature $-21^{\circ}\cdot721$, the cryohydric point has been reached on a rising gradient, but no melting has taken place. Melting is supposed to begin only when the temperature has reached $-21^{\circ}\cdot72$ exactly. Then there is a contraction of $0\cdot458$ c.c. with no change of temperature, so that, at this stage the coefficient of dilatation is $-\frac{0\cdot458}{0} = -\infty$.

When the temperature rises above $-21^{\circ}\cdot72$, the apparent volume U increases with the temperature, but at a gradually diminishing rate until, at $-7^{\circ}\cdot0$, the increase of volume due to simple expansion of the ice is exactly balanced by the contraction due to induced melting. At this temperature the coefficient of expansion changes sign, and, between $-7^{\circ}\cdot0$ and $-0^{\circ}\cdot1$, at which the ice has practically all melted, the coefficient of expansion is negative. Therefore the coefficient of thermal expansion of this ice changes sign three times when it is warmed from a temperature below the cryohydric point of solution of chloride of sodium to that at which liquefaction is complete.

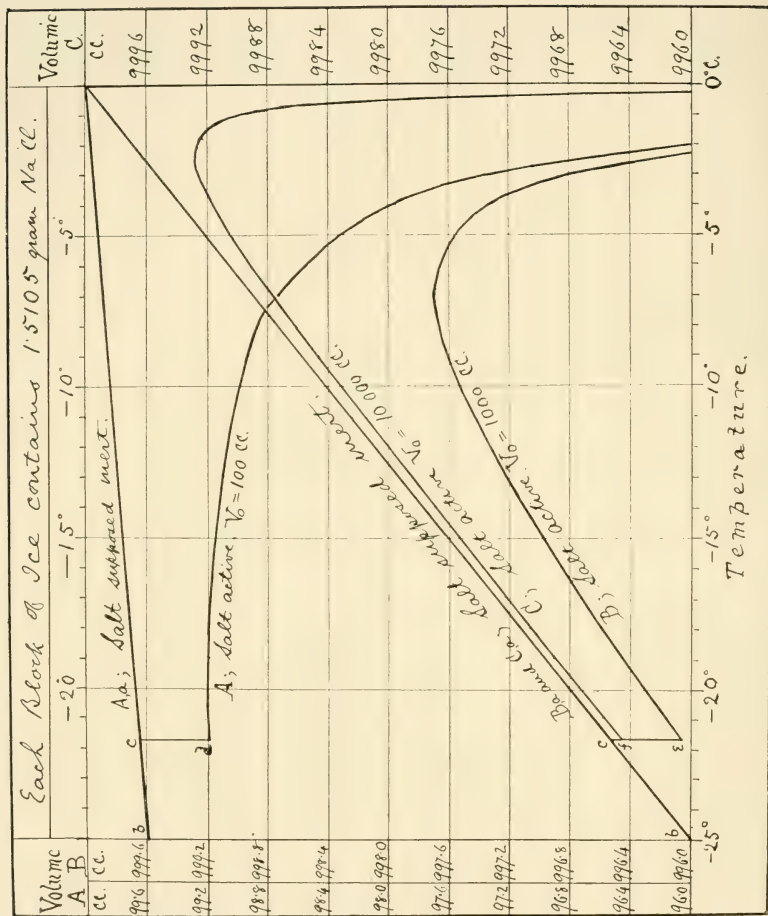


FIG. 1. THE THERMAL EXPANSION OF ICE CONTAINING 1.5105 GRAM NaCl.

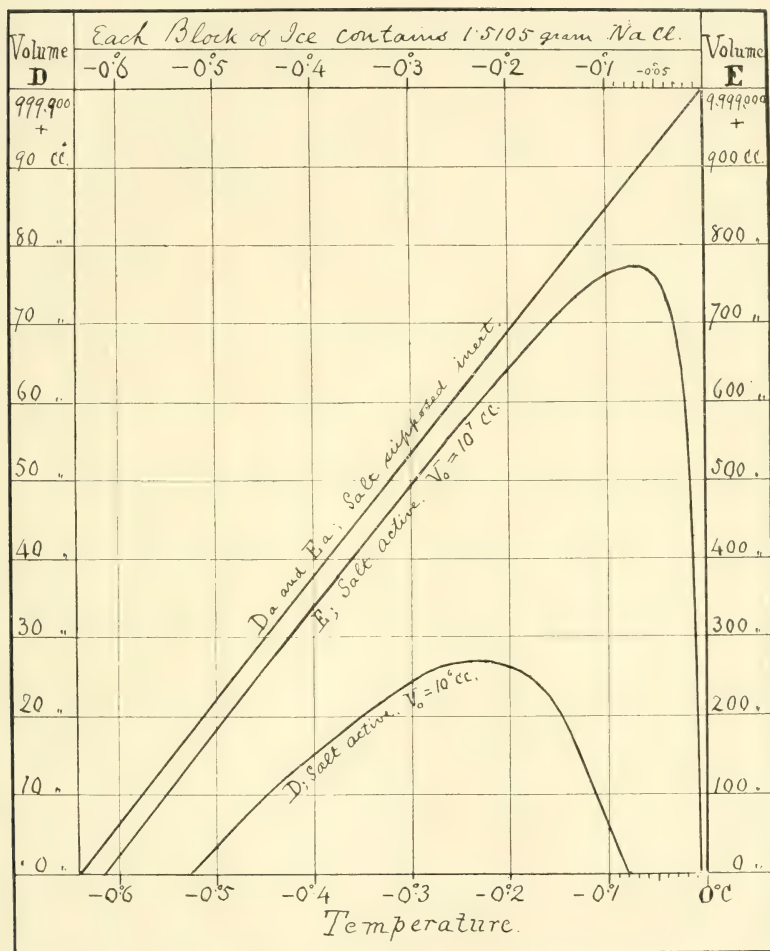


FIG. 2. THE THERMAL EXPANSION OF ICE CONTAINING 1.5105 GRAM NaCl.

Curves C and Ca refer to ice of which 9167 grams or 10,000 c.c. at 0° C. contain 1·5105 gram NaCl. Above the Z-like portion the curve shows a maximum volume of 9992·834 c.c. at -2°·3. At temperatures higher than this the coefficient of apparent expansion is negative.

In the same figure curves A and Aa represent the volumes of a block of ice weighing 91·67 grams and containing 1·5105 gram NaCl diffused through it. The line Aa represents its volume V at temperature t, the salt being inert; at 0° the volume is 100 c.c. The curve A, after the Z-like portion, shows a maximum volume at -20°·5, above which its apparent volume diminishes, as the temperature rises, and at -1°·0 the ice is practically all melted.

When 1·5105 gram NaCl is diffused in a block of ice weighing 88 grams, then the volume of the ice, the salt being inert, is 96 c.c. at 0°; and when the salt is active, the maximum volume is at the cryohydric temperature, so that from -21°·72, the apparent volume U diminishes as the temperature rises. Therefore for blocks of ice which contain, per 100 parts by weight of ice, less than 29·97, and more than 1·7164 parts of NaCl, the coefficient of apparent expansion is negative at all temperatures above -21°·72.

In Fig. 2 we have curves D and Da, and E and Ea. The same constant quantity of chloride of sodium, 1·5105 gram, is diffused, in the case of D, in 1 cubic metre, and, in that of E, in 10 cubic metres of ice. When melted these blocks of ice would furnish waters containing chlorine in the proportion of 1 gram to 1 ton of water in D and to 10 tons of water in E. The critical temperature at which the coefficient of expansion changes sign is -0°·2275 in D and -0°·0725 in E.

In Table III. we have the upper critical temperature (τ) at which the coefficient of apparent dilatation changes sign for blocks of ice having volumes ranging from 100 cubic centimetres to 100 cubic metres, each containing 1·5105 gram NaCl. Under V_0 we have the initial volume of the block of ice supposed pure and solid at 0°C., and under v the volume of ice which can be melted under the inducing influence of 1·5105 gram of chloride of sodium at the

TABLE III.

V_0	v	τ	V_0	v	τ	V_0	v	τ
c.c.	c.c.	°C.	c.c.	c.c.	°C.	c.m.	c.c.	°C.
100	5·73	-20·5	1000	14·20	-7·0	0·01	41·83	-2·3
200	6·74	-16·6	2000	20·00	-4·9	0·1	136·3	-0·725
400	9·85	-10·75	4000	27·80	-3·5	1·0	438	-0·2275
600	11·75	-8·65	6000	32·24	-2·95	10·0	1377	-0·0725
800	12·85	-7·8	8000	37·57	-2·55	100·0	4306	-0·02275

critical temperature τ , at which the apparent coefficient of cubic expansion of the ice is equal to 0.

A block of 100 c.c. of ice, which contains 1.5105 gram of NaCl diffused through it, furnishes on being melted 91.67 c.c. of water, which contain 0.9167 gram of chlorine, dissolved in it as chloride of sodium. This water contains chlorine in the proportion 1 gram to 100 grams of water, and represents a concentration about one-half that of average sea-water. When the volume of ice, V_0 , is 1 cubic metre, the water produced by its melting contains chlorine in the proportion of one part to one million parts of water by weight.

Waters which contain dissolved matter equivalent to no more than 1 gram of chlorine in 10,000 grams of water are in the category of ordinary fresh waters, and we see that the critical temperature of ice which furnishes such water lies as low as $-2^{\circ}.3$. When the dissolved matter is equivalent only to 1 gram of chlorine in 100,000 grams of water, the critical temperature is $-0^{\circ}.725$. The other waters are in the category of distilled waters, and it is doubtful if, by any chemical means whatever, we could determine as little dissolved matter as 1 gram chlorine in one ton of water; yet the critical temperature of such ice lies nearly a quarter of a degree below the melting temperature of pure ice. The critical temperature of expansion of ice affords a means of detecting impurity equivalent to quantities of chlorine as small as one gram in ten tons, and even one gram in one hundred tons of water.

In his work on *The Properties of Water and Ice*, Pettersson gives in Plate XXI. the curves of the change of volume with temperature, of three samples of ice frozen from samples of melted sea ice. They are numbered in descending order of concentration, VI., V. and IV. For the details, which are interesting, the reader must consult the original work. The nature of these samples is roughly indicated by their specific gravity at 0°C. , referred to that of distilled water at 4°C. , and by the percentage of chlorine in them.

No.	VI.	V.	IV.
Specific gravity	1.00940	1.00539	1.00030
Per cent. chlorine	0.649	0.273	0.014

I calculated by the method which I have above developed, the volume of ice at 0° , containing 1.5105 gram chloride of sodium, which would show the same volumetric behaviour as these samples. The results are given in Table IV.

Pettersson's volumes (P) for certain whole degrees of temperature (t) are taken. The water which agreed with the volumetric behaviour of each of Pettersson's samples was found empirically, and the volume of the ice at each temperature, t , for each sample calculated as in

TABLE IV.

<i>t</i>	-18°	-14°	-12°	-10°	-8°	-6°	-3°
Sea ice VI. $V_0 = 145$ c.c.							
U	144·058	144·020	143·975	143·898	143·770	143·494	..
B	..	108,363	108,330	108,272	108,175	107,968	..
P	108,383	108,358	108,330	108,261	108,148	107,923	..
B-P	..	5	0	11	27	43	..
Sea ice V. $V_0 = 250$ c.c.							
U	..	248·785	248·773	248·730	248·636	248·393	..
B	108,634	108,615	108,574	108,468	..
P	..	108,640	108,630	108,615	108,572	108,485	..
B-P	4	0	2	-17	..
Sea ice IV. $V_0 = 8000$ c.c.							
U	..	7981·435	7983·893	7986·330	7988·716	7990·953	7993·520
B	..	108,683	108,717	108,750	108,782	108,813	..
P	..	108,695	108,729	108,761	108,790	108,815	108,847
B-P	..	-12	-12	-11	-12	-2	..

Table II. The temperature, *t*, at which Pettersson observed the volume to be a maximum, was the chief guide. The volumes obtained by my method are found in line U. I then took my volume and Pettersson's volume for the temperature at or near the maximum of volume, and obtained a factor by which I reduced my volumes at the other temperatures to the same denomination as those of Pettersson. These are given in line B, those of Pettersson being given in line P. It will be seen that the agreement between calculation and observation is very good.

Cryoscopic Equivalence between Pressure and Salinity.—We have seen that the freezing-point of water and melting-point of ice is lowered by increase of pressure and by addition of salt in solution. The effect produced by these agencies is the same in kind.

When water, which holds salt in solution, freezes at a temperature below 0° C., the salt forms no part of the ice produced. But it is the cause of the lowering of the freezing-point, because, if it is removed, the freezing-point reverts to the normal. The depressed melting-point of the ice depends for its persistence on its continued contact with the saline solution from which it sprang.

When water, which is subjected to any pressure, freezes at a lower temperature than it does in a vacuum, the pressure, being immaterial, cannot form part of the ice. But it is the cause of the lowering of the freezing-point; because, on its removal, the freezing-point reverts to the normal. The depression of the freezing-point depends for its persistence on the continuance of the pressure to which the ice is exposed. We can explain its action only by describing it. It acts by

influence. This influence, however, is subject to quantitative law which has been well investigated.

On the same grounds it seems to be legitimate to say that the salt in solution acts also by influence; and its influence is subject to quantitative law which has been well investigated.

As both influences are subject to known laws, we are able to ascertain the equivalence which exists between them.

Further, the pressure, being immaterial, is of one kind, while the salt, which is material, is of as many kinds as there are chemically individual substances soluble in water.

In considering the two agencies, we must compare a certain absolute pressure with a certain weight of a particular salt. In this discourse we have generally considered the constant quantity 1.5105 gram chloride of sodium. The generally accepted pressure which lowers the freezing-point of water by 1°C . is 135 atmospheres, which is here taken as equivalent to 140 kilograms per square centimetre (kg/cm^2). If we consider the surface of the water, which is exposed to the pressure as 100 square centimetres, then the total pressure required is 14,000 kg. We therefore take as our constant absolute pressure 14,000 kg. When the distilled water exposes to it a surface of 100 cm^2 its freezing temperature is lowered by 1°C . If the lowering of the freezing-point of water by pressure were simply proportional to the pressure, then, when the surface exposed to the pressure of 14,000 kg. is $s\text{ cm}^2$, the lowering of the freezing-point would be given by t in Table V. It results, however, from the experiments of Tammann* that proportionality of the lowering of the freezing-point of water to the pressure to which it is exposed, is of the same order as that of the lowering of the freezing-point of water to the amount of salt dissolved in it. In Table V., s' gives the surface (cm^2) of water which must be exposed to a pressure of 14,000 kg. in order that, according to Tammann, its freezing-point may be lowered by t degrees. The corresponding volumes of ice melted, at ordinary pressure, under the influence of 1.5105 gram NaCl, are given by v . To be strictly comparable with s and s' , the numbers v should be increased in the ratio $98.352:100$; but this has been considered unnecessary.

It will be seen that the *surface* of the water exposed to the constant pressure, 14,000 kilograms, is roughly proportional to the *volume* of the ice exposed to the influence of 1.5105 gram NaCl, and to the same volume of water holding in solution 1.6475 gram NaCl, which contains 1 gram chlorine. The proportionality is the closer the greater the surface of the water compressed and the greater the volume of ice which contains the salt.

In comparing the power of inducing the freezing of water at

* Ueber die Grenzen des festen Zustandes, von G. Tammann, Annalen der Physik. [4], ii. 1.

TABLE V.

Lowering of freez- ing-point	t	1	2	5	10	15	20	21·72
		cm. ²	cm. ²	cm. ²	cm. ²	cm. ²	cm. ²	cm. ²
Surface of water exposed to pres- sure of 14,000 kilograms	s'	100	50·3	20·05	11·7	8·3	6·64	6·13
	s	100	50	20	10	6·67	5·0	4·55
Volume of ice con- taining 1·5105 gram NaCl	v	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.	c.c.
		98·35	48·35	19·54	10·44	7·41	5·82	5·50

temperatures below 0° C., possessed by a given absolute pressure and by a given mass of a particular salt, we see that the following law holds: **The surface of the water exposed to the pressure and the volume of the water which holds the salt in solution are approximately proportional.**

It was shown in my paper On Steam and Brines,* that the elevation of the boiling-point of water by pressure, and by dissolved salt, follows the same law: **there is the same approximate proportionality between the surface which supports the pressure and the volume which holds the salt.**

Influence of Impurity on the Apparent Latent Heat of Ice.—This is illustrated by the numbers in Table II. Thus, at -1° C., the apparent volume of the block of ice is 991·644 c.c., and it is made up of 901·49 c.c. ice and 90·154 c.c. water. When this is warmed to $-0^{\circ}\cdot 1$, we may take it that the whole of the ice is melted. Taking the latent heat per unit volume of ice as $66\cdot 5$ at $-0^{\circ}\cdot 1$, and its specific heat per unit volume as $0\cdot 45$, the heat required to raise the ice from -1° to $-0^{\circ}\cdot 1$ is 365·1 gram-degrees ($g.^{\circ}$); that required to raise the temperature of the water by the same amount is $81\cdot 14 g.^{\circ}$, and the heat required to melt the ice at $-0^{\circ}\cdot 1$ is 59,949 $g.^{\circ}$, the total heat used being 60,395·2 $g.^{\circ}$. If we ignore the possibility of partial melting, and assume that we have 999·84 c.c. solid ice at -1° , and that its temperature is raised to 0° , at which temperature it melts, we have the following expenditure of heat: for rise of temperature 449·9 $g.^{\circ}$, and for melting 66,489·3 $g.^{\circ}$, making together 66,939·2 $g.^{\circ}$, as against 60,391·5 $g.^{\circ}$. If from 60,395·2 $g.^{\circ}$ we deduct the heat calculated for warming the ice in the second case, 449·9 $g.^{\circ}$, we obtain 59,945·4 $g.^{\circ}$ as the heat required to melt 1000 c.c., or 916·7 grams, of ice at 0° , whence the latent heat would be, per unit volume, 59·94, and per unit weight 65·39.

* On Steam and Brines, by J. Y. Buchanan, F.R.S., Transactions of the Royal Society of Edinburgh (1899), xxxix. p. 549.

This example illustrates also the effect of impurity on the apparent specific heat of ice.

It will thus be seen how powerful is the influence of medium on the behaviour of ice in the laboratory: it will now be shown that, in nature, this influence is equally powerful and much more far-reaching.

The nature of the medium is responsible, in the case of sea ice, for depressions of freezing and melting temperature of 30, 40, and even more degrees of Celsius' thermometer, while the greatest pressure to which fresh-water ice is exposed in nature, cannot well produce an alteration of freezing and melting-point amounting to as many hundredths of a degree.

The ice which surrounded the "Vega" during her winter's imprisonment in the Arctic Ocean, had a pasty semi-liquid consistence, although the temperature of the air was at or below -30°C . It remained stationary only because it was on a level surface. Had it been shovelled up on an inclined plane it would have quickly flowed down it until it reached the lowest level again. If we pick up a piece of ice floating in the Polar Sea, we know that it will prove to be very far from homogeneous. It may have a foundation of genuine primary sea ice: but the ice forming the superstructure is sure to consist of snow, frozen spray, and very likely fragments of land ice, all cemented together into a species of conglomerate. We have seen that when this is exposed to warmth it begins to melt, at a temperature which may be one or two degrees below the melting-point of pure ice; and the liquid furnished by the melting is salt water. The further melting takes place in the ascending order of temperature: the salt ice of low melting-point disappearing first, and the purer ice melting later. We thus see how ice can be cemented by ice, just as metallic objects may be united by solder. In both cases the binding material differs from the objects united, chiefly in being more easily fusible.

If we have a number of cubes of pure ice, which fit each other exactly, and, if after being moistened with salt water they are exposed to frost, they will solidify to a single block. If this be exposed to the sun, the cementing salt ice will melt first, and, when it ceases to bind, the constituent cubes of pure ice will fall asunder, having themselves suffered practically no diminution due to melting.

The Glacier Grains.—Now this is precisely what happens when a block of sound glacier ice is exposed to the rays of the sun for a short time: and it is one of the most striking and instructive experiments that can be made. Under the influence of the sun's rays, the binding material melts first, the continuity of the block is destroyed, the individual grains become loose and rattle if the block be shaken, and finally they fall into a heap. A block of glacier ice is a geometrical curiosity. It consists of a number of solid bodies of different sizes and of quite irregular shapes, yet they fit into each other as exactly and fill space as completely as could the cubes above referred to.

The granular constitution of glacier-ice can nowhere be better

studied than at the Mergelin See, the well-known lake which exists so long as the Aletsch glacier maintains a watertight dam across the little side valley, which its waters occupy. It is roughly triangular in shape. As the ice of the glacier is subject to more or less disintegration, there are always icebergs and small fragments of ice floating on its waters. The portions projecting above the water are exposed in summer often to a very powerful sun, and are loosened by the radiant heat into their constituent grains. A lump lifted out is found to consist of these disarticulated grains which rattle when shaken, and in a strong sun fall to pieces.

Size of Glacier Grains.—In August 1895 I made an extended study of the structure of glacier ice, principally from the Aletsch glacier. The fragments of this glacier, which float as icebergs in the Mergelin See, are exposed to the powerful weathering influence of the summer sun, and are comparatively easily dissected into their constituent grains. A number of blocks were so dissected, in order to ascertain the weight and size of the largest grains. The following weights of single grains were determined: 700, 590, 450, 270, 255, 170, 155, and 100 grams. It was observed that blocks of ice contained grains of all sizes, which fitted each other so exactly that, in the fresh unweathered block, the whole volume was filled with ice.

The following table (p. 20) gives a summary of the results of dissecting seven blocks and weighing the grains.

The figures in the table do not give an exact statistical account of the blocks of ice. The smallest grains have most frequently escaped being weighed, therefore the average size of the grain comes out higher than the truth. On the other hand all were melting rapidly, so that the grains when on the balance weighed less than they did one hour before, and much less than they did the day before. The figures in the table give a general idea of the constitution or anatomy of a block of ice taken from the lower part of a large glacier. They are particularly interesting when we reflect that every grain, even the largest, has grown, according to the rigid laws of crystallomorphic development, from a single snow crystal which probably weighed no more than one or two centigrammes.

The action of the sun's rays on glacier ice is twofold; it disarticulates the ice into its constituent grains, and it splits the individual grain into laminae perpendicular to the principal axis of the crystal and bounded by the planes of fusion described by Tyndall. These planes are the distinguishing characteristic of the individual ice-grain.

Under the influence of radiant heat an ice-crystal begins to melt at the contiguous surfaces of these laminae, and the process of disintegration and decay is directed by their plane. On the other hand, an ice-crystal, floating in water and losing heat, generates ice laminae which are directed by the same planes, which form the continuation of the corresponding laminae of the parent crystal. This was well observed at the end of August 1895. Every night a thin skin of ice

Number of Grains Weighed.	Aggregate Weight of Grains.	Average Weight of one grain.	Number of Grains Weighed.	Aggregate Weight of Grains.	Average Weight of one grain.
	A			d	
16	1045	65.3	7	400	57.1
10	110	11.0	10	30	3.0
4	25	6.25			
10	25	2.5	17	430	25.3
40	1205	30.1			
	B		22	E	
			10	1125	51.1
11	1095	99.5	10	150	15.0
10	95	9.5	10	135	13.5
23	60	2.6	10	120	12.0
			10	60	6.0
			10	50	5.0
44	1250	28.5	10	40	4.0
			10	20	2.0
			10	15	1.5
	C		102	1715	16.8
15	1365	91.0			
5	50	10.0			
10	25	2.5			
30	1440	48.0	34	F	
			10	5765	169.6
			10	190	19.0
			10	190	19.0
			10	140	14.0
6	1090	182.0	6	60	10.0
5	30	6.0	40	70	1.75
11	1120	102.0	110	6415	58.3

was formed at the shallow end of the lake, where the ice blocks were collected. As the grains in a block of glacier ice are distributed quite irregularly, the water line of a floating block necessarily cuts a great number of grains, all of which are oriented differently. The ice which was formed during the night along this line was oriented crystallographically by the grain with which it was in contact and from which it appeared to spring in continuation of its crystalline laminae. This produces a remarkable pattern of lines on the surface of the lake ice contiguous to a block of glacier ice.

Tyndall has described and figured the minute features of the disintegration of the crystal under the absorption of radiant heat. Similar and complementary features are observed when ice is generated from an existing crystal under the dissipation of heat. To do justice to them, however, would require the services of a skilful, patient and resourceful artist.

The disarticulating and analysing action of the sun's rays is not accomplished without selection and the expenditure of energy. Accordingly we observe that one grain protects another. The dis-



FIG. 3. A GALLERY IN THE GROTTA OF THE MORTERATSCH GLACIER
IN JANUARY 1907.



FIG. 4. SOLAR ETCHING OF ICE IN THE MORTERATSCH GROTTA
IN SEPTEMBER 1907.

articulation into separate grains, although very thorough near the surface of a glacier, does not penetrate far. A stroke or two with an ice-axe reveals the fresh blue ice. The analysis of the individual grain into crystallographically oriented laminae can be particularly well studied in the Mergelin See. It is only the grains that are exposed to the sky, and above water, that are so analysed; and prolonged exposure of this kind reduces a grain to the last stage of dilapidation. The grains beneath the surface, *whether of ice or water*, are almost completely unattacked.

The importance of direct sky-light for the disarticulation of glacier ice into its constituent grains is very well seen in the artificial grottos which are maintained at easily accessible parts of most popular glaciers. One of the galleries in the grotto of the Morteratsch glacier is represented in Fig. 3. It is from a photograph which I took in January 1907. The hoar-frost on the roof and the sharp line where it ceases on the walls are well shown. The white object on the left is a stone working its way out through the ice. Many of these were visible in the body of the ice. The thickness of the layer of completely disarticulated ice is so small that it is hardly noticed, and the whole grotto appears to be cut out of pure blue ice. If the observer, on penetrating for a few paces, turns round and looks outwards, he sees the surface of the ice-walls of the grotto etched with strange linear figures. These are most strongly marked near the opening, and they extend as far as direct sky-light strikes the ice. The lines so developed are formed by the intersection of the surface of the ice-wall of the cave with the separating surfaces of contiguous ice-grains. The photographic picture thus presented is one of very great interest. The illustration, Fig. 4, shows this etching on a buttress in the grotto of the Morteratsch glacier, taken in September 1907.

After the autumnal equinox very little melting of ice takes place, and by the end of October it has, as a rule, ceased entirely. The etched figures on the walls of the entrance of the grotto, which were developed during summer, disappear quickly with the arrival of winter. But the winter brings with it another means of delineation of the grain which does not depend on solar radiation. Even at the lowest of winter temperatures, the atmosphere contains vapour of water, which it is prepared to relinquish under the same conditions as those under which dew is formed in summer. In the Alpine winter, however, it is deposited not as dew but as rime, that is, not as water but as ice. It is well known that very fine etching on a polished surface, which can with difficulty be seen without assistance, at once becomes visible if the surface be breathed on. In winter, the walls and roof of the grotto are cold, dry, smooth and polished like glass. The winter air entering from without and circulating in the grotto *breathes* on the polished surface of ice and develops the figure of the ice by the rime which is deposited on it. As rime always settles by preference

on sharp edges, it seeks out the lines of separation between the grains and settles on them, showing the whole granular structure. In January 1907, there was a wonderful exhibition of this natural damascening on the roof of the cave of the Morteratsch glacier; in January 1908, however, it was quite inferior and would not have struck the eye. The illustration, Fig. 5, represents a portion of the roof of the cave, which I photographed in January 1907. As the roof is not flat, but made up of shell-like cavities, worn by the hot air in summer, the delineation of the grain is sharp in some parts of the photograph and faint in others.

A precisely similar phenomenon was observed in 1886 by Professor Forel, in the remarkable natural grotto of the Arolla glacier, of which he has given so fascinating a description in the 'Archives des Sciences Physiques et Naturelles,' Genève, 1887, xvii. p. 498. The delineation of the etched figures by rime was observed by him in the month of July in a remote and secluded chamber nearly 250 metres from the entrance of the grotto. In artificial grottos, like that of the Morteratsch glacier, in which the air circulates freely, the hoar frost disappears very quickly with the end of winter.

Sun-Weathering of Granular Ice Produces White Surface of Glacier.—The surface features of glaciers cannot be better studied anywhere than on the Aletsch glacier, which is the largest in Switzerland. From the Mergeln See up to the Concordia hut, the surface is without danger, and is easily travelled. If the glacier be here crossed, the beaten track of the mountaineers is left, and, near the north side, the ice, though perfectly smooth and almost level, is quite untrodden. I often made expeditions alone over this part of the glacier in the summer of 1895, and frequently met with the remains of dead animals of different kinds, chiefly birds. At one place I fell in with what was evidently a family of chamois which had perished on the ice. There were the two parents and the kid. One of the parents and the kid were lying on a ridge of ice, and, having remained dry, were in the condition of mummies, with their skins drawn tightly over them. The other parent had been lying in a furrow, and had been completely macerated, leaving its skeleton in a broken up condition. I was unable to arrive at a satisfactory solution of how these animals had met their death. It was evident, however, that beasts or birds of prey must be rare, else the remains would not have been so preserved. As a souvenir, I collected a number of the vertebræ of the macerated individual, and took them home. In picking them up, I was much struck with their resemblance to the disarticulated grains of a block of glacier ice; or, rather, it struck me that they were the only objects which I had seen, with which I could compare the grains in respect of their outward form. Just as the vertebræ of the chamois fit exactly to each other, to form the vertebral column of the animal, so do the grains of the glacier fit exactly into each other to form a compact block. The vertebræ

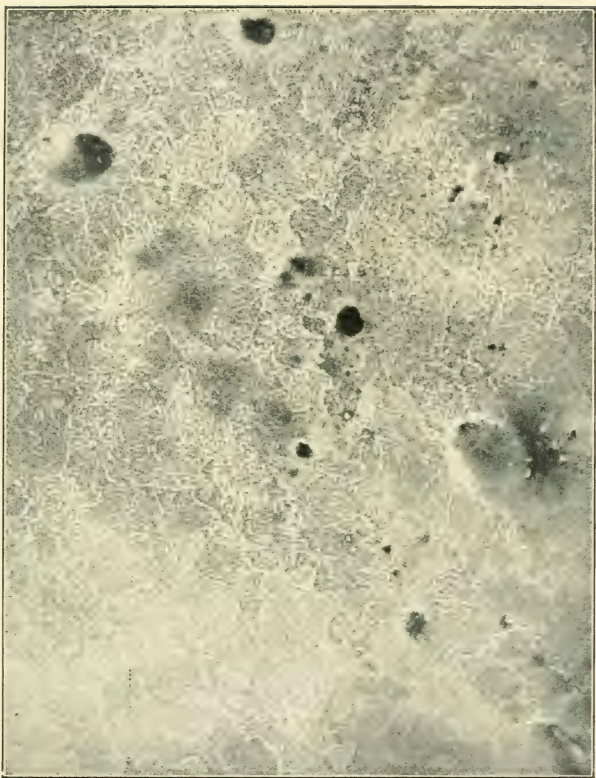


FIG. 5. ETCHING OF ICE BY HOAR-FROST: MORTERATSCH
GROTTO, JANUARY 1907.

are united and held together by ligaments, the grains of the glacier are united by an aqueous cement, which has a slightly lower melting temperature than their own.

When we walk on the glacier we crush under foot nothing but the grains of the glacier, which have been loosened for our benefit by the radiant energy of the sun. If the white surface layer of the disintegrated ice be chipped away with an ice-axe, so as to expose a smooth surface of blue ice, in the course of a single summer's day, this smooth blue surface will become as white and crumbling as any other part of the surface of the glacier. If it were not for this interaction between the solar rays and the granular ice, traversing a glacier in summer would be almost an impossibility.

Snow Nevé and Glacier.—In the lowlands, snow falls and melts again, and we have no opportunity of witnessing the metamorphoses which it may experience when lying for a long time on the ground. It is otherwise with the snow which falls in the high mountains. There the temperature of the air is nearly always below the melting-point, and the snow may remain for years without reaching that temperature. It is often assumed that the higher we climb amongst the mountains, the greater is the quantity of snow which falls in the year. But this is a mistake. In the Alps the greatest amount of snow falls at a height of from 2000 to 2500 metres above the sea. The crystalline snow of the mountains takes the granular form much more easily than does the flaky snow of the lowlands. The snow that falls on glaciers in the winter melts and disappears during summer like that on the neighbouring lands. Snow in the higher regions which has persisted through a summer passes into firn or névé. This is always in clearly granular form.

It is to Hugi that we owe most of our exact knowledge and detailed description of the névé or firn, of its genesis and of its metamorphoses. He built a hut on the Finsteraarfirn at an elevation of 3500 metres, and inhabited it for a considerable time for the sole purpose of studying the firn or névé and its natural history. He traces the development of the névé from the fine crystalline snow of the highest levels, and observes it as it passes into glacier. At a height of 3000 metres the transformation has taken place at a depth of 7 metres below the surface of the névé; at an elevation of 2700 metres it is met with at a depth of a few feet, and at a height of 2400 metres the névé has passed into glacier at the surface. In experimenting on the névé, he found that when a hard compact mass of it was exposed to the influence of rising temperature, the binding material of the grains soon dissolved to water without the grains themselves being apparently attacked at all. A lower temperature then reunites the grains so that the whole appears as a uniform compact mass. This shows the lower melting-point of the less pure cementing mass of ice. He sums up the whole history of the development of the glacier in a remarkable passage (p. 73) of his work

Ueber das Wesen der Gletscher. All the changes which we witness taking place on the incline between the most elevated névé and the lowest extremity of the glacier in the valley, are repeated on the vertical, between the upper and the under surfaces of the névé and the glacier. In both directions we observe greater age and more definite development of the mass. Further, what we observe in both these directions we observe also in the individual grain. The older kernel of the névé is compact and blue like the lower glacier, while the white spongy rind on the outside is more of the nature of snow, like the highest névé, and passes by layers into the compact central grain. Also in the case of the individual grain, the nucleus or kernel is the first and oldest, and only by continued development does the rind shape itself and gradually pass into the mass of the nucleus and so become a glacier-grain, which then continues its development as the glacier itself continues its own development. In these relations lies the foundation of the whole natural history of the glacier. It will be observed that eighty years ago Hugi held very modern ideas on the subject of development.

The Grain of Lake Ice.—It is not glacier ice alone which suffers disintegration when exposed to a powerful sun. Lake ice behaves in a similar way. Beautiful examples of this can be seen in Alpine seas every winter. During the harvesting of the ice from the lake, the blocks often lie for a day or more before they are carted away to the ice houses. Occasionally some of them get overlooked and remain for many days exposed to the powerful sun of February, while maintaining the low temperature of the air usual in that month. No melting takes place; but, after even a few hours' exposure to the sun, the block shows the figure of its grain in development. It is being etched by the sun's radiation.

The grain of lake ice has a very different appearance from that of glacier ice, but both are individual crystals. The difference in their appearance is to be traced to the difference of treatment which they have received during their existence. The glacier grains have been practically rolling over each other during their descent, while those of the lake have established themselves at right angles to the surface of the water and have remained there. So long as the ice is increasing in thickness, the temperature of its upper surface is very low. It is perfectly transparent, and its surface is smooth, dry and polished like glass, and it shows no trace of crystalline figure. When the ice is undisturbed, this develops itself only at the end of the season when the thaw sets in. Then the whole ice sheet rises to its melting temperature and is at the same time exposed to the direct radiation of the sun. This produces disarticulation of the ice into groups of vertical prisms, which are then floating independently: they are kept together only by crowding. Ice in this state is said to be rotten; and it will be recognised that, however thick the ice sheet may be, when it gets into this condition, it is dangerous. In the neighbourhood of the

outflow the crowding is relieved. The disarticulated groups become disengaged, the smaller groups and individual prisms are able to assume their attitude of stability and to float on their sides. All then drift towards the outlet. The ice breaks up, and the lake is cleared in an astonishingly short time.

If it were not for the law that even impure water in freezing always forms pure ice, the impurity remaining in the liquid and generally entangled in the interstices of the grains, and that the pure ice which is in contact with this impure liquid, melts at a lower temperature than that which is in contact with nothing but the water formed by its own melting, the ice covering a lake would be a continuous sheet offering no points of weakness, and it would have to melt as a whole. It is doubtful if lakes such as those met with in the upper Engadine would get rid of their ice covering at all. On the Silser See the ice is usually over 60 centimetres thick when the thaw sets in, but when once the ice begins to break up, the lake is cleared in a day. Sixty centimetres of ice would take a long time to disappear on the basis of surface melting alone.

While the winter lasts the ice on the lake shows no crystalline structure. This develops only after removal from the water and exposure to the sun. The ice then splits up into prisms in a vertical plane. These are at first of irregular section, and as sun-weathering proceeds the thicker prisms split up into thinner. When a block has lain exposed to the February sun and cold, it may fall to pieces, each piece being a long, thin triangular prism, with some resemblance to a razor blade. When the ice is cold and dry, the outlines of the grains are lines; when the ice has a temperature of 0°C. , it melts perfectly round the grain, forming troughs in which the water collects, and the aspect is that of a dark polygon, surrounded by light-coloured canals. In one piece, which was much weathered, I counted 24 such grains in an area of 9 square centimetres. In a slab which had not been lying long, I counted 23 grains in an area of 150 square centimetres, giving an average area of 6.5 square centimetres per grain; the largest had an area of 12 square centimetres. In another slab there was a very large grain which measured 7 centimetres in one direction and 4 centimetres at right angles to it. In a slab in which the sun-weathering had proceeded very far, I counted 113 grains in a disc of 5 centimetres radius, which gives 0.69 square centimetre as the average area per grain.

In the absence of actual experience, one is apt to expect a slab of lake ice, when subjected to sun-weathering, to be disarticulated into hexagonal columns; but this expectation is quite gratuitous. Ice may crystallise in a form bounded by plane faces, according to the laws of its crystallographic system if it has the freedom which it possesses when crystallising out of an independent medium such as a saline solution or air. But the foreign matter dissolved in fresh water is present in so small quantity that what we have before

us is the solidification rather than the crystallisation of ice, and each column as it tries to develop itself is interfered with by its neighbour, and the resulting slab of ice is made up of elementary prisms crowded together, but preserving parallelism of crystallographic axis.

Characteristics of an Advancing Glacier.—In the month of August 1895 I visited the valley of Chamounix, and had the good fortune to find that the Glacier des Bossons was advancing. This was particularly noticeable when crossing the moraine on its eastern flank. The ice was everywhere undermining it, and keeping it generally on the move. On the western side, where the glacier terminated in several points, these acted like ploughshares, in turning over the detritus in front of them. The glacier stream escaped from an *Antrum*, having a remarkably small entrance, about 2 metres high and 3 metres wide.

In pursuance of certain experiments which I was making regarding the melting of glaciers, I entered the antrum and bored a nearly vertical hole, about 12 centimetres deep and $1\frac{1}{2}$ centimetre wide, in the ice of the eastern wall. It was within a metre of the entrance, but so situated that the water produced by the melting of the surface ice, which poured over the entrance, could not reach it.

Although everywhere there appeared to be melting, the hole remained empty and dry; even the snow-turnings which remained in the hole were quite dry. It was still perfectly dry when I left the glacier at 6 p.m. When I visited it again the next day at 3.45 p.m. it was still perfectly dry. The stream flowing out of the antrum was so voluminous that it was impossible to explore it that afternoon. The next day, however, the 15th, at 11 a.m., the volume of water was small, and it was possible to penetrate into the antrum. It ran into the ice in a straight line for 21 metres, and terminated in a rock 2 metres high by 3 metres broad, which filled the whole area of the cavern.

Grooving of Ice by Rock.—While making these measurements, my attention was arrested by an appearance on the roof of the cave, close to where the top of the rock bore against the ice. From the line of contact, and for a distance of about half a metre towards the entrance, the ice was deeply scored and grooved. At first I could hardly believe it; but there was no doubt about it. Although I had often heard of glacial action, the idea of glacial *reaction* had never occurred to me, still less had I ever expected to witness it. The experience that in an auger hole bored in the ice within a metre of the entrance, and in very hot weather, the delicate ice-turnings which were left in it remained unmelted for at least twenty-four hours, made it no longer wonderful that the surface of the ice at a distance of 20 metres further from the entrance, should remain unmelted long enough to show the effect on it of the rock over which, on evidence external to the glacier, it was passing with

quite sensible velocity. In fact, the antrum of the Glacier des Bossons, in the summer of 1895, offered an example on a large scale of a cold-pressed tube, of which the material was ice, which yielded perfectly to continued pressure, while the rock was the resisting button.

It will be readily recognised that the scoring and grooving of the ice surface by the opposing rock can be witnessed only when a glacier is advancing, and, in summer, only in localities adequately protected from melting agency.

For years I imagined that I was the only person who had seen glacial action thus reversed, but later, when I acquired Hugi's works and after I had studied them carefully, I found that he had observed this, as well as almost everything else that it is possible to see in a glacier. Before starting on his memorable winter expedition to the Eismeer, he had the position of the lower extremities of both the upper and the lower glacier of Grindelwald exactly marked out, so as to be able, on his return, to ascertain, by direct measurement, any advance or retreat which had taken place. His way to the Eismeer lay along the flank of the upper glacier which at that date, January 1832, was advancing in great volume into the plain where it spread itself out like a fan, and he observed that on its western side the motion of the glacier was opposed by a mass of rock. It pushed itself over this rock with great energy, and for a distance of 41 feet down the valley the surface of the ice, which had passed over the rock, was deeply scored and grooved. It was observed that the glacier shoved itself over the rock at the rate of $5\frac{1}{2}$ or 6 inches per day: it was also noticed that the ice on the eastern and opposite side of the glacier hardly moved at all, not more than a few inches in three weeks. It was, however, reported in the following spring by the man whom Hugi employed to observe the glacier daily, that in February the western flank of the glacier became stationary, while the eastern flank pushed forward, digging up great masses of stone and earth.

This passage, which I have reproduced at length, not only gives valuable objective information about the glacier, it also enables us to form a subjective appreciation of the man who, with nothing but the stipend of a schoolmaster, was able to undertake an enterprise on so large a scale and so difficult as a *Winterreise in das Eismeer*, and who carried it out with such attention to detail as to have the two principal glaciers which were fed by the Eismeer on which he was going to sojourn for a fortnight kept under observation during the whole winter.

In comparing my observation of the striation of the glacier by the rock with that of Hugi, it will be noticed that his was made in January, the coldest month of winter, and in the year 1832, when the glaciers were in the plenitude of their power, while mine was made in August, the hottest month of summer, in the year 1895, when

almost all the glaciers of Switzerland were, and had for long been in an advanced state of decay, the Glacier des Bossons forming, as it turned out, a temporary exception. Owing to the high external temperature, I was obliged to creep into the innermost part of the glacier in order to observe the relatively slight effect of the agency, which, in Hugi's case, produced such a striking effect in the broad light of day. But, notwithstanding the fact that the advance of the Glacier des Bossons which I witnessed in 1895 was slight and temporary, it sufficed to show me the fundamental difference between the effects produced by a glacier on its surroundings, according as it is on the increase or on the decrease. When it is growing the glacier is a living bearer of energy like a river; when it is shrinking, it is inert as a salt lake.

It has been noticed by those who have observed the occasional advance of European glaciers in recent years, that there is little that is gradual about their start. They get under way at once at a fair speed, and proceed without delay in the work of handling the débris that has accumulated in front of them during their repose. When a glacier is really advancing there is no doubt about it; where there is doubt, it may be taken that the advance has not begun.

External Work of a Glacier.—Owing to the enormous diminution in the amount of land-ice in the course of the last half-century, we are accustomed to talk of the retreat of the glaciers. But a glacier never retreats; it stops advancing, and melts where it stands. Even in a stationary glacier, however, the flow of the ice in all its parts continues; but its effect outside of the glacier is almost if not quite *nil*. In the stationary state its function in nature is conservation. In the advancing state it adds the function of distribution. Its destructive effect is very small. It protects the rock beneath it from weathering, which is a chemical process, by the constant maintenance of a low temperature and the practical exclusion of the atmosphere. Any destructive action which it exerts must therefore be mechanical.

When two substances meet each other in mechanical strife it is the harder that wears the softer, and in the strife between rock and ice Nature makes no exception to this law. We have seen from Hugi's description of a strife between rock and ice which went on under his own eyes, what the effect was on the ice, which was at a temperature much below that of melting. In a week or two, however, the evidence of the effect on the ice would be obliterated, great though it was, while the reactive effect on the rock would remain, but it is doubtful if it would be perceptible. In the battle between ice and rock the ice suffers much; the rock comes out with a scar or two. The scars abide, but the destruction disappears and leaves no record; hence the neglect of the major effect and the exaggerated importance generally attached to the minor.

It has been said above that when the glacier begins to advance, it performs a distributive function by digging up and pushing before it

the rock débris which it finds in front of it in the valley. This débris consists partly of matter which has been brought down by the glacier at some previous time when its dimensions were greater, and partly from the general wasting of rock on the faces of the mountains, which form the sides of the valley. This is the source of the detritus which gravitates surely into the valley, whether it be occupied by glacier or river. The function of the river also is mainly distributive: it has very little share in the duty of procuring or shaping the pebbles in its bed. The stones in the river-bed are furnished by the chemical process of weathering on the rocks above. This loosens and separates, and, in many cases, rounds the fragments *in situ*. Gravity does the rest. It often helps the disintegration by fracturing blocks, the support of which has been sapped by weathering along the joints, and in all cases it brings the fragments down into the valley so soon as they have lost their support. The river cleans the stones that arrive in its bed. When a flood comes, it pushes on a certain quantity of them, and during each flood the fragments suffer some little attrition. But the greater the flood the more rapidly are the pebbles hurried on towards the region where the stream becomes a depositing rather than a moving agency. No single pebble can be exposed to the wearing action of the flood over a greater distance than that from the spot where it fell from the mountain into the valley to the mouth or beginning of the delta of the stream; the motion is always downwards, always in the same direction, and takes place only in floods. The low water of the stream rather protects than wastes the stones.

The real region of mechanical erosion and attrition is the sea-shore. In comparison with it, every other is insignificant. The power available and spent over it is enormous. This is provided by the winds which blow over the opposing ocean. Their energy is accumulated in the form of undulations by the waves which they generate, and it is carried without sensible dissipation by the waves until they meet the shallow water of the coast, where it is discharged as breakers. These are the symbol of the conversion of the potential energy of waves into the kinetic energy of currents. They sweep the pebbles up the beach, and both return together by their own weight. The work of that wave has been done; but with the extinction of one wave another follows, and so on for ever.

The great potential energy residing in rocks which occupy an elevated position, the imminence of its conversion into kinetic energy, by any, even the least decay of the material, and the far-reaching effects which the conversion can produce, have not received adequate appreciation.

A rock precipice is a seat of weathering, with gravity always at its foot. When weathering has produced decay, and decay has removed support, gravity claims the fragment as its own, and the result is a *talus*.

Whether the precipice sheds a fragment in a year, or a thousand of them in a day, the primary shape of the talus is the same. If there is no lower level for it to descend to, it weathers *in situ*, and its final state is a *pampa*.

Advantage of the Study of Tropical Lands.—The study of all matters relating to surface geology in temperate latitudes is made difficult by the variability of the meteorological conditions. Had the study of physical geography and of meteorology taken its rise in countries within the tropics, advance would have been much quicker. The surface geology of a place depends mainly on the meteorological conditions. If these are simple the dependence on them of the conditions of the surface geology is easily traced. The determining primary features of tropical meteorology are heat and moisture, and these together produce a secondary agency which is all-important, namely intensity of chemical action. This secondary agency is by far the most effective in altering the relief of the surface of the land. Its importance cannot even be guessed by those who have not visited tropical or equatorial regions and studied the soil and the rocks from which it has been formed. An important difference between the climate of tropical and of temperate regions is that, in the former, the wet weather is concentrated into a few months of rainy season, and the dry weather into a few months of dry season, while in the latter there is no such separation; wet and dry weather are distributed indiscriminately throughout the year. One secondary effect of the temperate climate is that the local streams have some water in them all the year round, which may be swelled to floods at any time of the year. In tropical, or rather sub-tropical regions, the stream beds are dry during the greater part of the year; and, in some cases, for whole years together. During the long hot summer the rocks on the mountain sides, which always, even in the driest season, retain moisture below the surface, are eaten into along every discontinuity and loosened up into fragments, which, so long as they remain *in situ*, tend more and more to lose their edges and corners, by the chemical action of weathering. When gravity dislodges and brings them down into the valley, they only require to be hurried along by one flood to come out clean and round. In these places no one can fail to recognise that the function of the river or stream is mainly distributive. Now, whether the river flows only during one or two days in the year, or is perennial with occasional floods, the nature of the action is the same, and there is no difference in the roundness of the stones.

I had the good fortune once to witness an example of this. It was in the island of Tenerife, in October 1883. It was said that there had been no rain for six months, and certainly all the beds of the streams were perfectly dry. During the forenoon clouds collected, and about mid-day there was a thunderstorm with a violent and prolonged deluge of rain. After the rain had been pouring for



FIG. 6A. SAN ANTONIO (C.V.). VIEW OF STREAM-BED AFTER THREE
RAINGLESS YEARS.

(From photograph by H.S.H. Prince Albert I. of Monaco.)

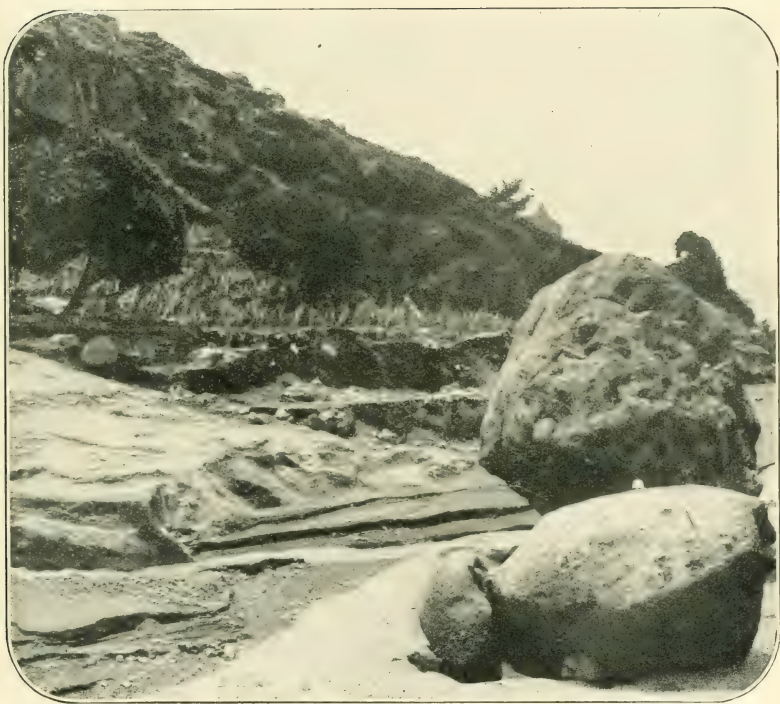


FIG. 6B. THE SAME VIEW ON THE SAME DAY AFTER A VIOLENT
RAIN STORM.

(From photograph by H.S.H. Prince Albert I. of Monaco.)

about half an hour, a strange rattling sound was heard, which every moment grew louder, without it being possible to say what caused it. At length it explained itself by the bed of the stream in front of the anchorage of the ship in which I was, suddenly filling with brown muddy water, descending with great velocity to the sea and charged with vast quantities of stones, sand and mud. The rattling noise was produced by these stones and rocks being driven violently over the rocky bed and against each other by the current, against which nothing could stand. Naturally all the other river beds in the island fared alike. Their accumulations were washed away into the sea likewise. The rocks of Tenerife are volcanic and produce in weathering, besides kaolin, hydrated oxide of iron and other ochreous substances, which have a reddish colour. The consequence was that for nearly a week the island was surrounded by a fringe of red water. The quantity of rain which fell was so great that when it reached the sea it floated on the top and retained in suspension the fine ochreous mud, which would have been precipitated by mixture with the sea-water had the supply of fresh-water been less abundant.

A still more remarkable instance of this kind is related by the Prince of Monaco. At the end of August 1901, he visited the island of San Antonio in the Cape Verde Group. Here rain is very scarce. It was said that it had not rained for three years. He landed opposite the entrance of the valley of Tarrafal and took a photograph, in which the dry river bed, encumbered with rocks and stones, occupied the foreground. About noon a cloud-burst occurred from which it was necessary to take shelter for a considerable time. On returning to the landing-place to rejoin his ship, he found that the stream had cut out a broad bed in the stones more than a metre in depth and precipitated the whole of the *débris* into the sea. He photographed the river-bed after this catastrophe, and the pair of photographs constitutes one of the most important documents in the natural history of denudation. By the kindness of his Highness I am permitted to reproduce these photographs. Fig. 6*a* represents the view in the morning. Dr. Portier, the well-known bacteriologist, is standing on the gravel plain in front of the larger boulder, and the whole of his body is visible over the smaller boulder in the immediate foreground, which is nearly covered by the gravel. Fig. 6*b* represents the same view in the evening, after the stream has done its work of distributing the accumulation of *débris* due to three years' *chemical and gravitational degradation*. All that is now seen of Dr. Portier, over the top of the nearer boulder which has been almost completely undermined by the torrent, is the top of his helmet. The stones in the river-bed in the morning were as round as those occurring in any perennial river, yet it was impossible in this case for the river to have had anything to do with the rounding of them. The sole function of the river in this island is, once every year or so, to clear the bed of rounded stones, not to make them.

It must be observed that, in the island of San Antonio, though rain is rare moisture is abundant, and the valleys are fertile and well cultivated. The moisture condensed on the ground, and retained beneath the surface, aided by the high temperature due to the low latitude, produces ideal conditions for the chemical decomposition of the volcanic rocks of which the island consists, and the consequent production of the best class of soil. This, owing to the rare occurrence of rain, is allowed to remain *in situ*, and contributes to the maintenance of a considerable population. The decomposition of the mineral constituents, which yield to weathering and furnish the soil, undermines the portions of the rock which resist it and removes their support. These then surely gravitate to the lowest level, where their weathering is continued, causing always more and more perfect rounding of the stones, with the production of the equivalent amount of soil in which the stones remain embedded, provided that the process is not interfered with by *running* water.

The "Crumble" Formation.—I never accepted with enthusiasm the teachings of the ultraglacial school of geology, but it was only during the course of the cruise of the "Challenger" that I became convinced that ice is not required to produce denudation. I found in all countries within the tropics, that the rocks were decomposed to a depth of 20 or 30 metres, the resulting material often remaining *in situ* with such a fresh appearance, that it was difficult to imagine that it could be anything but the unaltered rock.

It was only necessary, however, to touch it with a stick, or even with the fingers, for it to crumble into fragments of all sizes down to sand and clay. In my journal almost every new island or place visited is logged as consisting of "the usual *crumble* formation." There could be no doubt about the cause of it. It furnished convincing evidence of the powerful decomposing action of the heat and high vapour tension which characterise the tropical atmosphere. Fig. 7 represents the Chilian town Antofagasta. The configuration of the mountain slopes illustrates the effects of chemical and gravitational degradation in a rainless district. The landscape so produced consists of a succession of *taluses*. The *débris* thus accumulated on the mountain side would be cleared out if it was the side of a valley and a glacier passed along it. Applying this knowledge to the consideration of the conditions in my own country, which were referred to ice action, such as the enormous quantities of gravel in the Spey valley, I came then to the conclusion that the glaciers could only be responsible for clearing the *débris* out of the valleys and distributing it over the plain, that the production of the *débris* itself might be attributed to the occurrence of the last of the usually postulated warm inter-glacial periods, and that the existence of the *débris* furnished the best evidence of the reality of a previous warm period. It is not necessary for the climate during this period to have been anything like as warm as that of the equator; all that is required is moderate warmth and moisture.

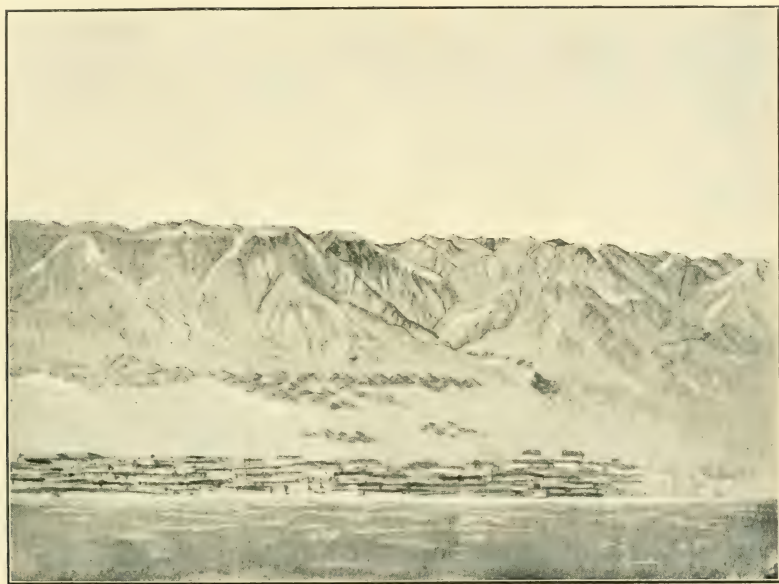


FIG. 7. CHEMICAL AND GRAVITATIONAL DEGRADATION OF ROCK ON
CHILIAN COAST.

I have attributed the presence of fragments of rock, whether great or small, and to a considerable extent their rounded form, to the chemical action of the moisture derived from the atmosphere. Water penetrates, by its own weight, into any cracks or joints which may occur in the rock, and into minute discontinuities of substance by capillarity. It spreads and extends its decomposing influence along the surfaces which lend themselves most readily to the process of soaking. The warmer the rock is, the more energetic is the decomposing action. On the tops of high mountains rock surfaces, exposed to the direct rays of the sun, acquire often, even in winter, a high temperature, and when they are covered by snow, they are protected from excessive cooling by radiation. All rocks have surfaces of relatively imperfect continuity. These are found by water, which enters easily provided the temperature of the surface of the rock is not such as to convert it into ice. Far-spreading decomposition is then only a question of time, and, after it has spread the falling asunder of the parts of the rock under the influence of gravity is a certainty. There is no necessity for assistance by any other agency. **The chemical action of atmospheric moisture and the tendency of every part of a mountain or rock to yield to gravity when not adequately supported, suffice to account for all the degradation of rock which we observe.**

I have not been able to discover the author of it, but it is a very old and generally accepted doctrine, that the rock fragments which are found so frequently covering the tops of mountains are split off from the parent rock by the energy liberated by water freezing in its interstices. I know of no detailed description of the process, nor have I met or read of any one who has actually witnessed a rock being split in this way.

Discontinuities in the rock are postulated in order to admit the water which is to be frozen, but no detailed specification is furnished of how the opening is to be closed and the freezing is to be effected after the water has entered. But it has been shown above that if the water gains admission, it will in time disintegrate the rock by chemical action, and gravity without assistance will complete the degradation. Therefore freezing is unnecessary in order to account for the facts. Moreover, the covering of a mountain top by fragments of its own rock is a common occurrence in latitudes where frost is rare or absent.

In discussing the natural history of ice, it has been necessary to some extent to include that of water and steam. In its physiographic relations we have found that ice is as efficient a preserver of mineral matter as it is of vegetable or animal matter in the everyday relations of life. We have also found that the substance which chemists indicate by the symbol H_2O has the most destructive action on mineral matter when it has passed from the gaseous state in the atmosphere to the liquid state on the surface of the earth, and when this takes

place in regions where the atmospheric temperature is high, chemical decomposition accompanied by disintegration is the result, and it is followed by degradation under the all-pervading influence of gravity. This is the primary process in the wasting of land substance, and we have called it *chemical and gravitational degradation*. The conditions which favour the exhibition of chemical and gravitational degradation are high temperature, abundance of moisture, and scarcity of water. These conditions are met with together in the tropical regions of both continents. The word "tropical" is here used in its restricted sense and excludes "equatorial." The equatorial climate is not only moist but very wet. It also is productive of energetic chemical and gravitational degradation, but its primary effect is profoundly modified by the secondary effect of running water. This modification is equivalent to intensification, in so far as the running water, by removing the products of decomposition, and thus *denuiding* the rock, exposes fresh surfaces to the decomposing agency.

In its simplest and most perfect form the effect of chemical and gravitational degradation, which has been active through all the ages that a distinctly tropical climate has existed, is to be witnessed on the prairie and the pampa.

Here the wayfarer finds himself on a surface as featureless as the ocean, but equally spherical; and he may use his horizon for the purpose of finding his position astronomically with almost as much confidence as he would at sea.

Water finds its level quickly; sea ice, slowly; land ice, more slowly; and land, in a time which, though very great, is still far from infinite.

[J. Y. B.]

WEEKLY EVENING MEETING,

Friday, May 15, 1908.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L.
Sc.D. F.R.S., President, in the Chair.

HERBERT TIMBRELL BULSTRODE, M.A. M.D. D.P.H.

The Present Phase of the Tuberculosis Problem.

THIS lecture will deal with certain aspects of the tuberculosis question.

THE NATURE OF TUBERCULOSIS.

It is desirable, in the first instance, to indicate the sense in which the term tuberculosis is here employed.

Tuberculosis is a diseased condition which is brought about by the inter-reaction of a vegetable parasite—the tubercle bacillus—and a susceptible animal host. When the micro-organism succeeds in obtaining a foothold in the lungs, the disease produced is known as Pulmonary Tuberculosis, or, in popular terminology, Phthisis or Consumption. This pulmonary tuberculosis is divided into “open” and “closed”; “open” when the lung tissue is breaking down and bacilli are expectorated, “closed” when no tubercle bacilli are thus set free. This distinction between “open” and “closed” is of considerable importance, both from an epidemiological and administrative standpoint.

When there is what may be termed an infective reaction between the parasite and the host in the intestines, glands, bones, joints, or other localities, tuberculosis of these parts ensues.

Almost every organ of the body is liable to the disease, and it has to be borne in mind that tuberculosis is in no sense limited to the human species; practically all domesticated animals, the ox, the pig, the horse, the sheep, the cat, the dog, and the fowl, in some degree are liable to develop the malady.

Prolonged residence in the tissues of one or other of these animals has, it may be believed, induced certain changes in the tubercle bacilli. It is probable that they are all allied, and sometimes indeed they are more or less interchangeable either at once or after “passage” through other animals.

There is advantage in laying stress on the circumstance that two factors are necessary to induce tuberculosis, the bacillus and a certain condition of the “soil” or tissues of the host. Since the discovery of

the bacillus, in 1882, there has been, it would seem, a tendency to concentrate attention almost exclusively on the parasite or bacillus, an attitude which, as certain administrative work based entirely upon this view has already shown, is not unlikely to lead to disappointment. The actual position cannot be better set forth than in the words of Professor Osler, who possesses a remarkable aptitude for clothing scientific ideas in the attractive garments of antiquity.

There are tissues in which the bacilli are in all probability killed at once—the seed has fallen by the wayside. There are others, again, in which a lodgment is gained and more or less damage done; but finally the day is with the conservative protecting forces—the seed has fallen upon stony ground. There are tissue soils in which the bacilli grow luxuriantly, caseation and softening, not limitation, and sclerosis prevail, and the day is with the invader—the seed has fallen upon good ground.

The importance of this soil “resistance” has not in the past been sufficiently appreciated, and it is not inconceivable that it may in the relatively near future come to be regarded as the major factor in the tuberculosis problem.

Evidence indicating the significance of this soil factor is forthcoming from the experimental data of the Royal Commission on Tuberculosis, wherein it has been shown that it is not the absolute number of bacilli which supplies the determining factor, but the dose in relation to the susceptibility of the animal. Bacilli from the same source may give rise to general progressive tuberculosis in one animal and to a limited retrogressive tuberculosis in another.

It is important, however, to observe that a sufficiently large dose will usually overcome all resistance.

Illustration of proclivity to the disease may also be seen in the frequency with which tuberculosis of the joints or bones follows some injury, and tuberculosis of the lungs some damaging affection of those organs, while the tendency of the disease to attack at one age period the joints, bones and glands, and at another the lungs, betokens, perhaps, different tissue proclivities at different ages.

Sanatorium treatment aims less at the direct destruction of the bacillus, than at the promotion of such a condition of the “soil” as shall prove hostile to the life of the parasite, and tuberculosis treatment based upon opsonic index values must seemingly be regarded as directed towards the same end. Finally, as will be seen presently, the post-mortem records of persons who have died of other diseases, or been accidentally killed, show conclusively that only a proportion of all existing cases of tuberculosis are recognised during life, so that in the vast majority of infected persons the resistance of the body has overcome the onslaught of the bacillus.

It is difficult, if not impossible, to procure really reliable data as to the susceptibility or resistance of different races of man, but there is some evidence pointing towards the conclusion that such obtains.

For instance, the Irish branch of the Celtic race is thought by some of those who have studied the question to be especially prone to develop the malady, no matter in which part of the world they are found.*

The incidence of tuberculosis upon the Irish at home in Ireland is apparently excessive, "apparently" because there is some doubt whether the increase which the official figures denote is actually the result of increased prevalence of the disease or of some altered nomenclature, or whether the emigration of the non-tuberculous has not altered the relation between the healthy and tuberculous at home, and thus led to an apparent increase in the death-rate. It is interesting in this connection to note that the incidence upon the Irish in America is stated to be higher than upon any other people, and that this excessive incidence is also observed in the offspring (born in America) of Irish parents.

On the other hand, the Hebrew race appears to be relatively immune to this disease, notwithstanding, at times, an environment which amongst non-Hebrew people would be likely to be associated with considerable prevalence of the malady.

Flick, of America, suggests that the history of tuberculosis in all new countries may indicate that the disease has a rise, a climax and a decline, and the epidemiology of certain other diseases would appear to lend support to this view. Still the registration of the causes of death in new countries is not very reliable, and even in old countries it dates back such a very short period that it does not enable us to decide this point with any certainty; and the English statistics, which are admitted on all hands to be the best which the world possesses, do not extend back far enough into the past to indicate the fact of a rise having taken place, and consequently to afford a sure indication of the climax. Nevertheless, it is not improbable that as

* It is a curious fact that this, too, seems to be the case with regard to insanity in Ireland in so far as there seems to be a remarkable parallelism between the behaviour of tuberculosis and insanity among the Irish born.

In the Supplement to the Fifty-fourth Report of the Inspector of Lunatics, which is a special report on the alleged increase of insanity in Ireland, the Inspector writes, after quoting certain American observations:—

"These observations by authorities on the subject in the United States, read in connection with the statistics of insanity in Ireland, point to the conclusion that the Irish branch of the Celtic race is specially predisposed to mental breakdown, and therefore the great increase in the number of registered insane all over the civilised world is for this, as well as other reasons, very marked in Ireland. As to why this should be so we can offer no reasonable explanation; but just as the Irish famine was, apart from its direct effects, responsible for so much physical distress in the country, so it would seem not improbable that the innutritious dietary and other deprivations of the majority of the population of Ireland must, when acting over many generations, have led to impaired nutrition of the nervous system, and in this way have developed in the race those neuropathic and psychopathic tendencies which are the precursors of insanity."

with animal and vegetable species the history of which can be read in geological formations, so with the disease-producing but perishable vegetable micro-organisms, there has been a period when they evolved and devolved, or when, from some sudden change, they disappeared within a short space of time. All we are in a position to say is, that in Europe within historical times certain diseases, such as plague, leprosy, typhus fever and relapsing fever, have apparently almost disappeared, and this without any very direct attack by the ingenuity of man, and, as will be seen shortly, there are indications that in this country the same natural law, if it be one, may be operating to promote the fading away of tuberculosis.

The devolution of certain diseases is probably due in the main to unsuitable environment brought about by improved social conditions, but it is a matter of doubt whether, in the case of diseases such as leprosy and syphilis, the factors of both progressive immunity of the host and exhaustion of the parasite, have not played a material part.

THE BEHAVIOUR OF PULMONARY TUBERCULOSIS (PHTHISIS) IN ENGLAND AND WALES SINCE THE MIDDLE OF THE LAST CENTURY.

In order to clearly apprehend the tuberculosis problem, it is essential to appreciate the manner in which pulmonary tuberculosis has behaved in the entire absence of any direct or conscious measures taken against it; in the absence, that is to say, of a belief that the disease was, as is now believed, communicable from person to person, or that its prevalence was seriously amenable to direct control.

With the view of bringing out this point, there is here reproduced from the official sources of the Registrar-General a chart showing the manner in which the disease in question has steadily declined over practically the whole period to which the chart relates, and it may be said that until quite the close of the last century no conscious measures which could in any way have influenced the figures for the country as a whole were being taken. Since then certain measures—such as notification, education by visits of inspectors as to control of sputum and wholesome living, disinfection of houses, and the erection of sanatoria—have been introduced; but, although such measures have no doubt exercised a beneficial effect in localities where they have been practised, they have not, as yet, been of sufficiently general character to exercise any detectable influence upon the behaviour of a curve representing the death-rate for the whole country.

It is desirable also to bring out the fact that the remarkable fall indicated in Chart I. has been participated in by practically every county in England and Wales, and by North and South Wales, and for this purpose Chart II. is introduced. It will be noted that the top of the black coloration indicates the death-rate in each county for 1871–80; the top of the double cross-hatching that for 1881–90; the top of the single cross-hatching that for 1891–1900.

CHART I.

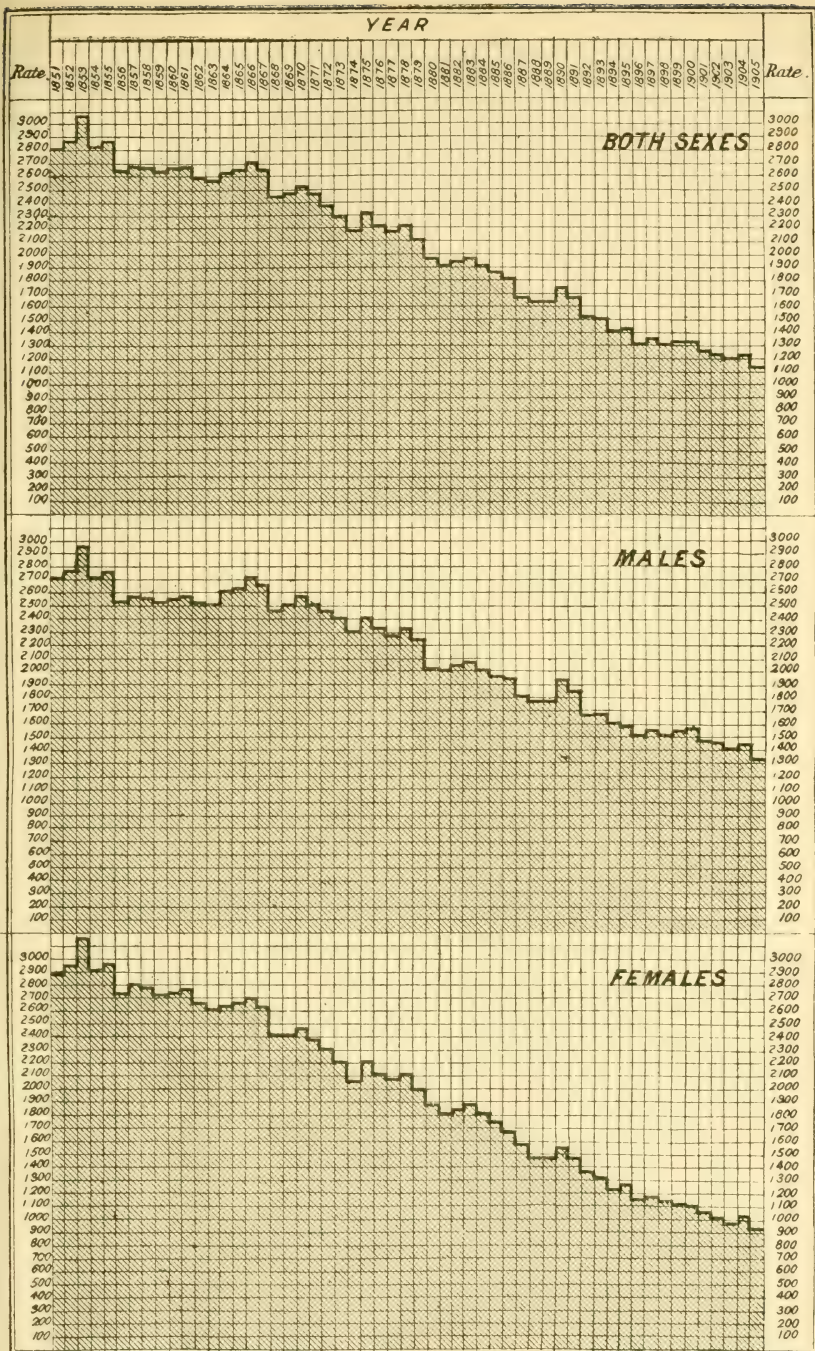


CHART SHOWING ANNUAL CORRECTED DEATH-RATE FROM PHTHISIS IN ENGLAND AND WALES PER 1,000,000 LIVING AT ALL AGES FROM 1851-1905.

(Reproduced from the Sixty-Ninth Annual Report of the Registrar-General, by permission of Dr. Tatham, Superintendent of Statistics, and Mr. Rowland Bailey, M.V.O., H.M. Controller of Stationery.)

As illustrative of the diagram, it may be mentioned that, as regards London, the death-rate fell from 25 per 10,000 in 1871-80 to 18 per 10,000 in 1891-1900; in Cornwall the rate fell during the same interval from 22 to 13 per 10,000; in Lancashire from 25 to 15 per 10,000, and so on throughout the several counties. The fall in some counties has been less than in others, but in all it has been a very material one. The excessive incidence of the disease in North and South Wales will be remarked, as also will the fact that this fall has been great in both divisions of the Principality.

The chart suggests the continued operation over all parts of England and Wales of some force which is not improbably making for the extinction of tuberculosis.

THE SEX-INCIDENCE OF THE DISEASE.

As is well known, there is in the case of many diseases what may, for the want of a better term, be called a sex-proclivity. In some cases, as in cancer, the explanation of this sex-proclivity is easier of explanation than in other diseases; in the case of pulmonary tuberculosis the problem is a difficult one, more especially when regard be had to the fact that in the earlier years of the period dealt with in Chart I. the greatest incidence of the disease was upon females, whereas for many years past, as will be seen by following the respective curves for males and females, by far the heaviest death-rate has been upon the males—the fall in the female curve having been much greater than that in the male; so much so, indeed, that at the present time the death-rate amongst males is about 1300 per million, that amongst females about 900 per million. This sex-incidence is of interest in connection with the subject of communicability, seeing that, on the current ideas as to the spread of the disease, the women, who are so frequently nurses of the tuberculous sick, are likely to be exposed for prolonged periods to a concentrated infection. But reference will be made to this subject later on.

THE MAIN FACTORS WHICH ARE REGARDED AS HAVING PROMOTED THE STEADY FALL WHICH HAS APPARENTLY TAKEN PLACE IN THE DEATH-RATE FROM PULMONARY TUBERCULOSIS DURING THE LAST FIFTY YEARS.

It is quite impracticable in an address such as this to enter fully into this question, and a few words only can be said upon it. In some measure the fall may be apparent only: that is to say, greater opportunities for more accurate diagnosis may have had the effect of relegating deaths which formerly would have been attributed to "consumption" or "phthisis" to the real causes, and if this increasing knowledge transferred more deaths from pulmonary tuberculosis to other diseases than it brought to pulmonary tuberculosis, a fall in the phthisis death-rate would, *ceteris paribus*, ensue. It must suffice to

CHART II.

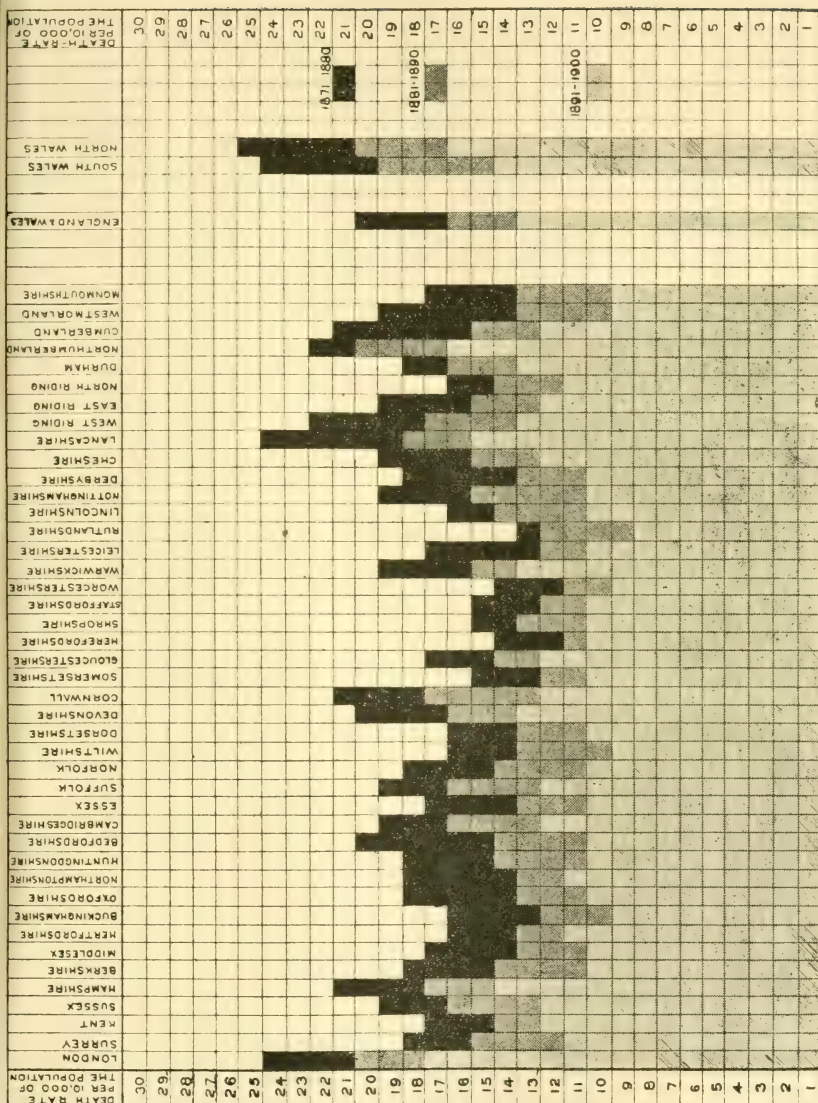


CHART SHOWING THE MEAN ANNUAL, UNCORRECTED DEATH-RATE FROM PULMONARY TUBERCULOSIS PER 10,000 OF THE POPULATION IN THE COUNTIES OF ENGLAND AND IN NORTH AND SOUTH WALES DURING EACH OF THE DECADES 1871-80, 1881-90, AND 1891-1900.

(This Chart has been reproduced from the Author's Report to the Local Government Board, with the sanction of Mr. Rowland Bailey, M.V.O., H.M. Controller of Stationery.)

observe as regards this matter that Dr. Tatham, the Superintendent of Statistics, regards this fall as in very large part a real one.

It is obvious that this fall cannot have been due to the conscious avoidance of infection, seeing that the fall in this and certain other countries commenced long before the discovery of the bacillus, and the disease was not, in so far as practice was concerned, regarded as communicable until recently; and, similarly, it cannot have been due, as Professor Koch thought in 1901, to the special institutions for the disease, seeing that they were, until this century, far too few to have exercised any appreciable effect upon the death-rate of the country as a whole.

Possibly the disease, while actually as prevalent as formerly, may be less fatal; and hence, fewer deaths in relation to the population may occur, a fact, if it be one, which may perhaps be attributed to earlier recognition of the disease and more rational treatment.

Again, as others have claimed, segregation in poor law and other institutions may have exercised an influence, as also may the decline of certain diseases which are believed to predispose to phthisis.

The general opinion amongst epidemiologists who have studied the tuberculosis problem is that the causes are multiple, and mainly social—that is to say, that consumption is very largely a social malady, associated with conditions which are found, for the most part, amongst the poor. Such conditions comprise insufficiency of food, overcrowding, absence of light, ventilation and cleanliness, overwork, over-indulgence in alcohol, dampness of soil, and other obviously unwholesome factors.

The evidence of such causation is based largely upon statistical evidence, some of which is not very reliable, but it may be said generally that the disease is found to be associated, although not entirely, with the conditions which may be summed up under the term “poverty,” a term which obviously embraces a large number of the separate factors enumerated above.

It would be possible to exhibit numerous charts illustrating the association of phthisis with overcrowding and other factors, but I must content myself with exhibiting a chart indicating the apparent parallelism between poverty and pulmonary tuberculosis in the three divisions of the United Kingdom.

It will be seen that the thick black curve represents the behaviour of total pauperism per 1000 of the population in each division of the Kingdom, and that the dotted curve represents the death-rate per 10,000 of the population from phthisis, or pulmonary tuberculosis. The lowest curve in each instance relates to indoor pauperism.

In England and in Scotland a declining pauperism is, generally speaking, associated with a declining death-rate from phthisis; in Ireland a rising total pauperism with a high and presumably rising phthisis death-rate. It will be noted that in England and Scotland the curve for indoor pauperism is low, while in Ireland it is high.



CHART INDICATING THE BEHAVIOUR OF TOTAL PAUPERISM, PHTHISIS DEATH-RATE, AND INDOOR PAUPERISM IN EACH DIVISION OF THE UNITED KINGDOM DURING THE LAST FIFTY YEARS.

(Reproduced from the Author's Report on Sanatoria to the Local Government Board, with permission of Mr. Rowland Bailey, M.V.O., H.M. Controller of Stationery.

In each case it is relatively stationary. But it must be borne in mind that the term "pauperism" is liable to be a misleading one, unless the user of the term is very familiar with the history of pauper administration, and that oscillations in the curves of indoor, outdoor, or total pauperism, may be produced by forces other than poverty; they may be produced, in fact, by a stroke of the pen in the shape of a departmental memorandum relaxing or tightening the administrative strings which govern the distribution of indoor and out-door relief, and which attract into or repel from the net of pauperism a large floating margin which suffices to materially influence the curves.

But I am assured by those who are masters of the history of poor-law administration in this country, that the curves of pauperism indicated upon Chart III. do, having regard to the numbers dealt with, sufficiently indicate the true position of affairs.

My main object, however, in touching upon this question of phthisis causation, has been to bring out the fact that the disease may be attacked by indirect (social) measures aimed at promoting the resistance of the host, as well as by direct measures aimed more particularly at the circumvention of the bacillus. Indeed, the history of tuberculosis in this country suffices, I think, to prove that the effects of indirect measures have already been very great.

Such indirect measures have involved mainly better social conditions which have meant a greater approximation of the poor in the matter of resistance to the conditions of the better-off. This approximation has not only meant an increased resistance and diminished opportunities for the development of the disease, but also better opportunities for the recognition and treatment of the disease, which have resulted in a diminished death-rate. The recognised and intimate association between overcrowding and phthisis clearly indicates that the diminution of overcrowding in our large towns must have exerted a beneficial effect upon the death-rate from tuberculosis, and one of the most hopeful lines of attack is obviously to be found not only in still further diminishing overcrowding, but by means of such legislative measures as Mr. Burns' Town Planning Bill to prevent in the future the erection of conditions in which overcrowding is associated with the absence of light and air. Provisions such as are embodied in this Bill will have the effect not only of diminishing tuberculosis but also other diseases, and, apart from specific diseases, of inducing that condition of individual well-being which is as much to be desired as the absence of certain infectious diseases.

In dealing with tuberculosis, we are presumably concerned with a receding foe, and the problem which we are now called upon to solve is how best, by direct and indirect means, are we to accelerate the decline of the malady. There is, doubtless, need for far more co-ordination between the several anti-tuberculosis forces than is at present the case, and such forces could, it seems to me, best be co-ordinated in the office of the medical officer of health.

THE PERSONAL COMMUNICABILITY OF PULMONARY TUBERCULOSIS.

This subject of the communicability of tuberculosis has been somewhat thrown out of perspective by inferences drawn from laboratory experiences. It has been inferred that because tuberculosis is inoculable, and since those susceptible little rodents, the guinea-pigs, develop tuberculosis when placed under conditions in which they have little else to breathe or swallow than tubercle bacilli, that therefore the malady in the human species is, in the ordinary relations of social life, as communicable as the more acutely infectious exanthemata such as smallpox and typhus fever.

But if this were so, and if defective "resistance" were not a more important factor in the production of the malady than the bacillus, it is difficult to understand, as Dr. Samuel West has observed, why so relatively few persons die from the disease. If pulmonary tuberculosis—the most communicable of all forms of tuberculosis—were as infectious as smallpox, we should have overwhelming evidence of the fact in the wards of hospitals for consumption, as, indeed, we have of the infectivity of smallpox and typhus fever upon nurses and others in attendance upon patients suffering from these diseases.*

On the other hand, in the consumption hospitals and sanatoria in this country, there is, as Dr. Theodore Williams, and others, have shown, no evidence which suggests that the nursing of consumptive patients in these institutions is attended with danger, and this notwithstanding the fact that the data as to this relative immunity of the staff relate largely to periods antecedent to the introduction of preventive measures such as are now being generally applied.

Moreover, were pulmonary tuberculosis possessed of a high degree of infectivity, the incidence of the disease upon married couples—one or other partner of which is already tuberculous—would in the past, having regard to the prolonged exposure and intimate relations, and, until recently, to a non-belief in the infectivity of the disease, have been conspicuous. Longstaffe, however, as the result of a most carefully reasoned investigation, concluded that the incidence of the disease upon married couples was no greater than would be expected as a mere matter of chance, and it has to be admitted, as Whitelegge and Newman have expressed it in their admirable text-book, "infection between married persons is not yet satisfactorily settled."

* Murchison, in his classical treatise, tells us that in twenty-three years no fewer than 288 cases of typhus fever originated in the London Fever Hospital alone, and as regards the Gateshead Fever Hospital, the medical officer of health wrote: "Every nurse who has been more than a fortnight in the typhus wards has suffered from typhus."

In this connection Professor Karl Pearson has recently, as the result of his own researches, arrived at the conclusion that "a theory of infection does not account for all the facts."

The careful and sustained investigations as, for instance, of Niven, and other observers, demonstrate, it would seem, that under certain circumstances of overcrowding, and prolonged exposure to mass infection, the disease possesses a considerable degree of personal infectivity. The evidence adduced under this head is what I have termed that of "association," i.e. persons who have developed the disease have been shown to have been previously associated with persons suffering either from recognised or suspected phthisis, and these associations have been regarded as those of effect and cause.

There are, however, two not impossible explanations of these occurrences. One, that they are cases of recent independent infection, the other, that they are cases of the re-awakening of old foci of infection, concerning the enormous prevalence of which more will be said directly. Moreover, all who have had experience of tracing the history of the onset of pulmonary tuberculosis recognise the enormous difficulty of the task, which was drawn attention to long since by Portal and Buhl, and, as Dr. West observes, experience in investigations of this nature indicates that—

The date of the original infection with tubercle is thrust further and further back—even, it may be, into early childhood; so that in considering the causation of phthisis it will be necessary to deal with the original sources of infection in early life.

It has been suggested that the term "conditionally infectious" is one which might be applied to phthisis, and seeing that this term implies that if the conditions which are thought to induce infection are avoided, infection is unlikely to ensue, the term may perhaps be employed as embodying a helpful working hypothesis.

THE DIFFICULTY OF DEALING ADMINISTRATIVELY WITH CONSUMPTION AS WITH SMALLPOX.

As definite proposals have been made to deal with a chronic disease such as consumption, in like manner to an acute disease such as smallpox, it will serve a useful purpose to indicate some of the social effects which might result.

Cases of smallpox are at once notified, removed to a fever hospital, and kept there until recovery or death ensues—one or other of which events occurs, as a rule, within six weeks. Those who have been in contact with the patients are kept under observation, or actually in quarantine at the expense of the rates, for a fortnight; children from the same house are kept from school, and, at times, associated adults are kept from work. After the removal of the patient to hospital, the

house and infected articles are disinfected, and, if no more cases occur among the "contacts," the trouble and expense are at an end. If a patient suffering from this disease exposes himself in public, or enters a public conveyance, he is prosecuted.

Let us, in imagination, apply these conditions to patients suffering from consumption. On recognition of the disease the patient would be isolated, not for six weeks but for months, and, often, for many years (until, in fact, death ensued, or his sputum, after repeated examination, contained no tubercle bacilli). During this time, in the case of the poor, the patient's dependants would be supported either by friends or by the rates. These dependants would be kept under observation, and upon symptoms of tuberculosis appearing they, too, would be withdrawn from the public.

In 1906, there were in England and Wales 56,841 deaths from all forms of tuberculosis, and 39,746 from pulmonary tuberculosis alone. It is the usual method in order to determine the actual number of cases of phthisis, to multiply the deaths by three, a process which, in this instance, yields over 109,000 cases. If all these cases were sought out and isolated and their dependants supported, the problem would, as regards cost, reach somewhat alarming proportions, though as we shall see directly by a study of post-mortem records, this estimate of 109,000 is, as regards actual prevalence, far below the actual figures. But probably it is already obvious that to attack the problem on smallpox lines is not one which can be seriously considered from an administrative standpoint.

SOME CURRENT VIEWS, AS REGARDS PULMONARY TUBERCULOSIS, AS TO THE MEANS BY WHICH, AND THE TIMES AT WHICH, THE TUBERCLE BACILLUS IS INTRODUCED INTO THE BODY.

The Vehicle of Infection.—It is not too much to say that the whole of this aspect of the tuberculosis problem is in the melting pot.

Up to within the last few years the prevailing belief, supported, it was thought, by adequate experimental evidence, was to the effect that the disease was chiefly spread by means of tuberculous sputum which had undergone desiccation to such an extent as to enable it to be detached from surfaces and transmitted, mainly by currents of air, to the lungs; and preventive action was largely based upon this conception. Those who hold this doctrine belong to what may be termed the "dust school."

Within recent years, however, another school, which may conveniently be spoken of as the "droplet school," has arisen, and those who are its adherents hold that the greatest danger is to be contemplated not from dust, but from the droplets given off when phthisical persons cough, sneeze, or speak excitedly. It is generally conceded that in quick respiration no micro-organisms are given off with the breath.

Tuberculous persons emitting bacilli in their sputum were placed in a glass chamber, in various parts of which were deposited suitable culture plates, and after periods of coughing on the part of the patients, the plates were collected, and when incubated, carefully examined; a large number of the plates placed both in front of and behind the patient were found to show growths of tubercle bacilli.*

With the dust school, the collection and destruction of sputum are alone necessary to ensure safety from infection; with the droplet school, safety is to be ensured by holding a handkerchief before the mouth and nose during periods of coughing, sneezing, and excited speech. This droplet school points to certain experiments with sputum which indicate that before sputum can, as a rule, reach the dust phase, the bacilli which it contains are dead or dying, and it is urged that bacilli freshly given off from the host in which they have thriven are more likely to possess potency for harm than those which have left their natural habitat many days.

Both schools believe that infection is brought about by inhalation, whether of droplets or of dust, into the lungs.

CHANNELS OF INFECTION.

Even as there are two sharply divided schools of thought relative to the vehicles of infection, so also are there two schools as to the channels by which the tubercle bacilli gain access to the body.

One school believes that the bacilli, wet or dry, stale or fresh, enter the body *viâ* the lungs; the other school holds that the parasites enter the body by way of the alimentary tract. There is experimental evidence in support of both these views.

For the most part each school believes that the bacilli, however taken in, set up definite disease within a short space of time, and, in so far as this belief obtains, both schools may be spoken of as schools of *immediate* infection.

Without expressing opinion as to which of these routes is the more important, it may be said that recent work carried out in this and other countries is in support of the view that the intestine is

* Some idea of the manner in which bacteria, given off from droplets of saliva, may be wafted about by the air may be gathered from the experiments by Gordon in our own House of Commons. Having exposed culture plates in all parts of the House—in the galleries as well as in the Chamber itself—he was able to ascertain that the air of the House contained, as a rule, none of the bacilli (*B. prodigiosus*) which he proposed to employ in the experiment. He then exposed fresh culture plates, and occupying the position of the Prime Minister, and washing out his mouth from time to time with a solution of the bacilli in question, he proceeded to address various sections of the House by reciting long passages from Shakespeare—Julius Cæsar being selected as affording suitable opportunities for declamation. It was found upon collecting and incubating the plates that some in practically every part of the House had become inoculated.

the route by which the bacillus most frequently enters the lungs—that is to say, that infection is brought about by ingestion rather than inhalation.

LATENT TUBERCULOSIS.

But now we come to another school holding the far-reaching doctrine that the actual appearance of the disease bears practically no relation to the actual date of the introduction of the bacilli. It teaches, in fact, that practically all of us are, either potentially or actually, already tuberculous. This school of “latency,” which is led by Professor von Behring, the discoverer of diphtheria antitoxin, takes the view that during infancy tubercle bacilli—mainly, but not entirely, of bovine origin—being taken into the intestinal tract with food, pass through the intestinal walls without apparent damage to such walls, and, becoming arrested in the glands of the body, remain actually latent, or at most but little active, until debilitating changes induced by illness, by overgrowth, by puberty, overwork, deficient food, unwholesome surroundings, and the like, enable the bacilli to awaken into activity, and so invade the lungs and other tissues.

It will be seen at once that a very great deal depends upon whether this view of latency is or is not correct. If it be wholly, or even largely true, the current practice of attributing cases of tuberculosis to association with persons already tuberculous at various periods previously, will have to be re-examined, and, if von Behring’s view has even something in it, human pulmonary tuberculosis is, as Dr. Müller, of Königsberg, has figuratively expressed it, “merely the end of the song which was sung to the young consumptive in the cradle; in other words, that pulmonary phthisis is the typical end of a chronic epizootic tubercular infection contracted in infancy.”

THE ACTUAL PREVALENCE OF TUBERCULOSIS IN THE HUMAN SPECIES AS EVIDENCED BY POST-MORTEM RECORDS.

If the dimensions of the tuberculosis problem are to be properly understood, it is necessary to ascertain as correctly as practicable the number of living persons who are now suffering or who in the past have suffered from some form of tuberculosis. This, however, is a difficult matter. We may, of course, take our stand as it were by that gate of death having the superscription “tuberculosis,” and count the number of persons who pass out *viâ* this gate, but, as measuring the prevalence of the disease, this method would be a very misleading one. To gauge, even approximately, the prevalence of the malady, we must stand by each and every gate of death and ascertain, as far as circumstances will permit, the percentage of the total passing out which shows, on post-mortem examination, any

lesion whatever of tuberculosis. We must be cautious, however, not to generalise too freely from post-mortem records, since it is, as a rule, only the poorer classes who reach the post-mortem table, and, as is well known, tuberculosis has a much greater death-rate amongst the poor than amongst those better-off. Some instructive figures may be quoted. Dr. Harris, working at the Manchester Royal Infirmary, found in 39 per cent. of all the post-mortems evidences of tuberculous lesions. Brouardel, in the Morgue of Paris, found that amongst the bodies of persons who had lived over ten years in Paris, and who had met their death mainly by violence in the streets, 50 per cent. presented tuberculous lesions. Lubarsch, dealing with 800 cases, found 61 per cent. with such lesions, and Naegeli, of Zurich, detected similar manifestations in 90 per cent. of 500 autopsies. Finally, it may be mentioned for what it is worth, that the application of the tuberculous test to a Bosnian-Herzegovinian regiment gave a positive reaction in 68 per cent.

There will probably be differences of opinion as to the value of the foregoing figures, but it must be admitted that there is ground for the statement of Professor Osler that: "The germ of tuberculosis is ubiquitous; few reach maturity without infection, none reach old age without a focus somewhere," a condition of affairs which is similarly conveyed by the German adage, "Jedermann hat am ende ein bischen tuberculose." In addressing an audience such as this, one would hesitate to repeat the following statement once made in an address by Sir Clifford Allbutt, "I am guilty of no extravagance when I suggest that one-third of you who hear me wittingly or unwittingly are or have been infected with tubercle," for the reason that our knowledge of the distribution of tuberculous lesions amongst the rich is limited, owing to the fact that post-mortem results of this class are few; a gap in our knowledge which would be soon filled up were the example followed of the illustrious surgeon, who expressed a desire in his last testament that after death his body was to be examined with special reference to a possible tuberculous lesion in the apex of his left lung. The autopsy was duly made, and a tuberculous nodule in his left apex discovered.

But all these messages from the post-mortem room are full of hope for consumptive patients. They tell us that, as Cardswell said, "Pathological anatomy has never, perhaps, given a more decided proof of the cure of a disease than it gives in the case of pulmonary tuberculosis."

Such records from the post-mortem room also show that tuberculosis is curable amongst the very poor, and therefore that the old French aphorism that "There are two kinds of consumption: that of the *rich*, and that of the *poor*; from the first recovery takes place sometimes, from the latter never," has no very sound basis in fact.

THE RELATION BETWEEN BOVINE AND HUMAN TUBERCULOSIS.

It may be said that the problem which calls immediately for solution is as to the proportion of this vast prevalence of tuberculosis revealed by post-mortem records that is due to bovine infection. Seven years ago, when much good work was being done, or about to be done, towards the control of bovine tuberculosis, Professor Koch referred the whole question back for reconsideration by declaring with all the weight of his great influence, that

Though the important question whether man is susceptible to bovine tuberculosis at all is not yet definitely decided, and will not admit of absolute decision to-day or to-morrow, one is, nevertheless, at liberty to say that, if such a susceptibility really exists, the infection of human beings is but a very rare occurrence. I should estimate the extent of infection by the milk and flesh of tuberculous cattle, and the butter made of their milk, as hardly greater than that of hereditary transmission, and I, therefore, do not deem it advisable to take any measures against it.

The effect of this statement was profound. It was made upon the basis of a somewhat limited series of experiments as regards the infection of cattle by means of bovine and human tuberculosis respectively. Generally speaking, the cattle inoculated with bovine tuberculosis developed the disease, while those inoculated with human tuberculosis remained unaffected. There were, however, indications amongst the cattle or swine inoculated with human tuberculosis which suggested to many persons who heard the address that the inference drawn from the experiments was not altogether justified, even although the alleged rarity of primary intestinal tuberculosis in the adult human subject had obviously influenced the lecturer in arriving at that conclusion.

Professor Koch's inferences were criticised by men such as Lord Lister, Nocard, Bang, Sims Woodhead, and Ravenel. Lord Lister urged need for caution. It was a serious matter, he pointed out, if the conclusion was wrong, and he raised the question as to the soundness of Professor Koch's views with reference to the rarity of primary intestinal tuberculosis. As Lord Lister pointed out, even if actual tubercular lesions of the intestines were as rare as Professor Koch thought, it must surely be admitted that tubercular disease of the mesenteric glands is quite a common malady in children, and he added, in words which subsequent experience has proved to have been almost prophetic, "If this be a fact, the natural interpretation is that the tubercle bacillus passes through the intestinal wall without producing a distinct lesion of the intestine."

A few days after this pronouncement, Royal Commissions were appointed in this country and in Germany, and the findings of each

up to the present moment agree that, although there are certain differences between the bacillus of bovine and that of human tuberculosis, the former can and does set up tuberculosis in the human being.

Our own Commission found that of the cases of tuberculosis which they examined, and which were in their opinion undoubtedly of intestinal origin, a large percentage had been infected from the bovine source, and they say later : "A very considerable amount of disease and loss of life, especially among the young, must be attributed to the consumption of cow's-milk containing tubercle bacilli," adding that "The fact that the bacilli of bovine tuberculosis can readily by feeding, as well as by subcutaneous injection, give rise to generalised tuberculosis in the anthropoid ape so nearly related to man . . . has an importance so obvious that it need not be dwelt upon."

The question to which this country is now awaiting an answer by help of the Royal Commission on Tuberculosis, which is still sitting, is—what amount of our total tuberculosis is due to the milk of cows? It seems clear that amongst infants the amount may be very considerable, since the bacilli of actual bovine origin are found disseminated in the lesions. With regard to tuberculosis developing in after life the answer must seemingly depend largely upon whether the bacillus of bovine origin can by long residence in the human economy become indistinguishable from the bovine bacillus. As to this the necessary data are still wanting.

It is, however, clear that, as Lord Lister predicted, bovine bacilli can traverse the walls of the intestines without their showing evidences of their passage; and the question is as to how far von Behring's view of prolonged latency will receive support from the further researches of the Commission.

The need for far more rigorous control of the tuberculous cows is obvious when we are told that the percentage of the milk reaching certain of our large towns from beyond the borough limits is as follows : Liverpool, 14·5, Birmingham, 14·0, Sheffield, 13·0, and Manchester 7·8.

It has, too, to be remembered that, contrary to what was formerly believed, apparently healthy cattle may yield tuberculous excretions and secretions, a fact which renders the danger from the consumption of unboiled cows' milk greater than was hitherto thought to be the case.

CERTAIN CONSIDERATIONS AS REGARDS THE SANATORIUM TREATMENT OF PHTHISIS.

As to this, time will only allow a few words to be said, but I am desirous to draw attention to the need for keeping the statistics relative to the "after-results" of sanatorium treatment in a fashion which will bring out the results in a clearer manner than is at present the case. It is well that we should boldly face the results in

this matter of sanatoria, in order that we may be in a position to improve our methods, if necessary. I shall confine my remarks exclusively to the poorer classes.

What are known as the immediate results of sanatorium treatment amongst the poor are, in a large percentage of the total number of cases admitted, decidedly good. The poor react well to the abundance of good food, the relative rest, the pure air, and the discipline of sanatorium life. In all these matters the poor have, as it were, a large margin to work upon.

Consequently, a large number of the cases admitted leave the institutions in conditions which are described by somewhat heterogeneous terms, such as "cured," "arrested," "greatly improved," or "improved;" and could the circumstances which have brought about this improved condition be maintained, a larger number of patients than is at present the case would retain such improvement more or less indefinitely.

The hard school of experience, however, after some years trial, compels us to face the fact that under stress of work, along with persistence in selecting unwholesome environments, a large proportion of the cases relapse. The proportions vary in different sanatoria according mainly to the success or failure to secure for treatment early and suitable cases of the disease, but there is at the present time no useful purpose to be served by attempting comparison between the results of one and another institution.

The usual method of presenting the "after-results" is to ascertain the condition at some given time of all the cases which have been discharged, thereby including cases discharged only a few weeks with those discharged perhaps three or four years or more. This method tends to present the after-results in a too optimistic fashion; in effect it amounts to confounding and mingling the after-results with the immediate results.

Sometimes, however, the figures for the last year are excluded from the after-results, and in this fashion the actual after-results are more nearly approached.

In this matter of sanatorium statistics, much may be learnt from the German methods, by means of which the after-results relative to patients discharged in any given year are kept quite separate from those relative to patients discharged in other years, and by this means it is possible to determine when a sufficient period has elapsed, in what manner the patients discharged year after year are (*a*) maintaining themselves, (*b*) surviving.

Unfortunately, the data as regards after-results in English sanatoria have not, as a rule, been so collected and arranged as to allow of grouping by the German method, but the figures for the Durham Sanatorium and also for the Kelling Sanatorium enable this to be done, and they are herewith presented in this fashion.

DURHAM (STANHOPE) SANATORIUM.

TABLE I.—All Cases considered together.

[illegible]

TABLE II.—Early Cases separately considered.

[illegible]

TABLE III.—Advanced Cases separately considered.

[illegible]

KELLING SANATORIUM.

Year	No. Discharged	Nos. known to be at work, or fit to be at work, on Dec. 31 of each year since discharge.				
		1903	1904	1905	1906	1907
1903	54	38 or 70.37 per cent.	28 or 51.85 per cent.	27 or 50.00 per cent.	24 or 44.44 per cent.	20 or 37.03 per cent.
1904	134	—	79 or 59.00 per cent.	62 or 46.26 per cent.	50 or 37.31 per cent.	43 or 32.09 per cent.
1905	146	—	—	77 or 52.74 per cent.	66 or 45.20 per cent.	65 or 44.52 per cent.
1906	172	—	—	—	90 or 52.32 per cent.	88 or 51.05 per cent.

TABLE V.a.
Including all cases.

Year	No. Discharged	Nos. known to be at work, or fit to be at work, on Dec. 31 of each year since discharge.				
		1903	1904	1905	1906	1907
1903	33	32 or 97.00 per cent.	23 or 70.00 per cent.	23 or 70.00 per cent.	20 or 60.60 per cent.	*22 or 69.70 per cent.
1904	80	—	60 or 75.00 per cent.	48 or 60.00 per cent.	40 or 50.00 per cent.	39 or 48.75 per cent.
1905	88	—	—	63 or 71.60 per cent.	49 or 55.68 per cent.	*57 or 65.91 per cent.
1906	99	—	—	—	78 or 78.78 per cent.	68 or 68.68 per cent.

TABLE V.b.
Including suitable cases only.

* Several old patients were heard of as being at work at the end of 1907 of whom no information was obtained in 1906.

Year	No. Discharged	Nos. known to be at work, or fit to be at work, on Dec. 31 of each year since discharge.				
		1903	1904	1905	1906	1907
1903	21	7 or 33.33 per cent.	3 or 14.29 per cent.	3 or 14.29 per cent.	3 or 14.29 per cent.	2 or 9.52 per cent.
1904	54	—	18 or 33.33 per cent.	11 or 20.37 per cent.	7 or 12.96 per cent.	5 or 9.07 per cent.
1905	58	—	—	16 or 27.58 per cent.	12 or 20.69 per cent.	10 or 17.24 per cent.
1906	73	—	—	—	27 or 37.00 per cent.	24 or 32.87 per cent.

TABLE V.c.
Including unsuitable cases only.

In reflecting upon these tables, it must be remembered that the numbers are small, and hence, that inferences must be drawn with caution; but with this reservation the tables may be briefly discussed.

It will be noted, with reference to Table I. of the Durham figures, in which all the cases, early and advanced, are considered together, that of 36 cases discharged, 6, or 16·7 per cent. were at work some seven years afterwards; and that the percentage of workers rises as the year of discharge becomes recent. When, however, the early cases are considered separately, as is the case in Table II., it is seen that the percentage of workers at the end of seven years is more than double that yielded when all cases were considered together. Turning, however, to the advanced cases as set forth in Table III., it is seen that, of 20 cases discharged, the complete story has been told at the end of seven years, while of 25 cases discharged in 1901-2, only one worker was left in April 1907.

Unfortunately, it is difficult to gather from these tables whether the class of cases admitted is becoming a better class—i.e. whether earlier and more suitable cases are being secured each succeeding year. The statistics of the German sanatoria bring out this point very clearly, and they show that year by year, better results are being obtained—i.e. that the good effects of the treatment last, on an average, longer and longer.

The figures relative to the Kelling Sanatorium are self explanatory, and, as far as they go, can be compared with those of the Durham Sanatorium.*

The result in both those institutions may, I think, be regarded as relatively encouraging, but those from another institution, now to be noticed, cannot be quite viewed in that light. Apparently, the methods of selection in this later case have not been so successful as in the two former. It will be seen that of 183 cases discharged between 1902 and 1905, only 23, or 12·6 per cent., had maintained their improvement, while the remaining 160 were either “worse,” “dead,” or “unaccounted for.”

* Since the date of the above lecture, Dr. Davies, the Medical Officer of Health of Bristol, has been good enough to furnish me with the following data relative to the Bristol cases treated in the Winsley Sanatorium:—

Year of Discharge.	Number of Patients Discharged.	Number year by year (a) alive: (b) supporting themselves by their own efforts.					
		1906		1907		1908	
		a	b	a	b	a	b
1905	45	39	23	29	21	25	16
1906	67	—	—	58	39	47	35

Year of Discharge	Stage when Admitted	Improvement maintained	Worse	Dead	Un-accounted for
1902	Early 11	4	—	2	5
	Moderately advanced.. 21	1	—	19	1
	Advanced 14	—	—	12	2
	46	5	—	33	8
1903	Early 12	4	—	4	4
	Moderately advanced.. 32	1	1	24	6
	Advanced 12	—	—	9	3
	56	5	1	37	13
1904	Early 9	4	—	4	1
	Moderately advanced.. 28	2	1	11	14
	Advanced 8	—	1	2	5
	45	6	2	17	20
1905	Early 11	4	—	5	2
	Moderately advanced.. 17	3	2	4	8
	Advanced 8	—	—	7	1
	36	7	2	16	11
1902-1905	Early 43	16	—	15	12
	Moderately advanced.. 98	7	4	58	29
	Advanced 42	—	1	30	11
	183	23	5	103	52

The figures from these several institutions will suffice to bring out one cardinal fact, and that is the enormous importance, in so far as the arrest of the disease is concerned, of securing cases in the earliest stage of the malady.

Unfortunately, a considerable experience on the part of some sanatoria has shown that this task is by no means as easy as it might be thought.

Patients, as a rule, fail to seek medical advice until the actual early stage has passed ; in fact, they are usually expectorating tubercle bacilli. But when medical advice is at length sought, patients frequently hesitate to relinquish work, more especially if they are bread-winners. Consequently, by the time the patient reaches the sanatorium, the initial and most hopeful phase of the malady has commonly passed.

It is clear, therefore, that there is great need for improvement in

existing machinery whereby cases are selected for sanatorium treatment, and with this end in view every encouragement ought to be given to the poorer classes to seek medical advice at an early stage of their illness, and, above all, nothing should be done, as for instance by social ostracism of tuberculous subjects, to prevent them from seeking medical advice at the earliest indications of illness. Any system which would tend to prevent the recognition of early cases of the disease is likely to defeat its own end by leading to the concealment of cases.

Here, again, the Germans are much ahead of us by virtue of their admirable compulsory insurance system for the working classes which is in operation in the Fatherland, and by means of which working men earning under 100% per year are compelled to insure themselves against old age, sickness, and invalidity.

It is that part of the system relative to invalidity which concerns itself more particularly with tuberculosis, and through its agency, patients are, in effect, encouraged to seek medical advice at the earliest moment. If they are found to be tuberculous, they are sent forthwith to a sanatorium provided out of the surplus funds of the insurance system, and their wives and families are supported in their absence. All this is done without any taint of charity, inasmuch as the workman has, during health, contributed weekly his portion to the system. To all the benefits appertaining to this vast system, which is a standing example to the world of the value of self-help and thrift, the workman when ill has a legal and honourable claim, and the effect of this system is to raise the working man into a higher social plane than without it he would occupy. It is admitted on all hands that tuberculosis is largely a social malady, and anything which will tend to furnish to the poor advantages in ill-health which are otherwise beyond the reach of their class, has the effect of attaching them, *quâ* sickness, to a class materially above them. The death-rate from tuberculosis in Germany has, since the introduction of this system, evinced a remarkable fall, and Germans who have paid special attention to the subject attribute the fall mainly to the social organisations brought about by the insurance system.

Herr Bielefeldt, who has devoted a life-time to the study of this question, writes: "Among all the factors entering into operation in the anti-tuberculosis battle in Germany, the German Workmen's Insurance undoubtedly occupies the foremost place."

I have myself seen something of this system in Germany, and I have no hesitation in expressing an opinion that a system such as this, if adapted to this country, would exert a marked effect upon the behaviour of tuberculosis, more marked in all probability than that which would be exerted by any other single factor in the whole anti-tuberculosis programme.

[H. T. B.]

WEEKLY EVENING MEETING,

Friday, May 22, 1908.

SIR WILLIAM CROOKES, D.Sc. F.R.S., Honorary Secretary and
Vice-President, in the Chair.

PROFESSOR DR. J. C. KAPTEYN,

*Director of the Astronomical Laboratory, and Professor of Astronomy
in the University of Groningen.*

Recent Researches in the Structure of the Universe.

INTRODUCTION.

I CONSIDER it an uncommon privilege to lecture on the Structure of the Universe in the country of the Herschels.

Even now their celebrated gauges are unrivalled, and they still form one of the important elements on which any theory of the stellar system must be based.

It is well known that the plan of these gauges consisted in directing the telescope successively to different points all over the sky, and simply counting the number of stars visible in the field.

REGULARITY IN THE ASPECT OF THE SKY.

There is one fact clearly brought out by these gauges to which I must call your attention. It is that in the outward appearance of our nightly sky, as seen with the telescope, there is a great regularity. In the Milky-way, that belt which we see with the naked eye encircling the whole of the firmament nearly along a great circle, the number of stars, as seen in Herschel's 20-foot reflector, is enormous. On both sides, this apparent crowding of the stars diminishes very gradually and regularly, till, near the poles of the Milky-way, we come to the poorest parts of the sky.

VARIATION WITH GALACTIC LATITUDE.

Let us look at this phenomenon somewhat more closely. If we direct our telescope first towards the part of the Milky-way near Sirius, and if from thence we gradually work up towards the North Pole of the Milky-way in the constellation called the Hair of Berenice, we shall clearly perceive this gradual and regular change in the number of stars. Now if we repeat the same process, beginning from some other point of the Milky-way, say in Cassiopeia, or the Southern Cross, we shall find that, not only is there a similar gradual change, but we shall approximately go through the same changes.

LITTLE VARIATION WITH GALACTIC LONGITUDE.

At the *same* distance from the Milky-way we shall find approximately the same number of stars in the field of the telescope.

Put in other words : the richness of stars varies regularly with the galactic latitude ; it varies relatively little with the galactic longitude.

Imitating most of the investigators of the stellar system, we will therefore disregard the longitude and keep in view only the changes with the galactic latitude. In reality this comes to being satisfied with a first approximation. For, in reality, there are differences in the different longitudes, especially in the Milky-way itself. But even here the differences are not so great as seems commonly to be supposed. There is every reason to believe, therefore, that our approximation will be already a tolerably close one.

REAL STRUCTURE.

Meanwhile what the Herschel gauges teach us is only relative to the outward appearance of the sky. What is the real structure of the stellar world ? If we see so many stars in the field, with the telescope directed to the Milky-way, is it because they are really more closely crowded there, as Struve thinks, or is the view of the older Herschel correct, who imagined that the greater richness is simply a consequence of the fact that we are looking in deeper layers of stars ; that our universe is more extensive in the Milky-way than it is in other directions ?

Imagine that we could actually travel through space. For instance, imagine that first we travel in the direction of the constellation Cassiopeia. If we travel with the velocity of light, not so very many years would pass before we get near to some star. Proceeding on our journey for many, many more years, always straight on, we will pass more stars by and by. How will these stars look thus viewed from a moderate distance—say, from a distance as that of the sun ?

Will they all be found to be of equal luminosity, as Struve practically assumed ? And in this case are they as luminous as our sun, or more so, or less so ? Or are they unequal ?

If so, how many of them are brighter than our sun, how many fainter ? Or, to be more particular, how many per cent. of the stars are 10, 100, 1000, etc., times more luminous than our sun ? How many are equal to the sun, or 10, 100 times fainter ? Or in two words : What is the nature of the mixture ? or, lastly, what is the *mixture-law* of the system of the stars ?

And furthermore :

In travelling on, shall we find the stars in reality equally thickly, or rather thinly, crowded everywhere ? Or shall we find that after

a certain time, which may be many centuries, they begin to thin out, as a first warning of an approaching limit of the system? Is there really such a limit, which, once passed, leads us into abysses of void space?

Herschel thought there was such a limit, and even imagined that his big telescope penetrated to that limit; that is, he assumed that his telescope made even the remotest stars visible. On this supposition is based his celebrated disc theory of the system.

Again, we may condense these questions in this single query: How does the crowding of the stars, or the *star-density*, that is the number of stars in any determined volume (let us say in a cubic light century) vary with the distance from our solar system?

But there is more. We supposed that our journey went straight on in the direction of Cassiopeia, which is in the Milky-way. What if our journey is directed to the Pleiades, which are at some distance from that belt, or to the Northern Crown, which is still further, or to the Hair of Berenice, which is furthest of all from the Milky-way? For different regions *equally* distant from the galaxy we have seen that outward appearances are the same. We may admit, with much probability, that in space, too, we would find little difference. Summing up, the problem of the structure of the stellar system in a first approximation comes to this:—

STATEMENT OF PROBLEM.

To determine, separately for regions of different galactic latitude, in which way the star-density and the mixture vary with the distance from the solar system.

I think that there is well founded hope that, even perhaps within a few years, sufficient materials will be forthcoming which will allow us to attack the problem to this degree of generality, with a fair chance of success. At the present moment, however, our data are yet too scanty for the purpose. Still, they will be sufficient for the derivation of what must be in some sort *average* conditions in the system. The method of treatment will not be essentially different from that which will be applied later to the more general problem, but we have provisionally to be content with introducing the two following simplifications:—

RESTRICTIONS.

1. We will assume that the *mixture* is the same throughout the whole of the system;

2. We will not treat the different galactic latitudes separately.

The consequence will be that the resulting variations of density to which our discussion leads, will not represent the actual variations which we would find if we travelled in space in any determined fixed direction, but a variation which will represent some average of what

we would find on all our travels if we successively directed them to different regions of the sky.

SIMPLIFIED PROBLEM.

Our present problem will thus be confined to finding out :

- (a) The mixture law ;
- (b) The mean star-density at different distances from the solar system.

If time allows, I will, at the end of this lecture, say a few words on the restrictions introduced, and the way to get rid of them.

As it is not given to us to make such travels through space as here imagined, we have to rely on more human methods for the solution of our problem.

DETERMINATION OF DISTANCE.

It is at once evident that there would be no difficulty at all if it were as easy to determine the distance of the stars as it is to determine the direction in which they stand. For in that case the stars would be localised in space, and it would be possible to construct a true model from which the peculiarities of the system might be studied.

It is a fact, however, that, with the exception of a hundred stars at most, we know nothing of the distances of the individual stars.

What is the cause of this state of things? It is owing to the fact that we have *two* eyes that we are enabled not only to perceive the direction in which external objects are situated but to get an idea of their distance, to localise them in space. But this power is rather limited. For distances exceeding some hundreds of yards it utterly fails. The reason is that the distance between the eyes as compared with the distance to be evaluated becomes too small. Instruments have been devised by which the distance between the eyes is, as it were, artificially increased. With a good instrument of this sort distances of several miles may be evaluated. For still greater distances we may imagine each eye replaced by a photographic plate. This would even already be quite sufficient for one of the heavenly bodies, viz. for the moon.

At one and the same moment let a photograph of the moon and the surrounding stars be taken both at the Cape Observatory and at the Royal Observatory at Greenwich. Placing the two photographs side by side in the stereoscope, we shall clearly see the moon "hanging in space," and may evaluate its distance.

But already for the sun and the nearest planets, our next neighbours in the Universe after the moon, the difficulty recommences.

The reason is that any available distance on the earth, taken as eye-distance, is rather small for the purpose. However, owing to incredible perseverance and skill of several observers, and by substituting the most refined measurement for stereoscopic examination,

astronomers have succeeded in overcoming the difficulty for the sun. I think we may say that at present we know its distance to within a thousandth part of its amount. Knowing the sun's distance we get that of all the planets by a well-known relation existing between the planetary distances.

But now for the fixed stars, which must be hundreds of thousands of times further removed than the sun. There evidently can be no question of any sufficient eye-distance on our earth. Meanwhile our success with the sun has provided us with a new eye-distance, 24,000 times greater than any possible eye-distance on the earth. For now that we know the distance at which the earth travels in its orbit round the sun, we can take the diameter of its orbit as our eye-distance. Photographs taken at epochs six months apart will represent the stellar world as seen from points the distance between which is already best expressed in the time it would take light to traverse it. The time would be about 16 minutes.

However, even this distance, immense as it is, is on the whole inadequate for obtaining a stereoscopic view of the stars. It is only in quite exceptional cases that photographs on a large scale—that is, obtained by the aid of big telescopes—show any stereoscopic effect for fixed stars. By accurate measurement of the photos we may perhaps get somewhat beyond what we can attain by simple stereoscopic inspection, but, as we said a moment ago, astronomers have not succeeded in this way in determining the distance of more than a hundred stars in all.

How far we are still from getting good stereoscopic views appears clearly from the stereoscopic maps which your countryman, Mr. Heath, constructed, making use of the data obtained in the way presently to be considered. In order to get really good pictures he found it necessary to increase the eye-distance furnished by the earth's orbit 19,000 times.

Are there, then, no means of still increasing this eye-distance?

MOTION OF SOLAR SYSTEM THROUGH SPACE.

There is one way, but it is a rather imperfect one. Sir William Herschel has been the first to show, though certainly his data were still hardly sufficient for the purpose, that the whole of the solar system is moving through space in the direction towards the constellation of Hercules. Later observations and computations have confirmed Herschel's conclusions, and we have even been able of late to fix with some precision the velocity of this motion, which amounts to 20 kilometres per second. This velocity is a 15,000th part of the velocity of light. In the 150 years elapsed since Bradley determined for the first time the position of numerous stars with modern precision, the solar system must thus have covered a distance of exactly a hundredth part of a light-year, i.e. we are thus enabled to make

pictures of the sky as seen from points of view at a mutual distance of a hundredth of a light-year. Our eye-distance of 16 light minutes is thus increased more than 300-fold. True, this distance falls still considerably short of that adopted by Heath, but it appears that, for a considerable part of the stars, it is, though not nearly so great as might be desired, still in a certain way sufficient.

CANNOT FURNISH DISTANCE OF INDIVIDUAL STARS.

There is, however, a difficulty in the way, which prevents our pictures from giving a stereoscopic view of the stars at all, and thus prevents the determination of the distance of any star in this manner. The difficulty is that the changed directions in which, after the lapse of 150 years, we see the stars, is not exclusively the consequence of the sun's motion through space, but is due also to a real motion of the stars themselves. The two causes of displacement which, in the case that we take the diameter of the earth's orbit as eye-distance, are separable by means of a simple device, become inseparable in the present case.

In order to see whether this difficulty be or be not absolutely insuperable, I will take a parallel case *on the earth*.

DISTANCE OF INSECT CLOUD

At a certain distance we observe a cloud of insects hovering over a small pond. In order to evaluate the distance separating the insects from our eye suppose that we make a photograph; then after a few seconds a second one from a slightly different standpoint. It must be evident that even if we have used an instrument which clearly shows the individual insects, the two pictures put in the stereoscope will not furnish a stereoscopic view of them individually; on the contrary, the picture as seen in the stereoscope will be perfectly chaotic. The reason, of course, is that in the interval between the taking of the two photographs the insects have moved. Does it follow that no evaluation of the distance can be obtained?

The answer must be: of any individual insect, *no*; but of the cloud, as a whole, we can, provided that the cloud, as a whole, has not moved; or expressed more mathematically: provided that the centre of gravity of the cloud has not moved we can derive the *average* distance* of all the insects. We shall be sure of the immobility of the centre of gravity if we know that the direction of the motions of the insects is quite at random; but this is by no means required.

* The expression *average distance* ought, strictly speaking, to be replaced by the distance corresponding to the *average parallax*. For clearness sake I have ventured here and in what follows to substitute one expression for the other.

The motion may be preferentially in a horizontal plane or along a determined line, say along the longer axis of the pond, provided only that the motions in any two opposite directions are equally frequent.

Not only that: even if the cloud, as a whole, is not immovable, we are not necessarily helpless. For, if the insect-cloud and the photographer were both on a sailing vessel, circumstances would be the same as on the mainland, though now the cloud is in motion. Only, instead of the absolute displacement of the photographic apparatus, we must know the displacement relative to the ship, or rather *relative to the insect-cloud*. This, then, finally is the real thing wanted. We may obtain the distance of the insect-cloud, or what comes to the same, the average distance of its members, as soon as we are able to find out the displacement of our point of view with regard to the centre of gravity of the cloud.

Our case is much the same in the world of the stars.

We shall be able to determine the average distance of the members of any arbitrary group of stars, provided that we can find the motion of the solar system, both in amount and in direction, relative to the centre of gravity of the group.

Now, astronomical observations such as those which led the elder Herschel to his discovery of the solar motion through space enable us to determine the *direction* of the sun's motion relative to such groups as the stars of the 3rd, 4th, etc., magnitude. Spectroscopy enables us to determine the *amount* of that motion.

We must be able, therefore, to find out the average distance of the stars in these groups. For other groups, such as the stars having an apparent centennial motion of 10'', 20'', etc., there is a difficulty. Still, however, we have succeeded in overcoming this difficulty by a somewhat indirect process, and pressing into service the stars of which the individual distances are known. This, then, is the upshot of astronomical work on the distances.

WHAT WE KNOW ABOUT STAR-DISTANCE.

By direct measurement we know the distance of some hundred individual stars.

*For the rest we know the average distance of any fairly numerous group of stars of determinate apparent magnitude and apparent motion.**

* At the present moment some objection might certainly still be made against the generality of this statement. In fact, the scarcity of spectroscopic data is the cause that, though the determination of the solar motion separately for such groups as the stars of determinate magnitude and proper motion is quite possible, it has not yet been carried through. As a consequence the results used in what follows still rest on the assumption that the centres of gravity of all the groups considered are at rest relative to each other. That this assumption must be probably true, follows from the near identity of the *direction* of the sun's motions, furnished by the several groups.

The question is : Can this imperfect knowledge of the distances be considered as in any wise sufficient for obtaining an insight into the real arrangement of the stars in space ?

I think it can, and I will now try to show in what manner.

LOCALISATION OF THE STARS IN SPACE BY A SORTING PROCESS.

The method may be best explained as a *sorting* process. The process was not actually followed, it would have been too laborious and would have met with some difficulty.* But the difference is immaterial, and the present description has, I think, the advantage in point of clearness.

Let each of the stars of the 2nd, 3rd, etc., to the 8th magnitudes be represented by a little card on which are inscribed the apparent magnitude and the apparent proper motion of the star.

Then imagine three sets of boxes.

CLASSIFICATION ACCORDING TO MAGNITUDE.

1st Set.—*Apparent magnitude boxes* represented in Fig. 1.—In the box for the 2nd apparent magnitude, as many cards are put as there are stars of the 2nd magnitude in the sky. The total numbers of stars for each magnitude are inscribed on the lid. We thus see that there are in the whole of the sky, 46 stars of the 2nd magnitude, 134 of the 3rd, and so on.

ACCORDING TO MAGNITUDE AND PROPER MOTION.

2nd Set.—*Magnitude-motion boxes* (Fig. 3). The stars in each of the former series of boxes are redistributed over a series of boxes, each of them containing stars of a determined apparent motion. By way of an example, Fig. 3 shows this new classification for the stars of the *fifth* apparent magnitude. There is of course another such series for each one of the apparent magnitudes. Those for the *fifth* have been distributed over 28 new boxes. In the first have been collected the cards representing the stars with a proper motion of 0" to 1" per century. The average motion is 0.5, and this has been inscribed on the lid. The little arrow indicates that this number represents a motion. The number 5 surrounded by a star refers to the fact that we have exclusively to do with stars of the fifth apparent magnitude. The second box contains the stars with proper motion between 1" and

* For many of the stars used the proper motion is still not known. What is known however is the percentage of the stars of each magnitude having a determined proper motion. This knowledge enables us to put in every box the required *number* of cards showing a determined proper motion, and this is all that is wanted in what follows.

2" per century, etc. For the larger motions the limits have been taken somewhat wider. In the 11th box the motions 10" to 15" are contained; in the 13th those between 20" and 30"; and so on. The number of star-cards in each box has been inscribed on the lower right-hand corner of the lid. The figure thus shows, for instance, that there are in the sky 90 stars of the 5th magnitude, having a proper motion between 0" and 1" per century. We have thus arranged the stars according to both the rough criteria of distance at our disposal. For we know perfectly well that in a very general way, the fainter the stars and the smaller their apparent motion, the further they must be away.

For each of the groups thus obtained we are now able, according to what has been said before, to derive the *mean* distance. This determination being made, we obtain the mean distances expressed in light-years which have been inscribed on the lid with the letters MD prefixed.

Already we may see now how incorrect it is to imagine all the stars of the 5th magnitude to be placed at one and the same distance, as Struve did.

According to the numbers in our figure, the distance varies from 1670 light-years for the stars of the first box, to 11 light-years for those of the last. It is true that just the data for these extreme boxes are the most uncertain; still, it is evident that even in these mean distances there must be an enormous range.

But to proceed—

The 86 stars in our 6th box (see Fig. 3) are at an average distance of 248 light-years. Are we compelled to stop here and to *assume* that the real distance of *all* the individual 86 stars is 248 light-years? If it were so we would *surely* still have gained a considerable advantage over Struve. For, owing to want of other data, he saw himself compelled to treat all the stars of the 5th magnitude, that is, the whole of the 28 groups in our boxes, as if they were all at the mean distance of the whole.

But yet there would remain in our solution a defect *of the same kind*, and it would be impossible to say in how far the results definitively to be obtained would be influenced.

Happily there is an escape.

For our last classification, the classification in the distance-boxes, it is of no particular advantage that every individual star gets in its proper distance-box. It will be sufficient to know how many stars will finally be found in each distance-box. If this result is obtained, we shall presently see how easy it becomes to study the problem put at the beginning of this lecture. Our aim will be evidently reached if we can find out *how many per cent.* of the stars in any one box have such and such a distance. Now, in order to determine these percentages, it will be sufficient to investigate *a sample* of our stars.



Fig. 1.

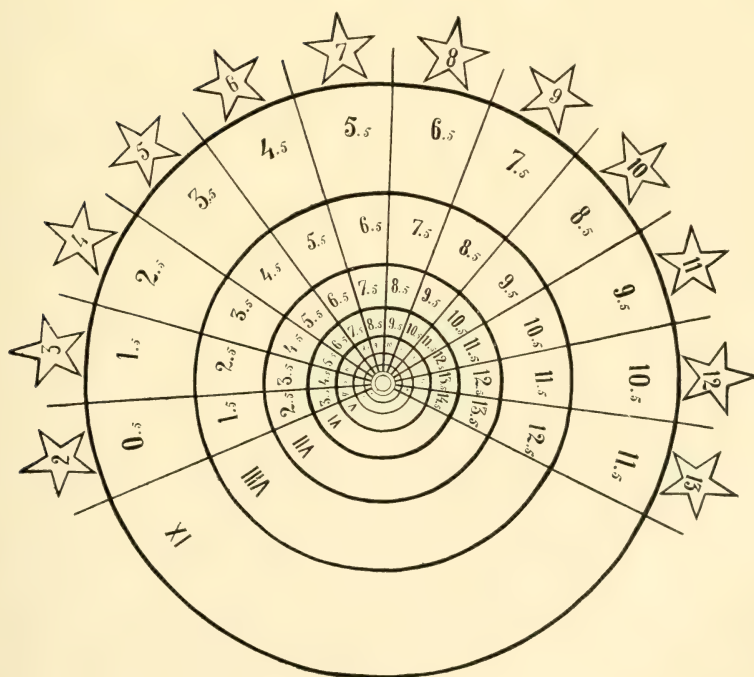


FIG. 2.

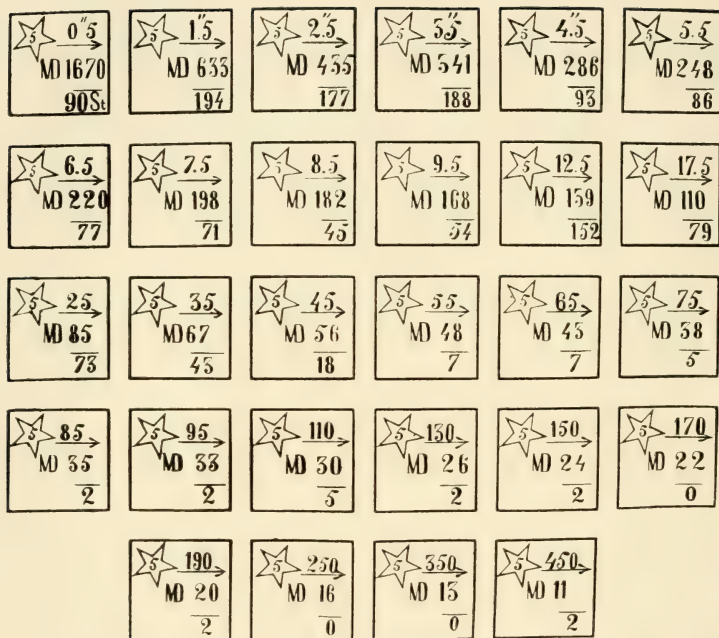


Fig. 3.

	2	3	4	5	6	7	8	9	10	Distance light-years
Shell XII	-2.5 1	-1.5 2	-0.5 3	0.5 4	1.5 5	2.5 6	3.5 7	4.5 8	5.5 9	3260
XI	-1.5 2	-0.5 3	0.5 4	1.5 5	2.5 6	3.5 7	4.5 8	5.5 9	6.5 10	2080
X	-0.5 3	0.5 4	1.5 5	2.5 6	3.5 7	4.5 8	5.5 9	6.5 10	7.5 11	1300
IX	0.5 4	1.5 5	2.5 6	3.5 7	4.5 8	5.5 9	6.5 10	7.5 11	8.5 12	819
VIII	1.5 5	2.5 6	3.5 7	4.5 8	5.5 9	6.5 10	7.5 11	8.5 12	9.5 13	517
VII	2.5 6	3.5 7	4.5 8	5.5 9	6.5 10	7.5 11	8.5 12	9.5 13	10.5 14	326
VI	3.5 7	4.5 8	5.5 9	6.5 10	7.5 11	8.5 12	9.5 13	10.5 14	11.5 15	206
V	4.5 8	5.5 9	6.5 10	7.5 11	8.5 12	9.5 13	10.5 14	11.5 15	12.5 16	130
IV	5.5 9	6.5 10	7.5 11	8.5 12	9.5 13	10.5 14	11.5 15	12.5 16	13.5 17	82
III	6.5 10	7.5 11	8.5 12	9.5 13	10.5 14	11.5 15	12.5 16	13.5 17	14.5 18	52
II	7.5 11	8.5 12	9.5 13	10.5 14	11.5 15	12.5 16	13.5 17	14.5 18	15.5 19	33
I	8.5 12	9.5 13	10.5 14	11.5 15	12.5 16	13.5 17	14.5 18	15.5 19	16.5 20	21

Fig. 4.

STARS OF MEASURED DISTANCE TAKEN AS A SAMPLE.

Happily there is the possibility of taking a sample that will help us out of the difficulty, for, as we know, there are in the sky a hundred stars of which astronomers have succeeded in determining the individual distance with some accuracy. We take these as our sample. They are distributed over a great many of our boxes.

We take them all out, having a care to note for all of them the mean distance of the stars in the box to which they belong. For all the hundred stars we now compare their mean distances to their true distances, and thus find out how many per cent. of them have true distances between *two* and *three*, *four* and *five tenths*, and so on, of the mean distance.

3rd Set.—*Distance boxes.* These percentages are all we want for our last distribution, the distribution over the distances. It is true that our sample is a somewhat undesirably small fraction of the whole; it shows besides some other weak points, but it appears happily *a posteriori* that even rather considerable uncertainties in these percentages have but an unimportant influence on the results. We are thus at last enabled to distribute our star-cards according to the true distances. I made the distribution over the spherical shells shown in Fig. 2.

The dimensions of these shells have been so chosen that if a star is removed from one shell to the next further one, the observer at the centre will see the star grow fainter by just one magnitude, that is, it will grow very nearly $2\frac{1}{2}$ times fainter.

The figure is not well fitted for bringing out the details of our results. The shells become too narrow towards the centre and the more central ones do not allow of the insertion of sufficiently clear figures. For this reason I constructed Fig. 4. The numbers valid for the several spherical shells have here been entered in equally broad horizontal rows. The drawing does not therefore show the real dimensions, but these as expressed in light-years, which may be read off on the right-hand side of the drawing. We thus see that the central sphere extends to a distance of 21 light-years; that the second spherical shell extends from 21 to 33 years, and so on. In these rows a last set of boxes is placed. There is a box for each apparent magnitude in each of the rows. The stars of the boxes of Fig. 3 are thus, of course, all contained in the vertical row of boxes, corresponding to apparent magnitude 5 in Fig. 4.

DISTRIBUTION ACCORDING TO DISTANCE ILLUSTRATED
BY EXAMPLE.

In order to illustrate by an example how the stars of the boxes in our Fig. 3 are distributed over our different shells, that is over our *distance boxes* of Fig. 4, take the 7th box. It contains 77 stars at a mean distance of 220 light-years. Our countings on the sample

showed that about *one-fifth* of the stars have *true* distances which are between 37 and 59 per cent. of their *mean distance* (derived from their apparent magnitude and proper motion). Therefore about one-fifth of our 77 stars must have true distances between 37 and 59 per cent. of 220 light-years, that is between 82 and 130 light-years—or finally, 15 stars of our box must find their place in the 5th shell of Fig. 4. that is in the box corresponding to the 5th apparent magnitude in that shell. In precisely the same way I find that 21 of them must be placed in the 6th shell ; 18 in the 7th ; 10 in the 8th, and so on.

If, after that, we repeat the process for all the remaining boxes of Fig. 3, we get, for the 5th apparent magnitude, the numbers inscribed on the lower side of the boxes corresponding to that magnitude in Fig. 4.

Further than for the 11th shell no numbers have been entered. They become too uncertain. As, however, we know the *total* number of stars of each apparent magnitude, we know the aggregate number which remains to be distributed over the whole of the further shells.

What has here been explained for the stars of the 5th magnitude, has been also done for the other magnitudes between the 2nd and the 8th. The whole of the results are shown in our Fig. 4.

STARS OF EQUAL LUMINOSITY BROUGHT TOGETHER.

The main result of the investigation is embodied in these numbers—and *first*, in every box stars have now been brought together of equal absolute magnitude—that is, of equal luminosity. For as the stars in each box are at the same distance, and as, at the same time, they are of equal *apparent* brightness, they must, of necessity, be of equal total light-power, that is according to our definition, of equal luminosity, or absolute magnitude. For the absolute magnitude of a star, I have taken the magnitude the star would show if placed at a distance of 326 light-years. The choice of just this number is simply a matter of convenience, and need not be explained here.

As a consequence, the stars at a distance of 326 years, which to us appear as stars of the 5th magnitude, will have also the absolute magnitude 5. Those of the same apparent magnitude, but at a distance of 517 light-years—that is, just one shell further—must have the absolute magnitude 4 in order to show us the same brightness, notwithstanding the greater distance. Now our 8th shell lies just between these limits of distance. In the middle of this shell, therefore, the stars of apparent magnitude 5 must have absolute magnitude 4.5. In the box, therefore, belonging to the 5th apparent magnitude, 8th shell, all the stars are of absolute magnitude 4.5. In the 9th shell a star must already have the absolute magnitude 3.5 in order to shine as a 5th apparent magnitude at this greater distance, and so on. In this way the absolute magnitudes were found which in our figure have been inscribed on the lids of the boxes.

MIXTURE-LAW.

We are now able to derive at once the *mixture law*—i.e. the proportions in which stars of different absolute magnitude are mixed in the universe. For in one and the same shell (11th) we find two stars of absolute magnitude -1.5 , as against three of magnitude -0.5 , fifteen of absolute magnitude 0.5 , seventy-six of absolute magnitude 1.5 , etc.

That is, our results for the 11th shell furnish us with the proportion in which stars of absolute magnitude -1.5 , -0.5 , etc., to 4.5 , are mixed in space. The 10th shell gives the proportions for all the absolute magnitudes between -0.5 and 5.5 , and so for the rest. All the shells together give the proportions for the absolute magnitudes -1.5 to 14.5 , that is for a range of not less than sixteen magnitudes. Not only that, but most of the proportions are determined independently by the data of quite a number of shells. So, for instance, the proportion of the stars of absolute magnitude 4.5 to those of absolute magnitude 5.5 . Each of the six shells from the 5th to the 10th furnishes a determination of this proportion. All of them are not equally reliable. If we take this into account, we find that the agreement of the several determinations is fairly satisfactory. By a careful combination of all the results, a table representing the law of the mixture of the stars of different absolute magnitude was finally obtained. Rather than show you the direct result, however, I will first replace the absolute magnitudes by luminosities expressed in the total light of our sun as a unit. This will have the advantage of presenting a more vivid image of the real meaning of our numbers.

By photometric measures it was found that the sun, placed at a distance of 326 light-years, would shine as a star of magnitude 10.5 . In other words, the sun's absolute magnitude is 10.5 . A star of absolute magnitude 9.5 will, therefore, have 2.5 times the light-power—that is, 2.5 times the *luminosity* of the sun. A star of absolute magnitude 8.5 will again have a luminosity which is 2.5 times greater, and so on.

Such results evidently enable us to transform our absolute magnitudes into luminosities. Thus translated, I found the results shown in the following table.

LUMINOSITY TABLE.

Within a sphere having a radius of 555 light-years, there must exist :—

1 star	10,000	to	100,000	times more luminous than the sun			
46 stars	1,000	„	10,000	„	„	„	„
1300	100	„	1,000	„	„	„	„
22,000	10	„	100	„	„	„	„
140,000	1	„	10	„	„	„	„
430,000	0.1	„	1	„	„	„	„
650,000	0.01	„	0.1	„	„	„	„

This table represents what, up to the present time, we know about the mixture-law.

The fainter the stars, the more numerous.

The rate at which the numbers increase with the faintness is particularly noticeable for the very bright stars.

Passing to the fainter stars, this rate gradually diminishes, and it looks as if we must expect no further increase in number for stars whose luminosity falls below one-hundredth of that of the sun. Meanwhile this is simply a surmise. For stars of this order of faintness data begin to fail. Here, as in nearly every investigation about the structure of the stellar system, the want of data for stars below the 9th apparent magnitude makes itself very painfully felt.

But let us come back to our Fig. 4. I will *first* remark that, knowing the mixture-law, we can predict the number of stars that we shall get in the empty boxes belonging to the 9th, 10th, etc., magnitude, as soon as continued astronomical observations will permit us to include these stars in our discussion. For the mixture-law, as derived just now, shows that in our universe the stars of absolute magnitude 5.5 are 3.5 times as numerous as the stars of absolute magnitude 4.5.

Now as in the 11th shell the number of stars of the absolute magnitude 4.5 is 5400 (see Fig. 4), there must be 3.5 times 5400, that is, 18,900 stars of absolute magnitude 5.5 in this shell. These belong all in the box of the 9th apparent magnitude of this shell. In the same way we obtain the number of stars to be expected in the boxes of the 10th, 11th, etc., apparent magnitude for all our shells down to the 11th. There is exception only for the boxes belonging to the lower shells, for which the absolute magnitude would exceed 14.5.

It is evident, however, that the number of stars in these exceptional boxes must be small, and for what follows they are of little importance.

STAR-DENSITY.

In the second place, our boxes now also lead to the determination of the *star-densities*. For the volumes of the consecutive shells are perfectly known; they are in the proportion of 1 : 3.98. For the sake of convenience, let us say that the volume of each shell is exactly four times that of the next preceding one. Now, to take an example of the determination of the densities, consider the 9th and 10th shells (see Fig. 4). In the 9th there are 49 stars of absolute magnitude 2.5. Therefore, if in the 10th the stars were as thickly crowded as in the 9th, there would occur in this shell four times 49, that is 196 stars of this absolute magnitude 2.5.

In reality we find but 140 of these stars. The conclusion evidently must be that the star-density in the 10th shell is about $\frac{140}{196}$, that

is about two-thirds, of that in the 9th shell. A similar conclusion is obtained by comparing the number of the stars of absolute magnitude 3·5 in the two shells. The values obtained from the magnitudes 0·5 and 1·5 may be neglected. Owing to the exceedingly small number of stars, they must necessarily lead to untrustworthy results. From all the rest I found that the density in the 10th shell must be about 64 per cent. of that in the 9th shell. The proportion between the densities in the other shells was determined in exactly the same way.

A slight defect in our results was then discovered.

We should exceed the limits of the time allowed for this lecture by entering into a consideration of this defect. It must be sufficient to state that it was not difficult to remove it. After that it appeared that the density in the first six of our shells is nearly the same. The density in these shells, that is in the neighbourhood of our sun, is such that about 2000 stars of a luminosity exceeding one-hundredth that of the sun must be contained in *a cubic light-century*. After the 6th shell the density diminishes gradually at such a rate that in the 11th shell the density has fallen to about 30 per cent. of what it is in the vicinity of the solar system.

In what precedes we tried to give a solution of the problem put at the beginning of this lecture—a solution, however, which embraces only that part of the universe which is contained within a distance of about 2000 light-years from our solar system.

Is there no possibility of getting beyond this distance ?

STAR-DENSITY FOR DISTANCES EXCEEDING 2000 LIGHT-YEARS.

I think there is, but, of course, you will not be astonished to find that the certainty of our conclusion diminishes as we get deeper and deeper in the abysses of space.

One of the reasons why the method thus far applied breaks down beyond the 11th shell, is that our data about proper motion are not refined enough to determine this motion with sufficient accuracy as soon as it is below 1" in a century. Even the somewhat greater motions are rather uncertain. The proper motions thus cannot help us much beyond a certain distance. But we have still one valuable element for the solution of our problem. This element is the total number of stars separately for the apparent magnitudes. Thanks mainly to the photometrical researches at the Harvard Observatory, it has become possible to determine with considerable accuracy the total number of stars of the 1st, 2nd, etc., to the 11th magnitude ; with a fair degree of accuracy even those for the magnitudes down to the 14th (inclusive).

The density in the shells beyond the 11th, not only for the stars down to the 8th apparent magnitude, but according to what has been said a moment ago, also for the apparent magnitudes of 9,

10, etc., to 14, has to be determined in such a way that the addition of all the numbers in any one vertical column of Fig. 4 produces just these totals for the corresponding apparent magnitudes.

It can be proved that after the 11th shell the density must, on the whole, continue to diminish. If we assume that this diminution is gradual and proportional to the increase in distance, it becomes very easy to determine the rate of this diminution, and consequently the distance at which the density becomes *zero*, that is the distance at which we reach the limit of the stellar system. We cannot enter into fuller particulars here. It must be sufficient to say that in this way we are led to conclude that the further diminution of density must be slow, so slow that in the assumption made above, the limit of the system is only reached at a distance of some 30,000 light-years.

HYPOTHESES UNDERLYING THE RESULTS.

In conclusion, a few words on the question: In how far are the results now obtained to be considered as established?

The answer must be: They can be considered to be established only in so far, and no further, than we can trust the truth of the hypotheses which still underlie our reasoning.

For future consideration there thus remains the question, in how far can we test the validity of these hypotheses?

These hypotheses are the following:

1. The mixture was assumed to be the same at greater and smaller distances from the solar system.
2. The same was done for different distances from the galaxy.
3. The universe was assumed to be transparent, that is, it was assumed that the absorption of light in space is *zero*.

Can we get rid of these hypothetical elements?

I think we can, at least to a very great extent.

As to the *first*. Our figure 4 already goes far in enabling us to judge whether it is true or not. For evidently both our 6th and our 9th shell give the nature of the mixture, at least of the stars of absolute magnitude 3.5 to 6.5. Therefore, as far as these stars are concerned, we are able to see whether or not the mixture is the same at the distance of 650 light-years as it is at the distance of 170 light-years. Likewise the figure enables us to make the comparison in other cases. As soon as we possess the necessary data for a longer range of apparent magnitudes, say down to the 14th or 15th, we shall be able to dispense to a very large extent with our first hypothesis.

As to the second, the possible variation of the mixture with the distance from the Milky-way, it is largely only the question of treating the stars in different galactic latitudes separately. As far as I can see there are no particular difficulties in the way of such a

separate treatment, at least not since the nature of certain anomalies in the distribution of stellar motions has been elucidated.

ABSORPTION OF LIGHT IN SPACE.

Last, not least. Is the universe really absolutely transparent? There are reasons which make this seem very doubtful. A couple of years ago I obtained some evidence in the matter which shows that the absorption of light in space, if it exists to an appreciable amount, must at least be so small that over a distance of a hundred light-years not more than a few per cent. of the light can be lost. To determine so small an amount to within a small fraction of its total value will be a difficult task indeed. Still we can even now see definite ways, which, given the necessary data for very faint stars and nebulae, will probably enable us to overcome this last difficulty.

CO-OPERATION FOR OBTAINING THE NECESSARY DATA FOR VERY FAINT STARS.

This want of data for very faint stars, which, in the present investigation, makes itself felt at every step, has led a number of astronomers to concerted action.

The express purpose of their co-operation is to collect data of every kind for stars down to the faintest that can practically be reached. As complete observation and treatment of these numberless stars is out of the question, the plan is confined to a set of samples distributed over the whole of the sky.

CONCLUSION.

If, at the end of this lecture, somebody summarises what has been discussed by saying that the results about the structure of the universe are still very limited and not yet free from hypothetical elements, I feel little inclined to contradict him. But I would answer him by summing up in another way, viz. :—

Methods are not wanting which, given the necessary observational data obtainable in a moderate time, may lead us to a true, be it provisionally still not very detailed, insight into the real distribution of stars in space.

I think this time need not exceed some fifteen years. They to whom such a time may still seem somewhat long, may be reminded of the fact that that time will be elapsed, that we shall have finished our work, before any but a very few of our nearest neighbours in space can be aware of the fact that we have begun.

[J.C.K.]

WEEKLY EVENING MEETING,

Friday, May 29, 1908.

THE RIGHT HON. THE DUKE OF NORTHUMBERLAND, K.G. P.C.
D.C.L. F.R.S., President, in the Chair.

SIR RALPH PAYNE-GALLWEY, Bart.

Ancient and Mediæval Projectile Weapons other than Firearms.

MISSILE SPEARS.

THE first weapon of attack or defence employed by mankind, whether for killing animals for food or in warfare with their fellow-beings, was doubtless a spear with a fire-hardened point. In the course of years the primitive spear was, however, improved by the addition of a head of sharp flint or metal. As animals became more wary and difficult of approach, what may be termed the thrusting, or stabbing spear, was in great measure superseded for one that could be cast as a missile. The survival of the thrusting spear is exemplified in the lance, sword and bayonet of to-day, and the Zulu assegai is a good example of the missile spear.

For killing animals, a missile spear was, of course, the most effective of the two kinds of weapon, for an animal could be struck down with it when on the move, and at a distance of many paces from the hunter, in positions, and at a range that would prevent his having any chance of success with a long heavy spear that was only suitable for thrusting with. It was but a natural transition that the missile spear of the hunter should be applied to warfare. It is probable that the missile spear could be cast from fifty to sixty yards at most. This is about as far as the warriors or hunters of any savage tribes of these days can cast spears by hand, if simple manual power is alone employed to do so.

There is no reason to suppose that the Zulus, for instance, show more strength or dexterity in casting missile spears than their ancestors were capable of thousands of years ago.

In course of time certain nations, who were chiefly armed with swords and spears, and who had more culture and ingenuity than savages, devised methods for casting their light spears to a distance that far exceeded what the power of the unaided arm could achieve. That any improvement of this kind was of the utmost value needs no explanation, as warriors or hunters armed with a weapon that could

be cast a hundred paces or more, would have a very decided advantage over men who could but throw their spears to only half this distance.

There were three methods by which the range and force of a missile spear were greatly increased.

The Greeks and Romans employed two of these methods. One method was by lashing a loop of soft leather to the shaft of the spear at its balancing point. The spear was held lightly in a horizontal position, and the first finger being placed in the loop bestowed the propelling power—a power that continued to be exerted till the loop became disengaged from the finger, as the spear sped forward.

The disadvantage of this method in warfare was the fact that the enemy could hurl back the spear in the same manner as it was thrown, as the means by which it was propelled were attached to it, and thus equally available to friend or foe.

The second method was much more effective, though slower in use. In this case a long thong of pliable hide was utilised. The thong had a knot at one end and a small loop at the other. The knotted end was hitched round the spear at about its centre of length. The thrower grasped the spear near its head, with his first finger inside the loop of the thong—the thong being stretched taut between knot and finger. As the spear was cast, the finger in the loop jerked hard at the thong, and continued to exert its force, even after the missile had left the hand, or till the moment when the thong was extended forward, and the knot hitched round the shaft of the spear dropped off it as a result.

In this case the soldier retained the thong in his hand ready for another spear; and the spear he had thrown could not be cast back by an enemy, unless the latter had a suitable thong of his own for the purpose.

The principle of the ancient thong and spear is now practised in West Yorkshire with a willow wand and a short piece of cord.

In this Riding, frequent competitions with the wand I refer to are held, sometimes before several thousand people, chiefly miners. The object is to throw the wand, which is about a yard in length and the thickness of a lead pencil, farther than can some rival in the sport. Throws of from 300 to 320 yards are not unusual, and a throw of 360 yards has been recorded. This gives us some idea of the force with which the light spear or javelin of the ancients was probably cast by means of a thong.

The third method of casting a spear is with a throwing-stick. This system was not practised by the Greeks or Romans, but is now common to various native races, such as those of Australia, New Zealand, Central America, and the sub-Arctic and Arctic regions, though not in Europe, Asia and Africa, where the more powerful and useful bow has always existed.

Throwing-sticks, wherever used, are always identical in action. The implement consists of a narrow strip of wood, some two feet in

length, that tapers into a handle at one end. One surface of the strip of wood has a longitudinal groove in it, and in this groove part of the shaft of the spear, for a foot or so of its butt end, is rested.

When about to cast the spear, the user fits its shaft into the groove and holds spear and throwing-stick horizontally above his shoulder in one hand.

He then jerks the throwing-stick violently forward. The shaft of the spear rises from the groove, and the end of the throwing-stick farthest from its handle presses against the butt of the spear and propels it forward.

The theory and practice of the throwing-stick is as if the user had a long extra joint to his arm to utilise when casting his spear.

Though the throwing-stick adds a considerable increase of range and force, it cannot compare in these respects with the system of the thong I have described. It is, however, quicker in action, and easier to use.

SLINGS.

I will briefly allude to Slings. There is no doubt that in ancient days, slingers formed a part of all armies, whether Greek, Roman or Oriental, and were always useful when they had a chance of slingng stones at men in close rank. I do not believe that slingers were ever able to strike a single mark at a distance with any certainty, whether man, beast or bird. I consider that the historic stories of the accuracy of slingers are mythical. For instance, the seven hundred slingers mentioned in "Judges," all, curious to say, left-handed, who could sling stones at a hair's breadth and never miss, or the Achæans, who slung in such deadly fashion that they were able to strike any feature of the human face that they aimed at.

Speaking personally, I am of opinion that a sling is, of all others, the most difficult and unreliable weapon to use.

With an unusual aptitude in the art of throwing projectiles of all descriptions, I, for many years, practised assiduously with the sling, and when casting stones at rooks in a tree, it was a mere chance that the stone from my sling went near the tree, much less its occupants, though onlookers, to one side or the other, and even behind me, were always in danger.

There were two kinds of slings, the one with a shallow pocket for the stone and cords or thongs attached to the pocket, the other with a strip of leather fastened to the end of a staff. Both kinds are commonly shown in ancient sculptures, and it is likely the staff sling was the more reliable of the two varieties. The sling of dried grass is also shown, precisely as it is made now by shepherds in Palestine, who to this day use it as a form of amusement, as well as a means of keeping prowling dogs at a distance from their herds.

If we can believe ancient military historians, the cracking of the

thongs of the slings and the buzzing of the stones thrown by them, were features of a battle that once heard could never be forgotten.

THE BOOMERANG.

The Boomerang may truly be called an ancient weapon, for its origin is unknown. It is a weird and erratic form of missile, and though I possess over fifty, and have continually practised with them for many years, I have not one which it may be said exactly resembles another, either in outline or performance. At the same time there is a general principle in them all that causes them to act in a similar manner when thrown.

It is impossible to reproduce a good returning Australian boomerang, for only by the aborigines of Australia are they properly constructed, and in no other country, in past or present times, have they ever existed, except as inferior copies in recent days.

The curious twists and hollows of a genuine Australian boomerang doubtless represent the experience of centuries of native boomerang artists.

We cannot obtain in our islands, or indeed in Europe, any wood with the indispensable natural curve in its grain, and without which a boomerang will soon fracture on falling, that is nearly so hard and heavy as the wood from which the boomerangs of Australia are fashioned.

This very hard wood allows the Australian to finish off his boomerang at its edges almost to the sharpness of a knife-blade.

And, as the wood he employs is very heavy, his weapon can be made so thin that it offers very slight resistance to the air, while at the same time it has sufficient specific gravity to travel a long distance.

There are two very distinct varieties of the Australian boomerang—the one used in warfare, and the returning one.

The Australian returning boomerang is more of a toy than a weapon, though it is used for killing birds for food.

It has several twists, which cause it to somewhat resemble in contour the propeller of a steamship, or the sail of a windmill. Its under surface is flat, but the top surface, or the surface that is uppermost when it is thrown, is convex, or rounded from its centre of width down to the edges.

The twists to be seen in the weapon are all carefully designed to cause it to return to the thrower. Without these twists, it would fail to act in the marvellous manner an Australian returning boomerang can be made to act in the hands of a native expert of that country.

Though we can make a boomerang in England that will return to the thrower, it is always a poor performer compared to the Australian implement, with the extraordinary flight of which no home-made article can ever compete.

The Australian war boomerang is nearly twice as large and heavy as the returning one. It has no twists, is rounded on both sides, and does not return to the thrower.

This weapon will travel, skimming low over the ground, to a range of from 150 to 180 yards, and the blow it gives a tree-trunk at 80 yards, is as if the latter were struck by a heavy blunt sword. As an instrument of savage warfare it would have a terrible effect on a scantily clad opponent.

The surfaces of all Australian boomerangs of good quality are closely notched all over, excepting their edges.

The Australian gave his boomerang this rough surface so that it might bite the air in its flight, just as the outside cover of a golf ball is roughened, for when golf balls were made with a smooth, china-like surface, as they were formerly, their flight was short and inaccurate.

As an example of what can be achieved with an Australian returning boomerang, I may relate that I have often had one circling round in the air for 30 seconds. I have also projected a boomerang for 100 to 150 yards forwards, that has returned over my head and then passed 60 or 70 yards behind me. It has then once more returned, skimming low along the ground, and knocked off an apple, impaled on a stick placed near me.

It is a common idea that the Australian returning boomerang was used in native warfare, and that after striking an opponent it returned to the thrower. Such a thing was impossible, as should either the returning or the war boomerang strike an object, its career is checked, and it then, of course, falls to the ground.

THE SIKH QUIT OR CHAKRA.

I consider these thin rings of steel were the most deadly and terrible projectiles the unaided human arm was ever capable of wielding, though they were, perhaps, rather too costly to produce for common use.

From time immemorial, and till the introduction of firearms to native races, this quoit was a favourite weapon in India, especially with the Sikhs, who now throw it in sporting competitions at a mark. It is made of the finest sword-steel, and is usually eight inches in diameter, including its inch-wide rim. It is a sixteenth of an inch thick and eight ounces in weight.

It is as sharp as a razor round its outside or cutting edge, though blunt and smooth on its inner edge where the fingers have to be placed when it is projected.

It was formerly the custom for a Sikh to carry a dozen of these quoits, either on his arm, or his turban, or even round his neck. They were then ready for instant use if required.

Though this weapon, when properly cast, floats so easily and

gracefully along, it flies with great force and rapidity, and as it revolves in its flight, it would give a terrible slicing wound. I have often seen one of these quoits cut through a tree branch a full inch in diameter, and this with hardly any check to its flight. This will give an idea of what a fearful weapon it must have formerly been in an Oriental country, where the limbs of soldiers of lower ranks were usually unprotected.

The upper surface of the quoit, or the surface that is held uppermost when the quoit is thrown, is slightly convex. The under side is flat. When the quoit is thrown, its convex surface prevents it from inclining upwards, and its flat surface prevents it from inclining downwards, and as the weapon is so thin, and is held in a level position when projected, it offers slight resistance to the air, and hence travels a very long distance. I have often thrown one of these quoits over 200 yards, its height above the ground, for two-thirds of its flight, not exceeding 4 or 5 feet. This shows what a deadly weapon it must have been in warfare when used by the native soldier, who could doubtless cast it with much more force and precision than I can.

The weapon is propelled entirely by the first finger. The second finger and the thumb—the former beneath its rim, the latter above it—being merely utilised to hold it in a level position as it leaves the hand. It is not given any intentional spin, nor is it first twisted round on the finger before it is thrown. The spin comes naturally when the quoit is projected, owing to its circular shape, and to the application of the propelling force to one point of its periphery only.

I have seen experts, selected from a regiment of Sikhs, throw these quoits at a small soft-wood tree, such as a plantain, about six feet in height, that was erected as a target at a distance of eighty to one hundred yards. Each man was allowed so many throws, and the man who cut off the lowest part of the stem of the tree above ground, or that part of it which was left untouched by the other throwers, won the prize, which usually consisted of a quoit inlaid with gold, and bearing the date of the competition and the name of the winner.

This quoit was formerly a sacred emblem of war, and is represented in India in many sculptures, wall paintings and illustrated manuscripts.

Vishnu, the Supreme God of the Hindus, is figured as having four arms, each of his hands holding a quoit. He was supposed to have used them with terrible effect against the demons who, according to Indian mythology, were for ever plotting evil against gods and men.

ARCHERY.

The Long-bow.—I shall not enter into a description of the bows of various countries, as this would require a long discourse in itself. I will merely deal with the two very interesting and distinct

forms of bow which were used respectively by European and Oriental nations. Of these, the long-bow, as the famous weapon of our ancestors, and with which they won many glorious victories, properly comes first.

The cloud of three or four thousand arrows, discharged simultaneously from long-bows, must have been a terrifying sight. Old historians, in picturesque fashion, were wont to speak of a great flight of arrows as darkening the sun. They were impossible to avoid when falling on troops in close rank, as was the ancient and mediæval formation in battle, and the noise of the feathers of thousands of arrows as they rushed through the air was like the sound of a gale in a forest.

Bullets could not be seen. They struck without warning, and hence did not cause terror before the blow was felt. The single shock of a bullet might cause a horse to start and plunge, but a barbed arrow working in its flesh would goad the animal to a state of irritation which would soon cause it to unhorse its rider. If a heavily clad knight, he then lay helpless on the ground, and was sometimes leisurely done to death by the enemy with a curved bladed knife, specially designed for inserting upwards between the overlapping joints of his armour.

The English long-bow was a mere staff of yew obtained from abroad and shaped into a bow at home. It had no decoration or finish, and the bows used by soldiers had not even horn nocks for the loops of the bow-string, but merely notches cut at their ends. The long-bow, owing to its great length, could not be used on horseback, and if retained for some hours in a strung condition, it lost much of its power.

The feats achieved with the long-bow in England have been grossly exaggerated, and the expression, "drawing the long-bow," has become, for this reason, a proverb implying inaccuracy and boasting.

Much nonsense has been handed down to us concerning the range and accuracy of the long-bow, even Sir Walter Scott being a sad culprit in this respect.

That Robin Hood ever shot an arrow a mile, or even a third of a mile, or that William of Cloudeslie cleft a hazel-wand at four hundred paces, a distance at which it is doubtful if it could even be seen, is simply idle nonsense. As to Robin Hood's feat, I may say I have had long-bows constructed of a far greater strength than any man could draw. These I fixed in a strong framework and bent and shot them by mechanical power, yet the longest range I obtained with a light arrow was only 360 yards.

It may be taken that the distance to which a very powerful long-bow man could discharge his war arrow, was from 260 to 270 yards, the usual distance being 250 to 260 yards, and that with a light fighting arrow, such as was of no use in warfare, he could attain, at most, a range of from 310 to 320 yards.

The very exceptional archer, which any country might occasionally produce where archery was formerly practised by the entire male

population, as in England, might possibly shoot a flight arrow, with a long-bow, to a distance of from 340 to 350 yards.

Very few of the most powerful and expert archers of the present day, even with specially constructed bows and light fighting arrows, are able to achieve a range of 300 yards, even 290 yards being an unusual feat.

There is no reason whatever to suppose that our forefathers were so vastly superior in strength and skill as to exceed these distances by so much as one hundred or more yards, as stated by many old authors, notably by Sir John Smyth, who gives 440 yards as the range of the English long-bow ! The farthest fairly well authenticated shot with the long-bow and a flight arrow is 340 yards. It is said to have been made in 1798, by a Mr. Troward, and represents the record of a century of effort on the part of many archers who have striven to shoot as far as possible with their bows, but who have, however, never approached nearer than 30 yards to the long shot credited to Mr. Troward !

Shakespeare tells us that 280 to 290 yards was a notable distance to attain with a flight arrow in his day, and it is curious that this is just what a very strong and skilful archer of our time is capable of.

Many of our castles that were built when archery flourished, are within 300 to 350 yards of eminences that overlook them.

The courtyard of Carnarvon Castle is commanded by a hill only 330 yards distant ! If mediæval archers shot from 350 to 400 yards, they could have poured their shafts into the garrison ! Berkeley Castle is another example. The church at Berkeley is within 50 yards of the castle keep. The tower of the church is, however, isolated, and stands at 170 yards from the courtyard of the castle. The tower was placed at this distance from the body of the church so that the archers of an enemy might not annoy the garrison of the castle should they have occupied the summit of the tower, had church and tower stood together in the usual way.

In the case of Berkeley Castle it will be observed that only 170 yards was considered to be a safe distance against the assaults of bowmen.

Whatever its extreme range may have been, there is small reason to doubt that, at 150 yards, the old English long-bow quite equalled if, indeed, it did not excel, the Brown Bess or flint-lock carried by our soldiers till 1840. If a hundred good marksmen armed with the flint-lock as used at Waterloo, and a hundred of our archers of Crecy and Agincourt could be opposed in line at 150 yards, the archers would, in my opinion, gain an easy victory, for they could discharge at least six arrows for every bullet fired by their opponents, and could also, I believe, shoot with more accuracy and effect.

As an example of the inferiority of the musket in range, when compared to the long-bow, I will quote from an old diary of May 10, 1811. On this day it is recorded that Wellington ordered one of the

guard near him to fire at a French soldier who had, with impudent gestures, approached the English lines. The guardsman rested his piece on the wheel of a gun-carriage, took careful aim and killed the man. The diarist writes that he witnessed this wonderful good shot, which on being measured was found to be no less than 80 yards!

After a persistent struggle against firearms, the long-bow, as a weapon of warfare, was laid aside by our ancestors about 1580 to 1590. It was, however, employed in desultory fashion till about 1615, and on a few occasions as late as 1620 to 1630, notably in the expedition to the Island of the Rhé in 1627, and, for the last time in regular warfare in our islands, by Montrose, in his defeat of the Covenanters near Perth, in 1644.

Its final appearance was probably in Scotland, as in a tribal dispute between Mackintosh and Lochiel, in 1665, Lochiel had 300 of his men armed with bows and arrows.

I will conclude this subject by alluding to the yew-tree. It is a common and incorrect supposition that the yew-tree was planted as a wood from which to construct bows. The yew-tree of our islands was most unsuitable for making bows from, as its wood is soft and inelastic, and it has not the length of trunk or limb, or the clean straight grain, of the foreign yew of warmer and drier climates, such as those of Spain, Italy, and the south of France. Practically speaking, the English long-bow was never made of English yew, or of any other British wood.

The English bow was imported from abroad in the form of a rough stave, and this was afterwards shaped into a bow at home. Statutes were enacted that obliged merchants trading to certain foreign ports, to bring to England in their ships a stated number of bow staves of yew with their merchandise. For instance, in the reign of Edward IV., for every ton of goods, four bow-staves, and for every butt of wine, ten staves, with a very heavy penalty for each stave deficient in number.

Why yews were so commonly planted in our churchyards is an unsolved question. If it had been for the purpose of supplying material for making bows they would have been planted in groves, and not as single trees. Possibly the yew formed a shelter for open-air ministration, as no doubt our detestable climate was as liable to spoil social and other functions in the days of our ancestors as it is now! Or, perhaps, this tree was common to churchyards on account of its well-known sanitary properties, or it may have been, as some say, because its gloomy and evergreen foliage was regarded as an emblem of death and resurrection. Whatever the reason may have been, the yew was certainly not planted in English churchyards in the interests of archery.

The Oriental Reflex Composite Bow.—This is a weapon of wonderfully skilful structure, and, it may be said, mechanically perfect as a bow, for not only could it be used with full effect from horse-back,

owing to its small size and light weight, but it far excelled in power and convenience the largest and strongest English or Continental long-bow. The origin of these bows is lost in antiquity. We only know that they are depicted in sculpture long before Christ, and that they were used with deadly effect by Turkish horsemen during the Crusades.

Most people are acquainted with the contour of the bow wielded by Cupid, as depicted in paintings and sculptures.

Many oriental bows were made precisely of this shape, or like the outline of the mouth of a beautiful female face, a resemblance which has given rise to the saying "a mouth like Cupid's bow."

The component parts of these bows consist of three substances. First, a curved and very thin lath of wood, averaging about an inch in width, and a sixteenth to an eighth of an inch in thickness. This lath gives no actual strength, but forms the mould or foundation of the bow. On one side of the lath of wood, two thin pieces of horn are glued longitudinally. The pieces of horn meet at the centre of the bow, and form its inside surface when it is strung, or that surface which is next the archer when he draws his bow. To the other side of the lath of wood, or on the inside curve of the bow when it is in an unstrung state, a continuous strip of animal sinew was moulded, and then attached by glue. Though these three strips, consisting of horn, wood and sinew represent the vital construction of the bow, they entailed the greatest care and accuracy in fitting together, so as to form the finished article.

It is now a mystery how the parts of the Oriental bow were so firmly attached, how the sinew, which gave the bow its strength and elasticity, was treated, and especially how the glue was prepared which had to withstand so great a strain when the bow was in use. The thick elastic lacquer, of an almost imperishable nature, with which the outside of the bow was coated as a protection from interior damp and decay, and which did not crack even when the bow was fully bent, is another puzzle. Its composition is unknown, as is that of various other old mixtures, such as violin varnish, monkish ink and Roman cement.

The cleverest artificer could not now construct one of these bows, nor have any of them been made in Persia or Turkey for the past 120 to 130 years, though very large and rough weapons, of a somewhat similar but very inferior kind, are still manufactured in remote parts of China.

The chief power of a Persian, an Indian or a Turkish bow was chiefly derived from the strip of elastic sinew which formed its outside surface when it was strung.

This sinew was ingeniously utilised for their bows by the Orientals, and was taken from the neck tendon of some large animal.

In life, it contracted or expanded as the animal raised or lowered its head. Without the assistance of this powerful and elastic ligament,

a stag, for instance, could not lift its heavy antlers from the ground after feeding or drinking.

The reflex shape of these bows is another great cause of their strength.

Take an ordinary English long-bow, and it is merely bent, when it is strung, out of a straight line. On the other hand, a reflex bow is bent, when strung, from a sharp reverse curve. The result in the latter case is, that when the bow is strung for use it is all the time striving to re-attain its reflex shape.

Hence its bow-string is much more strained, and the weapon is therefore a much more elastic and powerful one than could ever be a long-bow, that merely returns to a straight line when it is unstrung.

I have shot at a mediæval breast-plate with both a powerful long-bow and an Oriental bow, at a range of 50 yards. The heavy arrow of the former slightly perforated the metal plate, but the lighter arrow of the Oriental weapon passed clean through it, and into the ground beyond.

It is, of course, the high velocity of an arrow, and not its weight, that gives penetration. An example of this is to be found in the writings of Prescott, who describes how the reed-arrows, with small metal heads, used by the Mexicans from very powerful bows, pierced through and through the coats of mail worn by the Spanish soldiers.

Another feature of Oriental composite bows, in contrast to ordinary bows of wood, is their indestructibility. They cannot be broken by fair means.

I have obtained several from Turkey, Persia and India, that were as sound and efficient as when they were made nearly a couple of centuries ago, though they had probably lain neglected for over a hundred years. I may add that the dates of bows of this kind, with the names of their makers, are nearly always to be found painted in small letters near their extremities.

Of all Oriental bows those made by the Turks were the best and most efficient, though they were so short, being only from 3 feet 8½ inches to 3 feet 10 inches at most, if measured round the curve with a tape from one point of the bow to the other.

No other nation constructed them of such marvellous power, and at the same time so elegant in shape and decoration, or so compact and light.

The weight of a Turkish bow seldom exceeded half-a-pound, while a long-bow weighed from one and a half to two pounds!

Nor did any other people, not even the Persians, excel the Turks in dexterity as archers—a dexterity that was beyond anything that English or Continental bowmen were capable of.

It is beyond question, and is recorded in many Oriental books and manuscripts that can be fully relied on, as well as on the celebrated marble pillars that stood on the old archery ground near

Constantinople, that the Turks formerly shot arrows from 600 to 700 yards and more !

These feats were performed with light fighting arrows, it is true, but archers and bows that could send these light shafts, even from 500 to 600 yards, would be able to shoot the heavier arrow of warfare much farther than could any long-bowman with his war arrow and bow of yew.

It is recorded by Strutt, the historian, in his "Sports and Pastimes of the People of England," that in 1795, near London, Mahmoud Effendi, Secretary to the Turkish Ambassador, shot a flight arrow from his Oriental bow to a range of 480 yards. There can be no doubt about this incident, as it was witnessed by many well-known archers of the day, who have independently recorded the range, which was carefully measured at the time owing to the interest it aroused.

On this occasion the Turkish Secretary declared that he was not an expert archer, and could not shoot nearly so far as could many of his friends in Turkey !

Personally I have shot an arrow from one of these bows to a distance of 445 yards, or slightly over a quarter of a mile, and frequently from 420 to 430 yards.

Though very strong in the arm and wrist, from constant practice in drawing powerful bows and casting projectiles—I can even now throw a cricket-ball over 80 yards—I cannot, of course, at all compare in strength and skill with the Turkish archer of former days.

Still, what I am able to accomplish plainly shows what far more wonderful feats the trained Turkish bowmen must have been capable of in past times.

I have already referred to the great elasticity of the Oriental bow, and in which lies the secret of its marvellous power. As an example of this elasticity you may unstring one of these bows, even after it has been kept strung for three days, and its ends may be seen to slowly move back into their proper reflex position, the bow finally regaining the exact shape it was in before it was strung several days previously, its future strength being unimpaired. A bow of yew treated in similar fashion would, however, be damaged beyond repair.

It is said that no man can now string one of these short Oriental bows, if it is a very strong one and much reflexed, without the aid of an assistant to place the bow-string in position for him as he bends the bow. This I quite believe. The act is, however, as much a question of knack as of strength, as it is even difficult to string a weak reflex bow unless it is treated very carefully, and the strength of the manipulator is applied in precisely the right direction. How these bows were strung I learnt from a figure sculptured on an ancient Greek vase, on which an archer is shown stringing his reflex bow.

Though confusing accounts of how to do this act are to be found in various old Oriental manuals on archery, the figure on the vase, being an actual illustration from life, was the real origin of my learning the proper method.

Unlike those of European nations, the arrows of the Orientals were very highly finished, their shafts being often lacquered in red and gold, and their metal heads damascened. The short pliable bow-string, made of many lengths of silk, and with a hard noose of sinew at each of its ends, was admirably adapted for strangulation, formerly a common form of execution in Turkey!

I may add, that in warfare one arrow with a wide sharp-bladed head, the shape of a crescent, was usually carried by the archer for the purpose of severing the tendon Achilles of any prisoner who tried to escape. Some of the Oriental arrows were truly horrible, for their small barbed heads were loosely attached to their shafts, so as to disconnect when a stricken soldier tried to extract the missile from his body! The shaft he easily plucked out, but the barbed point remained in the wound, and, being impossible to extract, caused a cruel and lingering death.

The elasticity and unbreakable nature of the Oriental bow allowed the archer to draw its string back to such an extent that the ends of the bow almost appeared to meet! He was thus able to use an arrow in warfare that was long and heavy in comparison to his bow, which was short and light in itself and well adapted to mounted men.

Ulysses was armed with a composite bow that bent in this way and which was made from the horns of a wild goat. Strange to say, recent excavations have proved that bows made of goats' horns were formerly used in Minoan Crete of precisely the same construction as the bow Homer so graphically tells us Ulysses handled when he killed one by one the insolent suitors of his wife. Homer's description of the bow of Pandarus is another excellent reference to a composite bow, for we read, as rendered by the translator:—

“’Twas formed of horn and smoothed with artful toil
A mountain goat resigned the shining spoil
The workmen joined and shaped the bended horns,
And beaten gold each taper point adorns
Now with full force the yielding bow he bends
Drawn to an arch, and joins the doubling ends.”

The Cross-bow.—From the single-bow it is a natural transition to the cross-bow. The cross-bow was undoubtedly used by the ancient Romans, and is alluded to by their military historians as a hand weapon, and, on a much larger scale, for siege purposes.

From the fifth to the tenth century, cross-bows are very seldom mentioned, but from the middle of the tenth century it was a common arm in warfare.

This continued to be the case, even though its employment was forbidden by the second Lateran Council in 1139, under penalty of

an anathema, as being a dishonourable weapon, hateful to God and unfit for Christians. The exception was made, however, that it might be used against Infidels. At the close of the same century, Pope Innocent III. confirmed this prohibition against the cross-bow.

In the twelfth century, Anna Commena, in her history of her father, Alexis I., gives a minute description of the cross-bow of about the time of the first Crusade.

The primitive form of cross-bow had a bow of wood, and hence was comparatively weak in effect. For this reason the cross-bow of early times, and for many years after the Norman Conquest, was in great measure supplanted by the more powerful long-bow, in the use of which European, and especially English archers, were steadily progressing.

But by about the beginning of the fourteenth century, the cross-bow had been improved into quite a different arm to what it was in the days of the Crusades.

It had now a very powerful steel bow, and a stock and lock-work, and an appliance for drawing its bow-string, that were mechanically perfect.

Its bow-string was no longer drawn by hand, as when it had a bow of wood, as described by Anna Commena.

The weapon had at length developed into a very powerful and accurate one, and though considerably slower in its action, it quite equalled the long-bow in effect, and indeed more so when used in the defence of a fortress, as it could be aimed through narrow apertures, and from behind battlements and walls where a bowman could not stand erect, or where he had not space to draw his bow. The cross-bow, a Continental arm, was never popular in England, for our troops adhered to their long-bows, and statutes were passed from time to time forbidding the use of the cross-bow. This was partly from a jealousy, not quite unknown in these days, of foreign innovations, and partly from the fear that the cross-bow might supersede the historic and cherished long-bow, which had won for us fame at such victories as Crecy, Poitiers, and Agincourt.

Though very seldom seen in warfare in England, except in the hands of foreign mercenary soldiers, our ancestors commonly carried cross-bows for killing deer and other wild animals. Cross-bows, specially those for sporting purposes, in opposition to the plain and simple long-bow, gave the artist, the worker in metal and the decorator every chance of showing their skill. Near the close of the fourteenth century, this weapon had become such a costly and important arm, that in Spain cross-bow men were even granted the rank of knights, and on the Continent, generally, they were always placed in the honoured position of the front line in battle.

Though there are thousands of splendidly decorated and constructed mediæval cross-bows to be seen in the Museums and Armouries of Europe, many of them as sound and perfect as the day

they were finished for service, there is not, alas, one genuine contemporary long-bow existent. The long-bow was, of course, of too perishable a material to last through several centuries of neglect, though the cross-bow survived, as wood forms only a small portion of its construction, and that wood being, besides, of the hardest and most enduring kind.

I have fitted up and experimented with many of the large windlass cross-bows, of the latter half of the fifteenth century, and I find that their average range, with an ironshod bolt of 2 oz., is from 350 to 360 yards, or considerably further than any long-bow, with its ounce in weight war-arrow, was able to attain. With a large siege cross-bow, such as the cross-bow man rested on a wall or battlement, and which was too heavy for him to carry in the field, I have obtained a range of 450 yards.

As an example of the immense strength of the steel bows of these weapons when of large size, I may mention that a very powerful cross-bow I own, necessitates a strain, representing a weight of 1100 lb. to draw its string the required 7 inches to the catch of its lock, whilst the strain, or pull, of a powerful long-bow, when its arrow is drawn to the head, is only from 70 to 80 lb. at most.

Many and various were the mechanical means by which the strings of cross-bows were drawn, as, of course, with steel bows manual power was out of the question. The Windlass and the Crenequin, the latter being a form of ratchet, were the two devices that were applied to all the more powerful military and sporting weapons of the kind.

Hand guns commenced to supersede cross-bows about the middle of the fifteenth century. At the end of the first quarter of the sixteenth century, cross-bows were almost unknown in European warfare, though the weapon continued to be popular on the Continent and at home for killing deer till near the end of the first quarter of the seventeenth century.

For instance, in 1621, we read of the Court of twelve bishops who inquired into the death of a keeper, who was slain by Archbishop Abbot of Canterbury with a bolt from his cross-bow, which he had aimed at a stag. This prelate was then on a visit to his friend Lord Zouche, at Bramshill in Hampshire, and, at the time of his misfortune, had been ordered out-door exercise for the benefit of his health. Even later than this date, the cross-bow was employed for shooting deer in England, as there is a very curious tablet in Hunsdon Church, in Hertfordshire, dated 1591, on which is depicted the keeper of the deer park at that place in the act of killing a stag with his cross-bow. The last records of the kind I can find are in Switzerland, where the Swiss and Tyrolese hunters stalked and shot chamois with the cross-bow till about the year 1715. I may add, however, that many Chinese soldiers and hunters are still armed with cross-bows, some of these being of very ingenious construction, for they carry a store of arrows in a magazine attached to the stock, which is so arranged

that it enables the soldier to discharge his missiles automatically and with great rapidity, one after the other, in the manner of a modern repeating rifle. These weapons have, however, no real power, as their bows are of wood, and they are not worthy of further description.

PROJECTILE SIEGE ENGINES.

I will now give a short description of the great projectile engines that were employed for battering fortifications and slaying their defenders, and which were, of course, also used by the besieged as a means of repelling the enemy. These engines are of great antiquity, and are first mentioned in Chronicles, wherein we read "that Uzziah made in Jerusalem engines invented by cunning men, to be on the towers and upon the bulwarks to shoot arrows and great stones." I can find no earlier literary reference to weapons of this kind.

It is possible that the Assyrians first constructed them, as in some of the sculptures from Nimrud a primitive form of engine is represented in the act of throwing large stones, which are plainly indicated as if in transit through the air.

It was not, however, till the reign of Philip of Macedon, and that of his son, Alexander the Great, that the perfecting of projectile engines was carefully attended to, and their value in warfare fully recognised.

We have many detailed and graphic accounts of these engines in sieges. For instance, at the siege of Syracuse, 214 to 212 B.C., that great mechanical and mathematical genius, Archimedes, first constructed and then directed the manipulation of engines to repel the Romans. He discharged stones against the enemy, so Plutarch tells us, of such enormous size, and with such incredible force and velocity, that nothing could withstand them, neither ships nor men.

So effective were the engines employed by Archimedes, that the Romans ignominiously retired to a safe distance, from which they then blockaded the city, to finally capture it by surprise.

Again, Josephus, in his "Wars of the Jews," gives us an excellent account of the effects of siege engines. This author writes that at the siege of Jotapata, A.D. 67, Vespasian positioned his engines to the number of a hundred and sixty round the city, and that they continuously cast into it, by day and by night, stones of the weight of a talent.

A talent is 58 lb.

At the siege of Jerusalem, Josephus informs us that the stones cast by the besiegers were also of the weight of a talent, and were projected for two or more stades, two stades being equal to 404 English yards.

Descriptions of these engines and of the slaughter they caused, as well as destruction to buildings, are to be found in the writings

of numerous military historians and engineers, both Greek and Roman.

The books of Heron, Philo, Athenæus, Colonna, Vitruvius, Ammianus, Diodorus, Procopius, Polybius, Plutarch, and Caesar give many details, and for later times, Froissart, Camden, Holinshed and Père Daniel may be consulted.

Projectile engines were in general use in sieges for both attack and defence until near the last quarter of the 14th century, when cannon superseded them in warfare, though not entirely.

For example, we read in Guillet's *Life of Mahomet the Second*, "that at the siege of Rhodes in 1480, the Turks set up a battery of sixteen great cannon, but that an engineer in the besieged town made an engine for casting stones of such a terrible size that the enemy was prevented from pushing forward his breast-works, his mines were discovered, and his troops were filled with carnage when they came within range."

Later still, in 1521, at the siege of Mexico, Cortes had a huge engine built in order that it might take the place of his cannon, the ammunition for which was exhausted.

This engine was, however, a failure, as the first stone it cast flew straight upwards instead of into the town, and then dropping perpendicularly from a great height it fell on the engine itself and destroyed its mechanism.

The final appearance of any weapon of the kind in warfare was at the siege of Gibraltar by the French and Spanish Fleets, 1779-1782.

On this occasion General George Eliot, afterwards Lord Heathfield, had a catapult constructed for throwing heavy stones over the edge of a precipice, so that they might fall on a ledge of rock occupied by the Spaniards.

The old engines were of three distinct kinds, and though their names have been much confused by historians, one name being often applied to all three, they may be known as the Catapult, the Balista, and the Trebuchet.

The propulsive power of both the catapult and the balista was derived from a very tightly twisted coil, formed of horse-hair rope, or of hide cut into strips, the latter being obtained from the vast number of oxen consumed by the besieging army, or by the besieged as the case might be. Horsehair was perhaps more elastic than hide, but was not available in the same quantity. As to women's hair, we have all read the story of how the brave matrons and maids of Carthage cut off their tresses to supply power to the new engines that the Carthagenians were unexpectedly forced to construct for defence against the Romans, after all their old ones had been surrendered.

The Catapult had a single arm to which a sling for throwing its missile was attached. The primitive manual sling, with a staff and a pocket at its outer end for the stone, probably suggested the action of the arm of the catapult.

The Balista was like a great cross-bow, and differed from the catapult in that it had two arms, each working in its separate coil of twisted hair or hide.

The arms of the balista were connected by a rope, which acted as a bow-string. This rope, or bow-string as it may be termed, was pulled back by a windlass along the stock of the machine, the stock resembling that of a giant cross-bow. The rope or bow-string was secured by a catch, that was freed by a hand-lever which acted as a trigger.

The balista discharged great feathered arrows in the form of javelins, of several pounds in weight.

The Trebuchet was worked by means of an immense weight, and not by a coil of twisted cordage in a high state of tension. This engine consisted of a framework in which was pivoted, at about one-third of its length, a great arm or beam of wood. At the end of the arm nearest to the pivot a huge weight was hinged, and at the other end of the arm a sling was fitted. The weight caused the arm to retain a perpendicular position, the sling being at the upper extremity of the arm. When the engine was prepared for action, the upper end of the arm was pulled down, by from fifty to a hundred men, to a catch in the framework till the sling rested on the ground. The weight at the other extremity of the arm was then naturally lifted upwards and suspended in mid-air.

When the catch which secured the sling end of the arm was released, this end of the arm swung up with great force into its original perpendicular position, owing to the counterpoising weight at its other end falling by gravity.

In this way the missile was projected from the sling as if from a gigantic hand and arm. The trebuchet is said to have been a French invention, and did not appear in Continental warfare till about the twelfth century, though I consider it was known in some form or other by the Arabs long before it was seen in Europe. The use of the catapult and balista having been much neglected by the later Romans, and the art of manufacturing the coils of twisted cordage so necessary to their effectiveness having been almost forgotten, the trebuchet supplanted the two older engines.

One reason in favour of the trebuchet was that it could cast stones of from 300 lb. to 400 lb. in weight, or far heavier than those thrown by the most powerful catapults, its projectiles being thus so heavy that they beat down towers and walls and formed breaches for the besiegers to pass through. The size of the missile cast by a trebuchet was merely governed by the weight of the counterpoise that was the means of propelling it. Trebuchets with counterpoise weights of from 18,000 to 20,000 lb., and more, were used.

It has been calculated that one of these engines, with an arm 50 feet long and a counterpoise of 20,000 lb. could hurl a stone 300 lb. in weight to a distance of 350 yards.

We have, indeed, well authenticated accounts of trebuchets casting

dead soldiers, and even dead horses, into besieged towns as a means of starting a pestilence. Froissart tells us that at the siege of Auberoche an envoy was sent to treat for terms with the besiegers, but that the enemy treated his mission with contempt, seized him, placed him in the sling of an engine, and shot him back again into the town, and to make it more serious his letters were taken from him and hung round his neck. Froissart quaintly adds that the varlet arrived dead before the knights in the town, who were much astonished and discomfited when they saw him return in this dreadful manner.

I should mention that these old siege-engines varied in size. The smallest catapult weighed about a ton and was able to cast a stone of from 8 lb. to 10 lb. to a distance of from 400 to 450 yards.

The large catapult weighed from 4 to 5 tons and more, and was capable, as Josephus and other reliable authorities tell us, of throwing a stone a talent in weight, or 58 lb., to a range of from 400 to 500 yards.

With a model catapult, one of many I have constructed, which weighs a ton and a half, I have projected a stone weighing 7 lb. to a range of 400 yards.

With a balista, a hundred pounds in weight, I have shot heavy arrows weighing half a pound to a range of 350 yards.

The balista, though it shot heavy javelins, was not nearly so cumbersome as the catapult, and was often so portable that, when mounted on wheels, it could be employed in the field of battle as light artillery, or else placed on a parapet or tower for use against besiegers. The trebuchet was, however, always of great size, and, with its towering arm and massive frame-work, weighed very many tons, as was imperative when we consider it cast stones of a size sufficiently heavy to destroy towers and breach walls.

At the evacuation of Damietta in 1629, Louis IX. captured twenty-four trebuchets of such vast dimensions that they afforded timber for stockading his entire camp, and a single engine of this kind, used at the capture of Acre by the Turks in 1291, formed a load for a hundred carts.

The intricate and more valuable parts of these engines, such as the winches and metal fittings, were brought to the vicinity of the town to be attacked with other military stores, and the huge baulks of timber necessary to complete them were usually obtained from trees growing in forests in the district.

As an example of the amount of material that must have been required to form the coil or skein of cordage fitted to a large ancient catapult, a coil perhaps a foot and over in diameter and twelve to fourteen feet in length, I may remark that in my comparatively small model of a ton and a half in weight, the skein within which the arm works consists of just a mile of cord a quarter of an inch thick.

Cord, I may add, is a poor substitute for the hide, sinew, or hair, from which the Greeks and Romans derived the motive force of their catapults and balistas.

[R.P.-G.]

GENERAL MONTHLY MEETING,

Monday, June 1, 1908.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S.,
President, in the Chair.

The Right Hon. The Duke of Devonshire, P.C.

J. Franklin Adams, Esq.

W. Heptinstall Millar, Esq., M.D. M.R.C.S. L.R.C.P.

Lady Martin,

Paul Mavrogordato, Esq.

John Ziffo, Esq.

were elected Members of the Royal Institution.

The Chairman announced that he had nominated the following gentlemen as Vice-Presidents for the ensuing year :—

The Right Hon. The Earl of Halsbury, P.C. M.A. D.C.L. F.R.S.

Donald William Charles Hood, Esq., C.V.O. M.D.

Ludwig Mond, Esq., Ph.D. F.R.S.

The Right Hon. The Earl of Rosse, K.P. D.C.L. LL.D. D.Sc.
F.R.S.

Alexander Siemens, Esq., M.Inst.C.E.

The Right Hon. Sir James Stirling, P.C. M.A. LL.D. F.R.S.

Sir James Crichton-Browne, M.D. LL.D. F.R.S. (Treasurer).

Sir William Crookes, D.Sc. F.R.S. (Hon. Secretary).

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Lords of the Admiralty—Greenwich Photo-Heliographic Results, 1874–1885.
4to. 1907.

Greenwich Astrographic Catalogue, 1900. Vol. II. 4to. 1908.

Observations of Eros, 1900–01. 4to. 1908.

Cape Observatory Annals, Vol. II. Parts 5–6. 4to. 1907.

Catalogue of 1680 Stars, 1900. 4to. 1907.

The Secretary of State for India—Research Work on Indigo. By W. P. Bloxam.
8vo. 1908.

Agricultural Journal of India, Vol. III. Part 1. 8vo. 1908.

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche
e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XVII. 1° Semestre,
Fasc. 8–9. 8vo. 1908.

Allegheny Observatory—Publications, Vol. I. Nos. 3 and 5. 4to. 1908.

American Academy of Arts and Sciences—Proceedings, Vol. XLIII. No. 16.
8vo. 1908.

- American Geographical Society*—Bulletin, Vol. XL. No. 4. 8vo. 1908.
Astronomical Society, Royal—Monthly Notices, Vol. LXVIII. No. 6. 8vo. 1908.
Automobile Club—Journal for May, 1908. 8vo.
Bankers Institute—Journal, Vol. XXIX. Part 6. 8vo. 1908.
Berlin, Royal Prussian Academy of Sciences—Sitzungsberichte, 1908, Nos. 1-23. 8vo.
Boston Public Library—Monthly Bulletin for May, 1908. 8vo.
British Architects, Royal Institute of—Journal, Third Series, Vol. XV. Nos. 13-14. 4to. 1908.
British Astronomical Association—Journal, Vol. XVIII. No. 7. 8vo. 1908.
Buenos Aires—Boletín Mensual, Feb.-May, 1907. 8vo.
Carnegie Institution—Contributions from Mount Wilson Solar Observatory, Nos. 21-25. 8vo. 1908.
 Report of the Director, 1907. 8vo. 1908.
Chemical Industry, Society of—Journal, Vol. XXVII. Nos. 9-10. 8vo. 1908.
Chemical Society—Journal for May, 1908. 8vo.
 Proceedings, Vol. XXIV. Nos. 341-342. 8vo. 1908.
Chicago, John Crerar Library—Thirteenth Annual Report, 1907. 8vo. 1908.
Editors—American Journal of Science for May, 1908. 8vo.
 Analyst for May, 1908. 8vo.
 Astrophysical Journal for May, 1908. 8vo.
 Athenæum for May, 1908. 4to.
 Author for June, 1908. 8vo.
 British Homœopathic Review for June, 1908. 8vo.
 Chemical News for May, 1908. 4to.
 Chemist and Druggist for May, 1908. 8vo.
 Dioptric Review for May, 1908. 8vo.
 Dyer and Calico Printer for May, 1908. 4to.
 Electrical Contractor for May, 1908. 8vo.
 Electrical Engineer for May, 1908. 4to.
 Electrical Engineering for May, 1908. 4to.
 Electrical Industries for May, 1908. 4to.
 Electrical Review for May, 1908. 4to.
 Electrical Times for May, 1908. 4to.
 Electricity for May, 1908. 8vo.
 Engineer for May, 1908. fol.
 Engineering for May, 1908. fol.
 Horological Journal for May, 1908. 8vo.
 Illuminating Engineer for May, 1908. 8vo.
 Journal of the British Dental Association for May, 1908. 8vo.
 Journal of Physical Chemistry for April, 1908. 8vo.
 Law Journal for May, 1908. 8vo.
 London University Gazette for May, 1908. 4to.
 Model Engineer for May, 1908. 8vo.
 Motor Car Journal for May, 1908. 4to.
 Musical Times for May, 1908. 8vo.
 Nature for May, 1908. 4to.
 New Church Magazine for June, 1908. 8vo.
 Nuovo Cimento for April, 1908. 8vo.
 Page's Weekly for May, 1908. 8vo.
 Physical Review for May, 1908. 8vo.
 Revue d'Electrochimie for April, 1908. 8vo.
 Science Abstracts for May, 1908. 8vo.
 Terrestrial Magnetism for March, 1908. 8vo.
 Zoophilist for May, 1908. 4to.
Franklin Institute—Journal, Vol. CLXV. No. 5. 8vo. 1908.
Geological Society—Abstracts of Proceedings, Nos. 862-863. 8vo. 1908.

- Kenealy, Miss A., L.R.C.P. (the Author)*—The Failure of Vivisection and the Future of Medical Research. 8vo. 1908.
- London County Council*—Gazette for May, 1908. 4to.
- Manchester Literary and Philosophical Society*—Memoirs, Vol. LII. Part 2. 8vo. 1908.
- Martius, Dr. C. A. von, M.R.I.*—Die Allgemeine Elektrizitäts-Gesellschaft, 1883–1908. 4to. 1908.
- Das Königliche Materialprüfungsamt der Technischen Hochschule Berlin. 4to. 1904.
- Meteorological Office*—The Observer's Handbook. 8vo. 1908.
- Meteorological Society, Royal*—Journal, Vol. XXXIV. No. 146. 8vo. 1908.
- Record, Vol. XXVII. No. 106. 8vo. 1908.
- List of Fellows, 1908. 8vo.
- Mexico, Secretaria de Comunicaciones*—Anales, Num. 18. 8vo. 1908.
- Mexico, Sociedad Científica "Antonio Alzate"*—Memorias, Tome XXV. No. 2; Tome XXVI. Nos. 1–3. 8vo. 1907.
- Milan, R. Scuola Superiore di Agricoltura*—Annuario, Vol. VII. 8vo. 1908.
- Monaco, L'Institut Océanographique*—Bulletin, Nos. 115–117. 8vo. 1908.
- National Church League*—Church Gazette for May, 1908. 8vo.
- National Physical Laboratory*—Report of the Observatory Department, 1907. 8vo. 1908.
- Navy League*—Navy League Journal for May, 1908. 8vo.
- New Zealand, Registrar-General*—Report on Census, 1906. 8vo. 1908.
- North of England Institute of Mining Engineers*—Transactions, Vol. LVII. No. 7; Vol. LVIII. No. 3. 8vo. 1908.
- Onnes, Prof. Dr. H. K.*—Communications from the Physical Laboratory of Leiden, Supplements Nos. 15–17 to Nos. 97–108. 8vo. 1908.
- Paris, Société d'Encouragement pour l'Industrie Nationale*—Bulletin for April, 1908. 4to.
- Pharmaceutical Society of Great Britain*—Journal for May, 1908. 8vo.
- Photographic Society, Royal*—Journal, Vol. XLVIII. No. 5. 8vo. 1908.
- Physical Society of London*—Proceedings, Vol. XXI. Part 1. 8vo. 1908.
- List of Fellows, 1908. 8vo.
- Planinsky, G. J., Esq.*—Macedonia and the Reforms. By Draganof. 8vo. 1908.
- Quekett Microscopical Club*—Journal, Ser. 2, Vol. X. No. 62, April 1908. 8vo.
- Röntgen Society*—Journal, Vol. IV. No. 16. 8vo. 1908.
- Royal Engineers' Institute*—Journal, Vol. VII. No. 6. 8vo. 1908.
- Royal Society of Arts*—Journal for May, 1908. 8vo.
- Royal Society of Edinburgh*—Proceedings, Vol. XXVIII. Part 4. 8vo. 1908.
- Royal Society of London*—Philosophical Transactions, A, Vol. CCVIII. Nos. 431–432; B, Vol. CXCIX. No. 261. 4to. 1908.
- Proceedings, Vol. LXXX. Series A, Nos. 539–540; Series B, No. 539. 8vo. 1908.
- St. Paulo, Department of Agriculture*—Dados Climatologicos, Serie II. No. 2. 8vo. 1907.
- St. Petersburg, Imperial Academy of Sciences*—Bulletin, VIe. Série, 1908, Nos. 8–9. 4to. 1908.
- Mémoires: Classe-Phys.-Mat., Vol. XVII. No. 7; Vol. XVIII. Nos. 1–6; Vol. XIX.; Vol. XX. Nos. 1–11. 4to. 1905–7.
- Sanitary Institute, Royal*—Journal, Vol. XXIX. No. 5. 8vo. 1908.
- Smithsonian Institution*—Miscellaneous Collections, Vol. LI. No. 1791; Quarterly Issue, Vol. IV. No. 4. 8vo. 1908.
- Annals of the Astrophysical Observatory, Vol. II. 4to. 1908.
- Società degli Spettroscopisti Italiani*—Memorie, Vol. XXXVIII. Disp. 3–4. 4to. 1908.
- Transvaal Department of Agriculture*—Journal for April, 1908. 8vo.
- United Service Institution, Royal*—Journal for May, 1908. 8vo.

United States Department of Agriculture—Experiment Station Record, Vol. XIX. No. 8. 8vo. 1908.

Monthly Weather Review for February, 1908. 4to.

United States Geological Survey—Twenty-eighth Annual Report. 8vo. 1907.

Bulletin, Nos. 309, 316, 319, 321, 322, 325-327, 330, 331, 333, 334, 336, 339. 8vo. 1907-8.

Water Supply Papers, Nos. 207, 209, 210, 213-217. 8vo. 1907-8.

Mineral Resources, 1906. 8vo. 1907.

United States Patent Office—Official Gazette, Vol. CXXXIII. No. 9; Vol. CXXXIV. Nos. 1-3. 8vo. 1908.

Venice, Royal Institute of Science and Art—L'Ateneo Veneto for 1906-1907, 1908, Vol. I. Fasc. 1. 8vo. 1906-8.

Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1908, Heft 5. 4to.

Western Australia, Agent-General—Monthly Statistical Abstract for March, 1908. 4to.

Western Society of Engineers—Journal, Vol. XIII. No. 1. 8vo. 1908.

Zurich, Naturforschenden Gesellschaft—Vierteljahrsschrift, 1907, Heft 3-4. 8vo. 1908.

GENERAL MONTHLY MEETING,

Monday, July 6, 1908.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Lady Low,
Henry Nicholas Middleton, Esq., J.P.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Oswald Lewis, Esq., *M.R.I.*, for his Donation of £5 5s., and to "A Member" for a Donation of £50, to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Honorary Secretary announced the decease of Professor Liebreich on July 1, 1908, and read the following Resolution of Condolence passed by the Managers at their Meeting held this day :—

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the great loss Medical Science and the Institution have sustained in the decease of Geheimrath Professor Oscar Liebreich, D.C.L. LL.D., Officer of the Legion of Honour, an Honorary Member of the Royal Institution, and author of important works on Therapeutics.

Professor Liebreich was Director of the Pharmacological Institute of

Berlin for thirty-five years, and was distinguished by his brilliant researches and observations, which are of world-wide importance, on the use and application of Chloral and Lanolin.

Professor Liebreich was elected an Honorary Member of the Royal Institution on the occasion of the Centenary Celebrations in 1899, and was presented with his Diploma of Honorary Membership by His Royal Highness the Prince of Wales, the Vice-Patron.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their sincere sympathy and heartfelt condolence with Mrs. Liebreich and the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Secretary of State for India—Memoirs: Department of Agriculture, Botanical Series, Vol. II. No. 3; Entomological Series, Vol. I. No. 6, Vol. II. No. 1. 8vo. 1908.

History of Buddhism in India. 8vo. 1908.

Astronomer Royal—Report to the Board of Visitors of the Royal Observatory, 1908. 4to.

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XVII. 1^o Semestre, Fasc. 10-11. 8vo. 1908.

American Geographical Society—Bulletin, Vol. XL. No. 5. 8vo. 1908.

Astronomical Society, Royal—Monthly Notices, Vol. LXVIII. No. 7. 8vo. 1908.

Automobile Club—Journal for June, 1908.

British Architects, Royal Institute of—Journal, Third Series, Vol. XV. Nos. 15-16. 4to. 1908.

British Astronomical Association—Journal, Vol. XVIII. No. 8. 8vo. 1908.

Memoirs, Vol. XVI. Part 1. 8vo. 1908.

Buenos Aires—Monthly Bulletin of Municipal Statistics for March, 1908. 4to.

Cambridge Philosophical Society—Transactions, Vol. XXI. No. 1. 4to. 1908.

Canada, Geological Survey—Annual Report, Vol. XVI., and Index 1885-1906. 8vo. 1906-8.

Summary Report, 1906. 8vo.

Falls of Niagara. 8vo. 1908.

Reports, Nos. 979, 988, 992. 8vo. 1907-8.

Maps of Rossland, B.C., and The Yukon Territory. fol. 1908.

Chemical Industry, Society of—Journal, Vol. XXVII. Nos. 11-12. 8vo. 1908.

Chemical Society—Proceedings, Vol. XXIV. Nos. 343-344. 8vo. 1908.

Journal for June, 1908. 8vo.

Clowes, Professor F., D.Sc. M.R.I. (the Author)—Quantitative Analysis. 8th Edition. 8vo. 1908.

Cracovie, Academy of Sciences—Bulletin, 1908, Classe des Sciences Mathématiques, Nos. 4-5; Classe de Philologie, Nos. 3-4. 8vo.

Editors—Agricultural Economist for June, 1908. 4to.

American Journal of Science for June-July, 1908. 8vo.

Analyst for June, 1908. 8vo.

Astrophysical Journal for June, 1908. 8vo.

Athenæum for June, 1908. 4to.

Author for July, 1908. 8vo.

British Homœopathic Review for July, 1908. 8vo.

Chemical News for June, 1908. 4to.

Chemist and Druggist for June, 1908. 8vo.

Concrete for June, 1908. 8vo.

Dyer and Calico Printer for June, 1908. 4to.

Editors—continued

- Electrical Contractor for June-July, 1908. 8vo.
 Electrical Engineer for June, 1908. 4to.
 Electrical Engineering for June, 1908. 4to.
 Electrical Review for June, 1908. 4to.
 Electrical Times for June, 1908. 4to.
 Electricity for June, 1908. 8vo.
 Engineer for June, 1908. fol.
 Engineer-in-Charge for June, 1908. 4to.
 Engineering for June, 1908. fol.
 Horological Journal for June, 1908. 8vo.
 Illuminating Engineer for June-July, 1908. 8vo.
 Journal of the British Dental Association for June, 1908. 8vo.
 Law Journal for June, 1908. 4to.
 London University Gazette for June, 1908. 4to.
 Model Engineer for June, 1908. 8vo.
 Motor Car Journal for June, 1908. 8vo.
 Musical Times for June, 1908. 8vo.
 Nature for June, 1908. 4to.
 New Church Magazine for July, 1908. 8vo.
 Nuovo Cimento for May, 1908. 8vo.
 Page's Weekly for June, 1908. 8vo.
 Science Abstracts for June, 1908. 8vo.
 Science of Man for April-May, 1908. 8vo.
 Zoophilist for June-July, 1908. 4to.
Electrical Engineers, Institution of—The Kelvin Lecture. By Professor S. P. Thompson. 8vo. 1908.
Journal, Vol. XL. No. 189. 8vo. 1908.
Florence Biblioteca Nazionale—Bulletin for May-June, 1908. 8vo.
Florence, Reale Accademia dei Georgofili—Atti, Quinta Serie, Vol. V. Disp. 1. 8vo. 1908.
Franklin Institute—*Journal*, Vol. CLXV. No. 6. 8vo. 1908.
Geographical Society, Royal—*Journal*, Vol. XXXI. No. 6: Vol. XXXII. No. 1. 8vo. 1908.
Geological Society—Abstracts of Proceedings, No. 864. 8vo. 1908.
Quarterly Journal, Vol. LXIV. No. 2. 8vo. 1908.
Haarlem, Société Hollandaise des Sciences—Œuvres Complètes de Christiaan Huygens, Tome XI. 4to. 1908.
Italian Society for the Advancement of Science—Atti, Prima Reunione, Parma, 1907. 8vo. 1908.
Jefferson Physical Laboratory—Contributions, 1907, Vol. V. 8vo. 1908.
Linnean Society—*Journal*, Botany, Vol. XXXVIII. No. 266. 8vo. 1908.
Lockyer, Sir Norman, K.C.B. F.R.S. (the Author)—On the Observation of the Sun and Stars made in some British Stone Circles. 8vo. 1905-8.
London County Council—Gazette for June, 1908. 4to.
Madrid, Royal Academy of Sciences—Revista, Tomo VI. No. 10. 8vo. 1908.
Manchester, Municipal School of Technology—*Journal*, Vol. I. Part 1. 8vo. 1908.
Massachusetts Institute of Technology—Quarterly, Vol. XXI. No. 1. 8vo. 1908.
Mechanical Engineers, Institution of—Proceedings, 1907, Parts 3-4. 8vo. 1907.
 List of Members, 1908. 8vo.
Metropolitan Asylums Board—Report for the Year 1907. 8vo. 1908.
Mexico, Sociedad Científica "Antonio Alzate"—Memorias y Revista, Tome XXV. No. 3; Tome XXVI. Nos. 4-5. 8vo. 1907.
Microscopical Society, Royal—*Journal*, 1908, Part 3. 8vo.
National Church League—Gazette for June, 1908. 8vo.
Navy League—*Journal* for June, 1908. 8vo.

- New York, Society for Experimental Biology*—Proceedings, Vol. V. No. 4. 8vo. 1908.
- Onnes, Dr. H. K., Hon. Mem. R.I.*—Communications from the Physical Laboratory at the University of Leiden, Nos. 100-102, 104. 8vo. 1908.
- Paris, Société d'Encouragement pour l'Industrie Nationale*—Bulletin for May, 1908. 4to.
- Paris, Société Française de Physique*—Bulletin, 1908, Fasc. 1. 8vo.
- Pennsylvania, University of*—Provost's Report, 1907. 8vo. 1908.
- Pharmaceutical Society of Great Britain*—Journal for June, 1908. 8vo.
- Photographic Society, Royal*—Journal, Vol. XLVIII. No. 6. 8vo. 1908.
- Post Office Electrical Engineers, Institution of*—Journal, Vol. I. No. 2, July, 1908. 8vo.
- Raymond, Professor G. L. (the Author)*—Ballads and other Poems. 8vo. 1908.
- The Aztec God and other Dramas.* 8vo. 1908.
- A Life in Song.* 8vo. 1908.
- Rome, Ministry of Public Works*—Giornale del Genio Civile for March-April, 1908. 8vo.
- Royal Engineers Institute*—Journal, Vol. VIII. No. 1. 8vo. 1908.
- Royal Society of Arts*—Journal for June, 1908. 8vo.
- Royal Society of Edinburgh*—Transactions, Vol. XLV. Part 4; Vol. XLVI. Part 1. 4to. 1908.
- Royal Society of London*—Philosophical Transactions, B, Vol. CC. No. 262. 4to. 1908.
- Proceedings, A, Vol. LXXX. Nos. 541-542; Vol. LXXXI. No. 543; B Vol. LXXX. No. 540.* 8vo. 1908.
- St. Petersburg, Imperial Academy of Sciences*—Bulletin, 1908, Nos. 10-11. 8vo.
- Sanitary Institute, Royal*—Journal, Vol. XXIX. No. 6. 8vo. 1908.
- Saxon Academy of Sciences, Royal*—Berichte, Mat. Phys. 1907, Part 4, 1908, Parts 1-2; Phil. Hist. 1907, Parts 4-5. 8vo. 1907-8.
- Selborne Society*—Nature Notes for June-July, 1908. 8vo.
- Smith, B. Leigh, Esq., M.R.I.*—The Scottish Geographical Magazine, Vol. XXIV. Nos. 6-7. 8vo. 1908.
- Società degli Spettroscopisti Italiani*—Memorie, Vol. XXXVII. Disp. 5. 4to. 1908.
- South Australian School of Mines*—Annual Report, 1907. 8vo. 1908.
- Catalogue of Minerals in the Technological Museum.* 8vo. 1907.
- Standards, Deputy Warden of*—Report by Board of Trade on Proceedings under the Weights and Measures Acts. 4to. 1908.
- Statistical Society, Royal*—Journal, Vol. LXXI. Part 2. 8vo. 1908.
- Swedish Academy of Sciences, Royal*—Arkiv: Botanik, Band VII. Hafte 1-2; Kemi, Band III. Hafte 1; Matematik, Band IV. Hafte 1-2; Zoologie, Band IV. Hafte 1-2. 8vo. 1908.
- Meddelanden, Band I. Nos. 8-11.* 8vo. 1907-8.
- Toronto University*—Studies: Geological Series, No. 5; Psychological Series, Vol. III. No. 1. 8vo. 1908.
- United Service Institution, Royal*—Journal for June, 1908. 8vo.
- United States Department of Agriculture*—Monthly Weather Review for March, 1908. 4to.
- United States Geological Survey*—Bulletin, Nos. 329, 332, 346. 8vo. 1908.
- Water Supply Papers, Nos. 211-212, 218.* 8vo. 1908.
- Professional Paper, No. 56.* 4to. 1907.
- Monographs, Vol. XLIX.* 4to. 1907.
- United States Patent Office*—Gazette, Vol. CXXXIV. Nos. 4-8. 8vo. 1908.
- Upsala, Meteorological Observatory*—Bulletin, Vol. XXXIX. 1907. 4to.
- Vienna, Imperial Geological Institute*—Jahrbuch, Band LVIII. Heft 1, 1908. 8vo.
- Verhandlungen, 1908. Nos. 2-6.* 8vo.
- Washington Philosophical Society*—Bulletin, Vol. XV. pp. 75-101. 8vo. 1908.

Western Australia, Agent-General—Monthly Statistical Abstract, April, 1908. 4to.

Supplement to Government Gazette, April–May, 1908. 4to.

Western Society of Engineers—Journal, Vol. XIII. No. 2. 8vo. 1908.

Wisconsin Academy—Transactions, Vol. XV. Part 2. 8vo. 1907.

Yorkshire Archæological Society—Journal, Part 77, Vol. XX. (Part 1). 8vo. 1908.

Zoological Society of London—Proceedings, 1907, Part 2. 8vo. 1908.

GENERAL MONTHLY MEETING,

Monday, November 2, 1908.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Viscount Ridley,

Andrew Bennie, Esq.

William Hunter, Esq., M.D. F.R.C.P.

were elected Members of the Royal Institution.

The Honorary Secretary announced the decease of the Right Hon. The Earl of Rosse on August 29, of Professor E. Mascart on August 26, and of Professor Henri Becquerel on August 25, and the following Resolutions of Condolence, passed by the Managers at their Meeting held this day, were read :—

Resolved, The Managers of the Royal Institution of Great Britain desire to record their sense of the loss sustained by the Institution in the decease of the Earl of Rosse, K.P. B.A. D.C.L. LL.D. D.Sc. F.R.S., Chancellor of the University of Dublin, and a Manager of the Royal Institution.

The Earl of Rosse was elected a Member of the Royal Institution in 1870, and always took an active interest in the work of the Institution, and frequently attended the Friday Evening Meetings. The Earl of Rosse delivered two Friday Evening Discourses on some of his most important Astronomical Observations made by means of the great Telescope at Birr Castle: in May 1873, on “The Radiation of Heat from the Moon, the Law of its Absorption by our Atmosphere, and its Variation in amount with her Phases,” and again in 1895, on “The Radiant Heat from the Moon during the Progress of an Eclipse.”

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with Countess Rosse and the family in their bereavement.

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their deep sense of the loss the scientific world has sustained

in the decease of Professor E. Mascart, D.Sc. Hon.F.R.S., Membre de l'Institut, Grand Officier de la Légion d'honneur, one of the most distinguished Honorary Members of the Royal Institution.

In May 1884, Professor Mascart delivered a Friday Evening Discourse on "Colours," and on the occasion of the Faraday Centenary in 1891 he was elected an Honorary Member of the Royal Institution, and attended on that occasion to have the honour conferred upon him.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with Madame Mascart and the family in their bereavement.

Resolved, The Managers of the Royal Institution of Great Britain desire to record their sense of the loss the scientific world has sustained in the decease of Professor Henri Becquerel, D.C.L. LL.D. Ph.D. Sc.D. Hon.F.R.S. Hon.F.C.S., Membre de l'Institut, Secrétaire perpétuel de l'Académie des Sciences, Paris, Officier de la Légion d'honneur, and one of the most distinguished Honorary Members of the Royal Institution.

Professor Becquerel was elected an Honorary Member of the Royal Institution on the occasion of the Centenary Celebrations in 1899. In March 1902, he delivered a Friday Evening Discourse on "La Radio-activité de la Matière," describing investigations on his great discovery of the Becquerel Rays.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with Madame Becquerel and the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

The Secretary of State for India—Memoirs of Department of Agriculture: Entomological Series, Vol. II. Nos. 2-6. 8vo. 1908.

Agricultural Journal of India, Vol. III. Parts 2-3. 8vo. 1908.

Geological Survey: Records, Vol. XXXVI. Part 3. 8vo. 1908.

Report of Superintendent, Archaeological Survey, Burma. 4to. 1908.

Geography and Geology of the Himalaya Mountains and Tibet. By S. G. Burrard and H. H. Hayden. Parts 1-3. 4to. 1907.

Varieties of Wheat Grown in Central Provinces and Berar. By G. Evans. 8vo. 1908.

Progress of Education in Bengal, 1902-3, 1906-7. Supplement, 1902-7. 4to. 1907-8.

Report on Madras Government Museum and Connemara Public Library, 1907-8. 4to.

Kodaikanal Observatory Bulletin, No. XIII. 4to. 1908.

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- List of Fellows. 8vo. 1908.

GENERAL MONTHLY MEETING,

Monday, December 7, 1908.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Walter Spencer Anderson Griffith, M.D. F.R.C.P, F.R.C.S. was elected a Member of the Royal Institution.

His Serene Highness Albert, Prince of Monaco, was elected an Honorary Member of the Royal Institution.

The Special Thanks of the Members were returned to Ludwig Mond, Esq., Ph.D. D.Sc. F.R.S. *M.R.I.*, for his Donation of £500 to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Honorary Secretary reported, That the Managers had, at their Meeting held this day, elected Frederick Walker Mott, M.D. F.R.S., Fullerian Professor of Physiology for three years (the Appointment dating from January 13, 1909).

The following Lecture Arrangements were announced :—

PROFESSOR WILLIAM STIRLING, M.D. LL.D. D.Sc. Six Lectures (adapted to a Juvenile Auditory) on THE WHEEL OF LIFE. On Dec. 29 (*Tuesday*), Dec. 31, 1908, Jan. 2, 5, 7, 9, 1909.

PROFESSOR KARL PEARSON, M.A. F.R.S. Two Lectures on ALBINISM IN MAN. On *Tuesdays*, Jan. 19, 26.

PROFESSOR A. A. MACDONELL, M.A. Ph.D. F.B.A. Three Lectures on THE ARCHITECTURAL AND SCULPTURAL ANTIQUITIES OF INDIA. On *Tuesdays*, Feb. 2, 9, 16.

PROFESSOR FREDERICK WALKER MOTT, M.D. F.R.S. F.R.C.P., Fullerian Professor of Physiology, *R.I.* Six Lectures on THE EVOLUTION OF THE BRAIN AS AN ORGAN OF MIND. On *Tuesdays*, Feb. 23, March 2, 9, 16, 23, 30.

PROFESSOR JOHN OLIVER ARNOLD, D.Met. Two Lectures on MYSTERIES OF METALS. On *Thursdays*, Jan. 21, 28.

WILLIAM ARCHER, Esq., M.A. Two Lectures on THE REVIVAL OF MODERN DRAMA. On *Thursdays*, Feb. 4, 11.

HANS GADOW, Esq., Ph.D. M.A. F.R.S. Three Lectures on PROBLEMS OF GEOGRAPHICAL DISTRIBUTION IN MEXICO. On *Thursdays*, Feb. 18, 25, March 4.

A. D. HALL, Esq., M.A. Two Lectures on RECENT ADVANCES IN AGRICULTURAL SCIENCE. On *Thursdays*, March 11, 18.

PROFESSOR G. H. BRYAN, Sc.D. F.R.S. Two Lectures on AERIAL FLIGHT IN THEORY AND PRACTICE. On *Thursdays*, March 25, April 1.

PROFESSOR SIR HUBERT VON HERKOMER, C.V.O. D.C.L. LL.D. R.A. Two Lectures on 1. THE CRITICAL FACULTY; 2. SIGHT AND SEEING. On *Saturdays*, Jan. 23, 30.

SIR ALEXANDER C. MACKENZIE, Mus.Doc. D.C.L. LL.D. *M.R.I.* Three Lectures on 1. MENDELSSOHN; 2, 3. CHAMBER MUSIC. (1. With Musical

Illustrations; 2, 3. With the kind assistance of the Members of the Hans Wessely Quartette.) On *Saturdays*, Feb. 6, 13, 20.

PROFESSOR SIR J. J. THOMSON, M.A. LL.D. D.Sc. F.R.S. M.R.I., Professor of Natural Philosophy, R.I. Six Lectures on PROPERTIES OF MATTER. On *Saturdays*, Feb. 27, March 6, 13, 20, 27, April 3.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Secretary of State for India—Geological Survey Records, Vol. XXXVI. No. 4 ; Vol. XXXVII. No. 1. 8vo. 1908.

Commercial Products of India. By Sir George Watt. 8vo. 1908.

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Mémoires: Collection in 8vo, 2^e Série, Tome II. Fasc. 3. 1908.

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British Architects, Royal Institute of—Journal, Third Series, Vol. XVI. Nos. 1-3. 4to. 1908.

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Cambridge Philosophical Society—Transactions, Vol. XXI. Nos. 5-6. 4to. 1908. Proceedings, Vol. XIV. Part 6. 8vo. 1908.

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List of Fellows, 1908.*† 8vo.

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- Cracovie, Academy of Sciences*—Bulletin, 1908, Classe des Sciences Mathématiques, Nos. 7-8; Classe de Philologie, No. 5. 8vo.
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- Devonshire Wills*, Part IX. 8vo. 1908.
- Editors*—Agricultural Economist for Nov. 1908. 4to.
- American Journal of Science* for Nov. 1908. 8vo.
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HODGKINS TRUST.

ESSAY BY PROFESSOR HENRY E. ARMSTRONG.

Low-Temperature Research at the Royal Institution, 1900-1907.

IN the account given, in 1901, by Miss Agnes Clerke, of Low-Temperature Research at the Royal Institution during the years 1893-1900,* the achievements chronicled included the solidification of oxygen, the liquefaction of fluorine and the liquefaction and solidification of hydrogen; the only gas remaining uncoerced was helium.

During the period 1900-1907, of which the present account is in a measure a record, siege has constantly been laid to the formidable entrenchments behind which this gas was established; the few who have been privileged to follow the work are aware that operations have been carried on with all the ingenuity and tenacity of purpose which have characterised previous attacks on the gaseous state made in the laboratory of the Institution. But the difficulties met with have been many and great; moreover, progress has been much hampered by Sir James Dewar's continued ill-health and—the confession is a sad one to make—of late especially by lack of funds. Another circumstance has delayed the prosecution of the attempt to degrade helium from its virtuous position as gas: genius has been defined as the capacity for taking pains, and doubtless one of the qualities of genius is perseverance in pursuit of an object, when intuition does not carry it straight to the mark; a more distinguishing feature is the artistic longing for novelty of effect and its manifestation in new works: it is a striking fact that elsewhere liquefied gases have been utilised practically only as mere cooling agents; at the Royal Institution an extraordinary variety of new applications have been made of the intense frigorific effects there available: the aim constantly in view has been not merely to improve the appliances but to develop the use of liquefied gases to the solution of the manifold problems involved in the study of the properties of matter at excessively low temperatures. The period under review has been almost more productive than any previous period from this point of view, one contribution of superlative importance having been made which has both revolutionised practice in the region of low vacua and extended to an altogether remarkable extent our power of dealing with gases and discriminating between

* A summary of the work carried on with the aid of the Hodgkins Trust is, by the authority of the Managers, incorporated in the Proceedings of the Royal Institution every seven years.



SIR JAMES DEWAR

PORTRAIT TAKEN IN THE LABORATORY IN THE SUMMER OF 1902 BY DR. A. SCOTT

them. The first septenate may be termed *the Hydrogen period**; the second *the Charcoal Vacuum period*. The discovery of the marvellous power of charcoals to absorb gases at low temperatures will render this latter period ever memorable.

The reduction of helium from the gaseous state became practicable only when this discovery was made, but to utilise such a new appliance fully was not easy; naturally others have entered the field meanwhile and success has fallen to those who have completest command of the necessary appliances and the ample funds, as well as the skilled assistance, required for the rapid prosecution of such work.

THE USE OF CHARCOAL IN THE PRODUCTION OF HIGH VACUA.

Charcoal, it has long been known, even under ordinary conditions, has the power of absorbing gases and vapours, often in considerable proportions; it is noteworthy also that it is commonly used in depriving liquids of colour—on a very large scale, for example, in purifying sugar. The desire to explain its absorptive power has given rise to much experimental inquiry; indeed, of late years, the property which charcoal possesses, in common with many other materials in a fine state of division, including soils, of withdrawing substances from solution has been the subject of many discussions.

In sketching the history of the subject in 1905, Sir James Dewar pointed out that speculations on the porosity of matter date back several centuries. He drew attention to the following significant passage in a discourse published in 1684 by Boyle and entitled “Experiments and Considerations about the Porosity of Bodies” :—

“When I consider how much most of the qualities of bodies and consequently their operations depend upon the structure of their minute and singly invisible particles and that to this latent contexture, the bigness, the figure and the collocation of the intervals and pores do necessarily concur with the size, shape and disposition or contrivance of the substantial parts, I cannot but think the doctrine of the small pores of bodies of no small importance to Natural Philosophy.”

Modern inquiry has thoroughly justified Boyle’s acute surmise that “the qualities of bodies and consequently their operations” are functions of their latent contexture,† organic chemistry being one

* It should not be forgotten that the Dewar vacuum vessel came into use during this period and was proved to be indispensable to the successful prosecution of inquiry at low temperatures; that period may therefore well be ranked also as the vacuum vessel period.

† Contexture—“the disposition and union of the constituent parts of a thing with respect to each other: constitution”; as an apposite expression of internal structure, the term may be preferred to configuration, which is now commonly used.

continuous proof of the accuracy of his contention : achievements such as the production of the natural colouring matters (the madder colours, indigo, etc.) by artificial means are entirely the outcome of the command secured by chemists through their studies of the internal structure of molecules and of function as determined by structure. Yet it must be confessed that the doctrine of the small pores of bodies is but beginning to attract the share of attention which is due to its importance, especially in relation to the properties of soils.

Priestley, the pioneer investigator of different kinds of air, in the third volume of his "Experiments and Observations relating to various branches of Natural Philosophy," published in 1786, refers to "the property that charcoal has of absorbing air" as "a remarkable circumstance first observed by the Abbé Fontana, physicist to Duke Ferdinand II. of Tuscany"; and he also speaks of having repeated the Abbé Fontana's experiments by introducing hot charcoal through mercury into vessels containing different kinds of air. Priestley, indeed, seems to have been made acquainted with the power that heated charcoal has of absorbing gases by Fontana himself in 1770.

That the subject attracted attention at about the beginning of last century is clear from a statement made by Dalton in his "New System of Chemical Philosophy" (1810, part ii. p. 235) :—

"Several authors have maintained that charcoal after being heated red has the property of absorbing most species of elastic fluids in such quantities as to exceed its bulk several times; by which we are to understand a chemical union of the elastic fluids with the charcoal. The results of their experiments on this head are so vague and contradictory as to leave little credit even to the fact of any such absorption. I made 1500 grains of charcoal red hot, then pulverised it and put it into a Florence flask with a stopcock; to this a bladder filled with carbonic acid was connected; this experiment was continued for a week and occasionally examined by weighing the flask and its contents. At first there appeared an increase of 6 or 7 grains from the acid mingling with the common air in the flask of less specific gravity; but the succeeding increase was not more than 6 grains and arose from the moisture which permeated the bladder: for the bladder continued as distended as at first and finally upon examination was found to contain nothing but atmospheric air. Yet carbonic acid is stated to be the most absorbed by charcoal. One of the authors above alluded to asserts that the heat of boiling water is sufficient to expel the greater part of the gases so absorbed. Now this is certainly not true, as Allen and Pepys have shown; and most practical chemists know that no air is to be obtained from moist charcoal below red heat. Hence the weight acquired by fresh made charcoal is in all probability to be wholly ascribed to the moisture which it absorbs from the atmosphere; and it is to the decomposition of this water and the union of its elements with charcoal that we obtain such an abundance of gases by the application of a red heat."

In view of the crudeness of Dalton's methods, it is perhaps not

surprising that he failed even to confirm the observations of earlier workers.

The subject was first dealt with in a thorough manner by Theodore de Saussure in 1812. An English translation of his important memoirs is to be found in the "Annals of Philosophy," vol. vi. pp. 241-255, 331-347. In his experiments red-hot box-wood charcoal was plunged under mercury and introduced into the gas to be absorbed after it was cool and without ever coming into contact with atmospheric air. He gives the following table:—

Charcoal of Box-wood Absorbs, of	Volumes.
Ammoniacal gas	90
Muriatic acid	85
Sulphurous acid	65
Sulphureted hydrogen	55
Nitrous oxide	40
Carbonic acid	35
Olefiant gas	35
Carbonic oxide	9.42
Oxygen	9.25
Azote	7.5
Oxy-carbureted hydrogen	5
Hydrogen	1.75

De Saussure showed that the property of absorbing gases was common to all porous solids but found no substance possessing the property in so high a degree as charcoal. He arrived at interesting results with mixtures of gases and proved the incorrectness of the statement made by Rouppe and Norden that water was formed by the interaction of hydrogen and oxygen when these two gases were absorbed by charcoal. The volume of gas expelled from charcoal by another gas was found to vary according to the proportion in which both gases exist in the unabsorbed residue. In several cases, the presence of one gas in the charcoal was found to facilitate the condensation of the other.

An interesting series of communications was made to the Chemical Society during the "sixties" by John Hunter. Besides confirming De Saussure's observations, he described the results of measurements of the absorption by charcoal at temperatures extending up to 200° C. not only of gases but also a large number of vapours. Hunter showed that logwood, ebony and cocoanut charcoal exceed that from boxwood in absorptive power, the very dense variety prepared from cocoanut being superior to all other forms.

Charcoal has been used in respirators as a protection against infection; it has been enclosed in covers placed over the openings of sewers to prevent the escape of noxious emanations into the streets; its value as a means of purifying drinking water has long been recognised; but no scientific application was made of its extraordinary power of condensing and holding gases until recently:

Professors Tait and Dewar, in 1874-5, were the first to recognise and take advantage of the property it has of retaining them even under very low pressures. A brief account of their work was published, under the title "On a New Method of obtaining very Perfect Vacua," in the 'Proceedings of the Royal Society of Edinburgh' (1875, viii. 348, 628) and also in 'Nature' (1875, xii. 217). The method consisted in heating charcoal to redness in a tube attached to a mercury pump while the exhaustion was proceeding and to seal the vessel when this was completed. When the charcoal was cold the vacuum was found to be so complete that, even when a powerful coil was used, no spark would pass between platinum wires sealed into the tube only one-fourth of an inch apart. The account referred to is of special interest also as containing a noteworthy contribution to the theory of that most wonderful of instruments the *Radiometer*, the discovery of which by Sir William Crookes has been the point of departure for all modern research on the phenomena of high vacua.

In closing their communication to the Royal Society of Edinburgh, Professors Tait and Dewar remark :—

"We need hardly say that this easy means of obtaining high vacua will be of importance in spectroscopic observations and we intend shortly to communicate observations in this direction."

Nevertheless the method remained undeveloped until 1904 and was then resuscitated almost by accident—which is but another illustration of the manner in which the obvious is neglected until, for some reason, it compels attention. Sir James Dewar had made experiments with finely divided platinum and palladium, which are known to occlude gases, in particular hydrogen and oxygen, even at ordinary temperatures and to take up larger quantities when heated. Finding that their absorptive power was but little affected by cooling, he bethought himself of charcoal and proceeded to contrast its behaviour with that of the metals before mentioned. The momentous discovery was then made that when cooled by liquid air charcoal has an altogether extraordinary power of condensing gases.

Those who have attended the lectures at the Royal Institution during the period under consideration are aware that this has been demonstrated in the most striking manner possible and in a great variety of ways.

All charcoals possess the property. The light variety used in making gunpowder or that prepared by carbonising blood both act; that made from cocoanut, however, is the most effective variety for general use.

The difference in the behaviour of various gases with cocoanut charcoal at 0° and at -185° is well shown in the following table, in which the results are recorded of a series of experiments made with one and the same portion of charcoal, the volume absorbed being

that of the gas measured at 0° and under a pressure of 760 mm. of mercury :—

	I. Volume absorbed at 0° C.	II. Volume absorbed at -185°	III. Ratio of I. to II.
Helium	2 c.c.	15 c.c.	7.5
Hydrogen	4	135	34.0
Electrolytic gas	12	150	12.5
Argon	12	175	14.6
Nitrogen	15	155	10.3
Oxygen	18	230	12.8
Carbonic oxide	21	190	9.0
Carbonic oxide and oxygen	30	195	6.5

These observations were made at an early stage of the inquiry. Afterwards it was found that the quality of the charcoal depends much on the way in which it is prepared and that the absorptive power is enhanced by carbonising the cocoanut shell slowly at a gradually increasing temperature; whereas the specimens at first used absorbed only about 150 c.c. of air per gramme at -185° , those prepared subsequently with these precautions absorbed from 350 to 400 c.c. per gramme. Porous materials other than charcoal, such as alumina, meerschaum and silica, also absorb an increased proportion of gases at low temperatures but their retentive power is much inferior to that of charcoal, which clearly has a special power.

Pressure has but relatively little influence in increasing the amount of gas absorbed. Thus in one case it was observed that 6.7 grammes of a particular specimen of charcoal would not absorb more than 1 litre of hydrogen, even when the pressure was raised from 10 to 25 atmospheres, the amount taken up having reached a limit at the lower pressure and being then less than double what it was at ordinary atmospheric pressure.

Absorption of a Mixture of Gases by Charcoal.—One of the most interesting series of observations made is that relating to the equilibria established on saturating charcoal at low temperatures with a mixture of gases. Charcoal which has been heated, exhausted and then allowed to absorb ordinary air at -185° presumably contains within its pores a mixture approximately of the composition represented by Fig. 1.

If, at the same low temperature, a stream of air be passed slowly and continuously over the charcoal, at first almost pure nitrogen escapes, showing that the system has a preference for oxygen; after several hours, however, the occluded gas has a new and apparently definite composition. On displacing the whole of the gas from the charcoal, a mixture is obtained containing on the average about 60 per cent. of oxygen, corresponding nearly to Fig. 2.

If the charcoal saturated with such a mixture be subjected in a similar manner to the action of a slow current of hydrogen, about

one out of every five of the molecules of oxygen and nitrogen ($3 + 2$) is displaced by hydrogen, giving rise to the system represented by Fig. 3. When charcoal saturated with oxygen is exposed in hydrogen, the composition of the mixture finally occluded is about that represented by Fig. 4; whilst if it act on charcoal saturated with nitrogen it gives rise to the system represented in Fig. 5. Lastly, if air be passed over charcoal saturated with

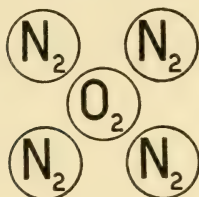


FIG. 1.

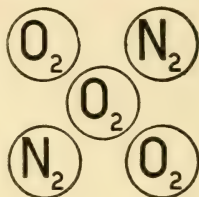


FIG. 2.

hydrogen, the whole of the latter is ultimately displaced, the occluded gas being of the composition represented by Fig. 2.

It is clear from these observations that the more volatile or less condensable the gas, the less it is absorbed and retained in the charcoal. Charcoal may therefore be made use of most effectively in separating the constituents of a mixture of gases of different degrees of volatility.

In the perfectly gaseous state, at and above the critical temperature, the molecules of a substance are free from mutual control. In the liquid state the molecules are in a state of what may be termed

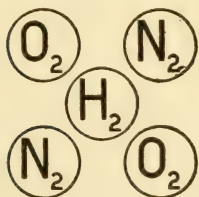


FIG. 3.

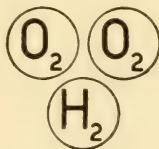


FIG. 4.

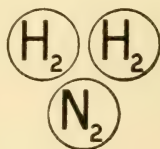


FIG. 5.

shifting association, their vibratory activity being so much reduced that they are able to cling together during an appreciable interval of time in virtue of the surplus or residual affinity with which they are endowed. In the passage from the one state into the other, an increasing proportion passes from the condition of freedom into that of limited association or *vice versa*, according as the temperature falls or rises. A liquid, therefore, always has a certain attraction for the gaseous molecules of its own kind with which it is surrounded; the passage from the one state into the other may be regarded not

as a mere physical change but as a chemical change, inasmuch as it involves a change in the number of kinetic units, in other words, in molecular composition, such as is exemplified by the expression $xM \rightleftharpoons M^x$.

In a solid, the association of the molecules which are the units in the gas is of a rigid character; so soon as they oscillate outside certain mean positions, the mass ceases to be solid and passes into either the viscous fluid or the liquid state.

When charcoal is brought into contact with a gas, presumably its surfaces become coated initially with a layer of molecules which may be regarded as practically solidified. If the temperature be low enough, this surface layer of molecules attracts other molecules, thus forming the equivalent of a liquid layer. The office of the charcoal may therefore be supposed to be that of acting, in virtue of its rigidity and its affinity, as an anchorage, as it were, for the molecules which are presented to it in the gaseous state; once anchored, these molecules can exercise their blandishments over their own kind in a manner which is impossible so long as they are flying about violently in every possible direction.

Density of Gases Absorbed in Charcoal.—As Sir James Dewar points out, the surface presented by charcoal is enormous. According to Mitcherlich, the cells of charred wood are on an average $\frac{1}{2400}$ of an inch in diameter. A cubic inch of charcoal cut up into small equal cubes having edges $\frac{1}{2400}$ of an inch long would offer a surface area of 100 square feet; taking into account, however, the space occupied by the charcoal itself, the area would be about 73 square feet. If the amount of carbon dioxide absorbed by such an amount of charcoal under ordinary conditions were spread as a liquid over such an area, the thickness of the layer would be about 0.000002 of an inch.

Taking into account the real and apparent density of charcoal such as has been used, the average pore space in 100 grammes may be taken as being about 15 cubic centimetres. Calculating with the aid of this value the average density of the material condensed within the charcoal and contrasting it with that of the liquid obtained on condensing the gas alone, it seems that the density of the absorbed material is equal if not a little superior to that of the same substance in the liquid state at the same temperature. The results are given in the table opposite.

It will be noted that the density of the occluded hydrogen at 80° absolute is practically that of liquefied hydrogen; as this temperature is about four times that at which hydrogen boils, the density of the occluded helium at 15° absolute being 0.17, it may be inferred that this would be about the density of liquid helium at its boiling point, about 4° absolute. Actually, Prof. Onnes has found the density of liquid helium at about 4°C.5 to be 0.15.

Gas.	Temperature of Absorption.		Density of Gas occluded in Charcoal.	Density of liquefied Gas.
	Cent.	Abs.		
Carbon dioxide .	+ 15°	288°	0·7	0·8
Oxygen . . .	— 183	90	1·33	1·12
Nitrogen . . .	— 183	80	1·00	0·84
Electrolytic gas	0·58	?
Hydrogen . . .	— 193	80	0·06	0·07
Hydrogen . . .	— 210	63	0·08	..
Hydrogen . . .	— 252	21	0·11	..
Helium . . .	— 258	15	0·17	0·15

PERFECTION OF VACUA OBTAINED BY MEANS OF HIGHLY-COOLED CHARCOAL.—SEPARATION OF GASES.

The perfection of the vacuum obtained by means of charcoal at low temperatures is readily demonstrated with the aid of a sparking tube C A B, such as is shown in the left-hand side of Fig. 6, C being a bulb containing cocoanut charcoal. When such a tube of 1300 c.c. capacity, sealed to a bulb containing 30 grammes of charcoal, was filled with air at atmospheric pressure and then cooled by immersing the charcoal bulb in liquid air, the pressure fell to 50 mm. of mercury : when, however, the tube was filled at the pres-

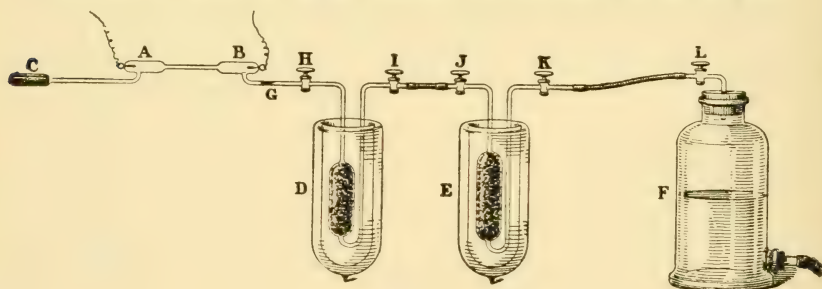


FIG. 6.

sure of only half an atmosphere, on cooling the charcoal to -185° , the exhaustion reached beyond the striae stage ; and no spark would pass when the initial charge was only at the pressure of a quarter of an atmosphere. Using a tube containing only 1 gramme of charcoal and charging it with air at a pressure of 3 mm. of mercury, the vacuum produced on cooling the charcoal just reached the beginning of the phosphorescent stage. Starting with an air pressure of between 1 and 2 mm. a volume of 300 c.c. connected with a tube containing 5 grammes of charcoal cooled in liquid air can reach a vacuum of 0·00005 mm. in one hour.

When the tube was charged with hydrogen, to raise the vacuum

to the striæ stage it was necessary either to use a larger amount of charcoal or to reduce the pressure of the gas to less than an atmosphere; but on lowering the temperature of the air bulb to -210° , by boiling off the air, the exhaustion in the tube just reached the beginning of phosphorescence round the cathodes. Helium was absorbed to but a very slight extent; neon more readily.

In tubes charged with air, it is easy to observe the gradual disappearance of the spectra of oxygen, nitrogen, etc., in the order of the volatility as the exhaustion proceeds; the F line of hydrogen and the yellow line of neon are always noticeable, so that the test for the latter gas is a very delicate one, as the amount of neon in the air cannot well exceed $\frac{1}{70000}$.

To obtain a satisfactory spectrum of helium, it is necessary to enrich the air in the sparking tube. Fig. 6 shows the apparatus used for the purpose. In an experiment in which 200 c.c. of air were supplied to the tube D containing 15 grammes of charcoal cooled by liquid air, on passing the residue on to the sparking tube and examining it spectroscopically, the lines seen were the C and F lines of hydrogen, the yellow and some of the orange lines of neon and also the yellow and green lines of helium. On using the residuary gas from a litre of air, all the helium lines were seen, as well as the yellow neon and the F line of hydrogen—from which it may be inferred that $\frac{1}{360000}$ part of helium by volume may be detected.

As 40 to 50 grammes of charcoal at the temperature of liquid air can absorb from 5 to 6 litres of air, it is easy to accumulate the more volatile gases for spectroscopic examination by using the two condensers E and D shown in Fig. 6. As soon as the charcoal in E is nearly saturated and the less condensable gas has been transferred to D by closing the cock K and opening J and I, the condenser E is removed and rapidly raised to the air temperature, so as to expel the condensed gas; it is then replaced in the circuit and used as before. 50 litres of air can be treated in this manner within a short time; sparking tubes charged with the residuary gas show brilliant spectra of all the more volatile constituents of air.

A variety of interesting demonstrations of a similar kind illustrating the differential condensation of gases by charcoal have been given by Sir James Dewar, such as the separation of the gaseous products from minerals and radio-active bodies, the gases dissolved in rain, well and river waters, together with samples taken from the ocean. Speaking generally, the lower the boiling-point, that is to say the less condensable it is, the less a gas is absorbed.

Krypton and xenon are readily separated from air either by passing a current of air (purified by cooling it with liquid air) over charcoal cooled to -183° or by covering a few hundred grammes of charcoal with old liquid air and allowing this to evaporate in a silvered vacuum vessel, then allowing a further quantity of the retained air to escape from the charcoal at the temperature of solid carbon dioxide, finally extracting the residual gas from the charcoal

and purifying it from carbon compounds and oxygen. The remaining mixture of nitrogen with krypton and xenon is separated into its constituents by condensation and fractionation.

No more effective demonstration of the extraordinary absorptive power of charcoal could well be imagined than that given at the Friday evening lecture in June 1908, when liquid hydrogen (surrounded by liquid air) was solidified in the course of very few minutes by the cold produced by its own evaporation, this being brought about by means of charcoal cooled by liquid air. The arrangement was that of a cryophorus in which the one bulb contained liquid hydrogen the other charcoal.

Evolution of Heat on Absorption of Gases by Charcoal.—When gases are condensed by charcoal the amount of heat liberated is considerably in excess of that which necessarily attends the passage from the gaseous into the liquid state. The first values deduced by Sir James Dewar are as given below, those in the middle column of figures being the amounts of heat (in gramme calories) liberated by the mere liquefaction of gramme molecular proportions of the gases, those in the last the amounts liberated when the same quantities of gas are condensed by charcoal at the temperature of liquid air:—

	Molecular Heat of Liquefaction.	Molecular Heat of Absorption.
	Calories.	Calories.
Hydrogen	238	1600
Nitrogen	1372	3684
Oxygen	1664	3741
Argon	3636
Carbonic oxide	3416
Carbonic oxide and oxygen (equal volumes)	3960
Electrolytic gas	2414

The surprising fact brought out by these figures is that hydrogen, although the least condensable gas, is that which has the greatest affinity for charcoal. The behaviour of helium, however, is even more remarkable. When hydrogen is absorbed at -185° , it is at a temperature about four and a half times its boiling-point (20° abs.); but helium is being absorbed at a temperature between fifteen and twenty times its boiling-point (5° abs.). To make a fair comparison, hydrogen should be taken at a temperature at least fifteen times its boiling-point, so that the absorption of helium at -185° C. should be contrasted with that of hydrogen at 0° C. It was therefore to be inferred that at 15° to 20° absolute helium would be condensed to a more remarkable extent than hydrogen at -185° ; the following figures showing the number of volumes of the two gases condensed by the charcoal are proof of the accuracy of this conclusion:—

Temperature.	Helium.	Hydrogen.
-185° C. (boiling-point of liquid air)	$2\frac{1}{2}$	137
-210° C. (liquid air under exhaustion)	5	180
-252° C. (boiling-point of liquid hydrogen)	160	258
-258° C. (solid hydrogen)	195	..

As the relation between the volume absorbed and the temperature is nearly lineal in the later stages of the condensation of hydrogen and helium, it may be inferred that at a temperature of from 5° to 6° absolute helium would be as freely absorbed by charcoal as hydrogen is at its boiling point, so that, in all probability, the boiling point of helium is not below 5° . That such an inference is legitimate cannot be denied in view of the fact that at the boiling points of liquid hydrogen, nitrogen and oxygen respectively, good charcoal absorbs (at atmospheric pressure) equal volumes of each of these gases, namely 260 c.c. per gramme. It is to be noted that the rate

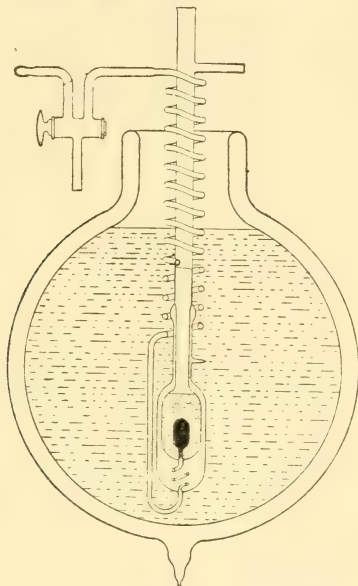


FIG. 7.

at which helium is absorbed increases far more rapidly than does that of hydrogen, a degree or two making a great difference in the volume absorbed. Judging from such results therefore, it was highly probable that the boiling point of helium is about one-fourth that of hydrogen just as that of hydrogen is about one-fourth that of nitrogen.

The heat values given above are those deduced from an early series of experiments. Later, more refined determinations made with the apparatus depicted in Fig. 7 (which is a modification of the liquid air calorimeter suitable for this investigation) show that at 18° absolute the molecular latent heat of absorption of helium in charcoal, 483 calories, is nearly equal to the molecular heat of hydrogen absorption

in charcoal, 524 calories at 20° absolute. As at 78° abs. the molecular latent heat of absorption of hydrogen in charcoal is 2005 calories, a value about four times as great as that determined at about one-fourth the temperature, it may be inferred that the molecular latent heat of absorption of helium at its boiling-point, supposing this to be about 5° absolute, would be about one-fourth the value observed at 18° , viz. $\frac{483}{4} = 120$ calories. As the amount of heat given out in the liquefaction of a gramme molecular proportion of hydrogen at its boiling-point is about half as great as that given out when it is absorbed by charcoal at the same temperature, by analogy it may be inferred that the molecular heat of liquefaction of helium at its boiling-point would be about $\frac{120}{2} = 60$ calories. Knowing the latent heat, the boiling-point, and the fluid density from the helium charcoal absorption experiments, assuming by analogy a similar behaviour to that of other gaseous elements, all the data are available to calculate the vapour pressures of liquid helium.*

As bearing on the properties of the hydrogen and helium molecules, these results are of extraordinary significance. It is clear that when cooled to a temperature at which their motility is so much reduced that they are no longer indifferent to other molecules, they are possessed of powers of attraction which are by no means inconsiderable.

METALLIC VACUUM VESSELS.

One great advantage attaching to the use of charcoal is that it has rendered possible the maintenance of a very high vacuum during any required period of time. In the pre-charcoal period, this was impossible, owing to the leakage of gas into the exhausted vessel either as a consequence of the mechanical imperfection of the glass vessel or because of the existence of air imprisoned in bubbles or tubules within the glass: such leakage may now be counteracted by means of charcoal.

Metallic vacuum vessels could not be made formerly for a similar reason, the gas occluded within the metal escaping gradually and spoiling the vacuum in the vessel. By enclosing a quantity of charcoal in a globular space A (Fig. 8), so that it is cooled by the liquid air in the inner vessel, this difficulty is entirely obviated: such metallic vessels are now made of nickel, brass or copper, provided with necks made of an alloy of low conducting power. When properly constructed, these vessels are as effective as chemically silvered glass vacuum vessels; as they are not fragile like the glass vessels, it is to be expected that they will be of the greatest service in future work with liquefied gases.

The manufacture of Dewar vacuum vessels is now a German

* The following formula gives an approximation to the vapour pressure in mm.; T being the absolute temperature $\log P_{\text{He}} = 5.324 - 11/T$. A similar formula for liquid hydrogen gives $\log P_{\text{H}} = 5.778 - 54.6/T$.

industry of considerable magnitude ; it is well known, in fact, that under the name of "Thermos flasks" they are now in popular use both for storing hot beverages and for keeping liquids cold in summer time. In view of the importance of food being always

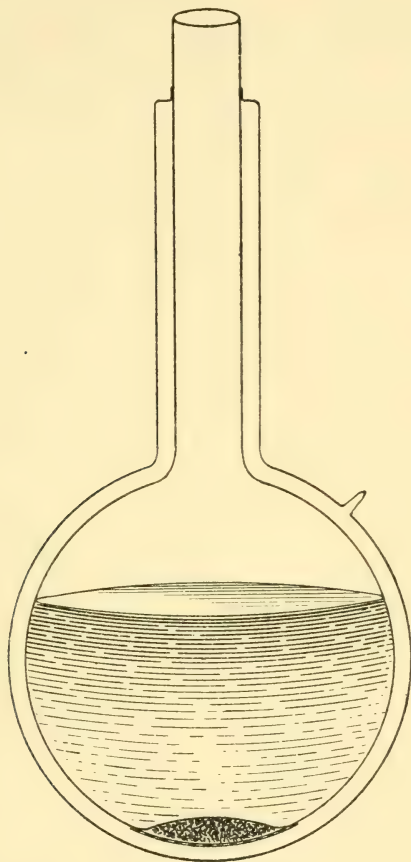


FIG. 8.

administered to infants at an even temperature, the use of such flasks may be expected to become very general. This is one illustration of the way in which the research work of the Royal Institution has conducted to the realisation of the objects of its founder, Count Rumford.

PROPERTIES AND STRUCTURE OF CARBON.

The differences, which are both qualitative and quantitative, between various kinds of charcoal depend probably both on differences in porosity and on differences in composition. Charcoal is by no means a single substance but contains more or less hydrogen, oxygen and nitrogen; the amount of actual carbon, in an uncombined form, which is present in it cannot even be surmised.

Analyses of blood charcoal, for example, show that it may contain nearly 2 per cent. of hydrogen, 7 of nitrogen and 15 of oxygen, together with about 6 per cent. of mineral matter. Soot will contain over 60 per cent. of carbon, together with 7 or 8 of oxygen. Sugar charcoal may contain nearly 20 per cent. of oxygen.

Whatever the composition of charcoal may be, even if it consist entirely of compounds which are all but carbon, its properties are very nearly those of carbon in the amorphous state. It is therefore desirable to consider the peculiarities of amorphous carbon in elucidation of the remarkable power which charcoal possesses of attracting gases. In view of the very different behaviour of the three forms of carbon, it is almost certain that in the amorphous state it offers structural peculiarities which condition its special activity as an absorbent: there are two points of view, from which these peculiarities may be considered with advantage—the one being that of the genetic relationship of charcoal to carbon compounds, especially the hydrocarbons, the other that of its colour.

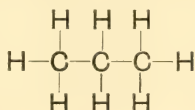
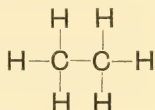
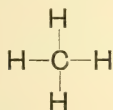
Genesis of Amorphous Carbon.—Of all the elements, carbon is the most remarkable on account of the endless multiplicity of compounds to which it gives rise, over 130,000 being already known. The properties of the element are to be inferred from the study of this vast host. By considering the peculiarities, by contrasting and correlating the idiosyncrasies of the various compounds, the chemist is eventually enabled to paint a picture of the ideal substance symbolised by the letter C—the symbol significant of the carbon unit or atom; that is to say, of the elementary material carbon but not of any of the forms of carbon actually known to us, the presumption being that these are all substances of great molecular complexity the formation of which is a consequence of peculiarities inherent in the carbon atom.

The potentialities of the carbon atom are best elucidated by reference to the behaviour of the hydrocarbons—the compounds formed by the association of hydrogen with carbon. The simplest of these, which is represented by the formula CH_4 , methane or marsh gas, is a *saturated* compound; nothing can be combined with it but one or more of the hydrogen atoms it contains may be displaced by another radicle of equivalent value in combining power—chlorine or bromine or iodine, for example. Taking the hydrogen atom as the unit, the atom fixing power or *valency* of carbon is indicated by the fact that

in methane the single carbon atom is satisfied by and in turn satisfies four single separate atoms of hydrogen—in other words, it is quadri-valent or tetradic.*

Methane is but one of a long series of hydrocarbons all of which contain carbon and hydrogen in the same relative proportions—those expressed by the formula C_nH_{2n+2} , as there are twice as many atoms and two more of hydrogen as of carbon in the molecule of each hydrocarbon. The petroleum pumped from the oil wells in Pennsylvania and elsewhere is a very complex mixture of such hydrocarbons, the natural gas from the same wells consisting of methane and other gaseous terms of the series; petrol, which now plays so important a part in motor practice, is a mixture of the lower liquid terms, such as *pentane*, C_5H_{12} , *hexane*, C_6H_{14} , *heptane*, C_7H_{16} ; the petroleum or paraffin oil used in lamps consists for the most part of much higher terms of the series; the mineral lubricating oils which have rendered such service in the high pressure steam engine, the unguent vaselin and the solid paraffin wax of which candles are made consist mainly of still more complex hydrocarbons similar to methane in composition.

The name paraffin has reference to the almost complete chemical indifference (*parum affinis*) of paraffin wax†; as all the terms of the series from methane upwards manifest this indifference, the name is now applied to them generally. The fact that they are thus indifferent is of significance as a proof that whatever the complexity, whatever the number of carbon and of hydrogen atoms in the hydrocarbon, the affinities of the atoms are mutually satisfied; if this were not the case, some terms would be more active chemically than others. As hydrogen atoms, *ex hypothesi*, cannot link other atoms together, it follows that in all the terms above methane the carbon atoms must be directly linked together and that only their spare affinities are satisfied by hydrogen. On these assumptions, the paraffins are formulated as chains of tetrad carbon atoms simply linked together by single affinities, thus—



But there is reason to believe that the affinities of the carbon atom can only act in certain directions and that when a number of such atoms are united together they do not form a long straight chain—an uneconomical mode of packing—but that they become

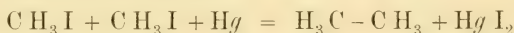
* A variety of considerations all justify and indeed necessitate the conclusion that the hydrogen atom acts uniformly as a univalent or monad radicle.

† As *cerotic acid*, $C_{27}H_{54}O_2$, may be obtained by oxidising paraffin wax, it follows that hydrocarbons containing at least 27 atoms of carbon are present in the wax.

arranged spirally, so that a complex compound such as paraffin wax may be pictured as resembling a *curl* or *helix* rather than a straggling chain. The most appropriate model, in fact, that can be constructed to *symbolise the functions* of a carbon atom is a regular tetrahedron.* And supposing the tetrahedron to be inscribed within a sphere, the four affinities of the atom may be pictured as proceeding from the centre of the sphere to the four solid angles of the tetrahedron; the angle at which two affinities meet is therefore $109^{\circ} 28'$ and while one pair of the affinities is situated in one plane the second pair lies in a plane at right angles to the first pair.

A skeleton wire model of the carbon affinity system is easily made by bending two pieces of wire each at an angle of $109^{\circ} 28'$ and then soldering the two pieces together at their angles so that they meet in two planes at right angles to one another; the four arms represent the affinities and the directions in which they act. Models of paraffins are easily constructed by joining the proper number of such skeleton tetrahedra together by laying an arm of one tetrahedron against an arm of another and then lashing or soldering the two together.

The representation of the carbon atoms in the paraffins as united by single affinities is but the expression of the fact that the higher hydrocarbons are prepared from the lower by displacing a single atom of hydrogen—say by a single atom of iodine—in any lower hydrocarbon, then withdrawing the iodine by means of a metal; the hydrocarbon residue thus formed at once unites with a like group formed from another molecule of the iodide. Thus, when methyl iodide from methane is exposed over mercury to sunlight, it is converted into dimethyl or ethane—



It is possible, however, not only to associate carbon atoms by single affinities in open chains or curls having their two ends free but these ends may be united together so as to form closed chains or rings (*cycloids*). Such a ring is formed with particular readiness by the union of five carbon atoms; the hydrocarbon of these dimensions is particularly stable and unattractive, like the paraffins.

When the number of carbon atoms united in a ring is less than five, the compound is no longer unattractive but, on the contrary, is acted upon more or less readily by a variety of agents, the ring being broken in the process. Each of the angles within a regular pentagon being very nearly $109^{\circ} 28'$ (the angle at which the affinities meet in the tetrahedron model of the carbon atom constructed in the manner described above) it is a striking fact that when five such tetrahedra are joined together they form practically a complete

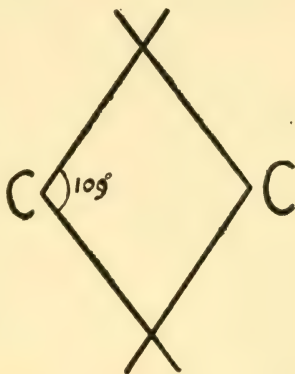
* Such a model may be constructed of four equilateral triangles, cut out of stout cardboard, joined together at their edges so as to form a solid figure.

pentagon. Any smaller number cannot be joined together in such a way that the wires representing affinities are brought into parallelism; they can only be joined by crossing the affinities and binding them together at the junctions. The size of the angle between two affinities may be regarded as the departure from parallelism, being greater the smaller the number of atoms united, indicating in some degree the relative stability of the ring system, the approximation of the affinities being inversely proportional to the exterior angle between crossed affinities—thus

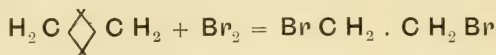
The behaviour of the closed chain hydrocarbons is in striking agreement with the geometrical peculiarities of models constructed in the manner described, so that however far removed they are from being representations of the manner in which the carbon atoms are actually combined, such models nevertheless serve a most useful purpose in indicating the peculiarities of the different types of compound.

Cycloids such as have been described are all paraffinoid compounds, in the sense that the carbon atoms are individually united in pairs by single affinities, as in the paraffins; they are also paraffinoid or saturated in the sense that they cannot enter directly into combination with other molecules; they differ from the paraffins, however, in being more or less attractive of other molecules to a greater extent than are the paraffins, which all but shun other molecules.

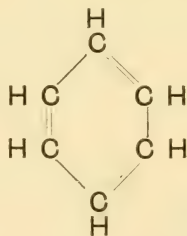
But carbon atoms may be united in pairs by more than single affinities—a fact which is clearly brought out in the tetrahedral model, as two tetrahedra may be united not only by joining an apex of the one to that of another but also by approximating an edge of the one to an edge of the other or even by bringing the triangular base of the one into contact with that of the other. Both these forms of combination are well known: the one occurs in ethene or ethylene (olefiant gas), C_2H_4 , the other in acetylene, C_2H_2 . When models of such compounds are made with skeleton tetrahedra, it is obvious that the affinities cannot be made to overlap and saturate one another as in the paraffins but that they can only be crossed: in other words, they saturate one another imperfectly and therefore such hydrocarbons should be attractive of other molecules, to a greater extent, moreover, than is the case with the cyclo-paraffins. In point of fact, both ethylene and acetylene behave as eminently unsaturated compounds, being, for example, immediately absorbed



by bromine and converted into derivatives of the corresponding paraffin ethane, in which the carbon atoms are united by single affinities :



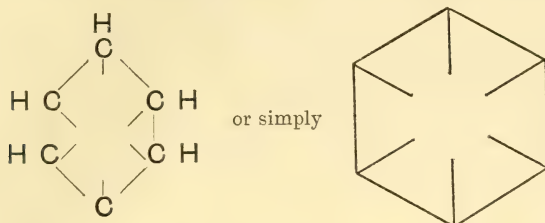
The hydrocarbon discovered by Faraday in 1825, now known as *benzene* on account of its relationship to benzoic acid, has peculiar properties which distinguish it from the paraffins and the ethenes, its behaviour being apparently that of a saturated and not that of an unsaturated hydrocarbon : to judge from its composition, C_6H_6 , assuming that the six carbon atoms are united together in a ring by single affinities and that six of the affinities are satisfied by hydrogen, six affinities still remain to be accounted for : as the compound appears to be saturated, these must be disposed of in some other way. There has been much dispute on this matter : Sir James Dewar, as far back as 1867*, himself suggested a way out of the difficulty but the arguments are now against the ingenious solution of the problem which he put forward. Kekulé, in 1865, enunciated his celebrated hypothesis that the carbon atoms in benzene are united in a ring alternately by single affinities as in the paraffins and by double affinities as in the ethenes ; he represented it, therefore, by the formula :—



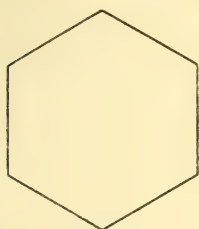
On this assumption, the behaviour of benzene should be that of ethylene exaggerated, which is in no way the case ; gradually, this objection has been recognised and Kekulé's formula no longer holds the field. At present, the tendency is to accept the centric formula as a more suitable symbol. In this the six affinities not engaged in the ring and not saturated by hydrogen are represented as acting towards a common centre and as neutralising one another by their

* On the Oxidation of Phenyl Alcohol, and a Mechanical Arrangement adapted to illustrate structure in the Non-saturated Hydrocarbons. Proc. Roy. Soc. Edin. 1866-7.

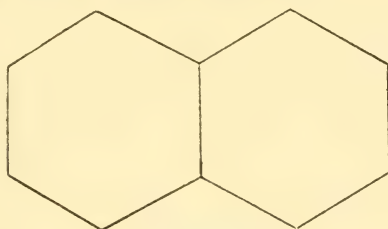
mutual influence, without, however, directly entering into combination. The symbol is written—



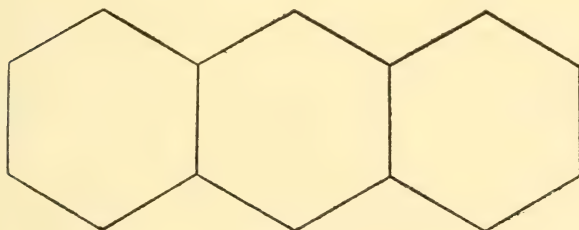
Benzene is one of the hydrocarbons which are produced in manufacturing gas by heating coal; other hydrocarbons formed at the same time are naphthalene and anthracene. It is now established that naphthalene, $C_{10}H_8$, may be regarded as formed by the fusion of two and anthracene, $C_{14}H_{10}$, of three benzene rings, thus—



Benzene



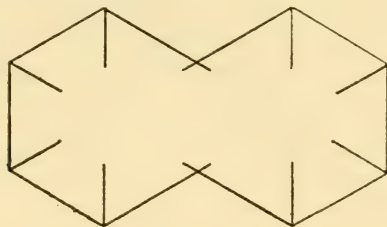
Naphthalene



Anthracene

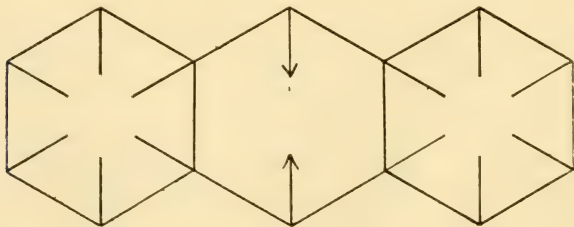
More complex hydrocarbons than these containing larger proportions of carbon and smaller proportions of hydrogen are present in coal tar, all of which apparently are built up in a similar manner: it would seem, indeed, *that the formation of carbon* from the simpler hydrocarbons involves not only the gradual loss of hydrogen but also a correlative growth in complexity, due to the fusion of ring upon ring in the manner illustrated in formulæ such as the above.

The conventional ring formulæ assigned to the benzenoid hydrocarbons, such as have been used above, however, are expressions which are to be regarded as *symbolic of the functions* of such substances rather than as absolute representations of their structure. Thus the simple hexagon by which benzene is represented above is symbolic of a symmetrical closed system. In representing naphthalene merely by two conjoined hexagons therefore, expression is given to the fact that it also functions as a symmetrical closed system. Actually, it must be supposed that it is impossible—assuming that benzene has a centric structure—to fuse two benzene rings together and yet preserve the centric structure of both. If the affinities of the carbon atom operate in any rigid manner “tetrahedrally,” the ten carbon atoms of naphthalene must form a constricted *monocycloid* system, thus—



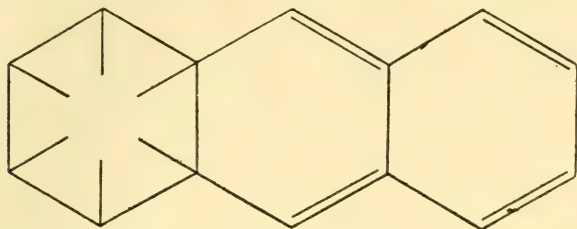
As the affinities at the waist of this system merely interlace, they do not satisfy each other; hence the unsaturated character of the molecule is represented by this formula, whilst at the same time naphthalene is shown to possess a special character of its own. Such a formula is in close accord with the general chemical behaviour of the hydrocarbon.

Anthracene, $C_{14}H_{10}$, may be modelled in carbon affinities in two ways, one of which is symmetrical and the other unsymmetrical:—



The first in no way corresponds to the properties of the hydrocarbon; the second, however, both gives expression to its unsym-

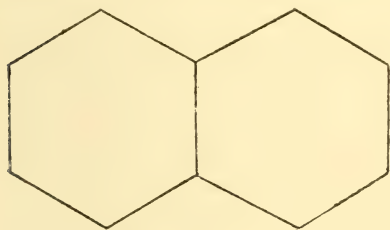
metric behaviour and is in harmony with the fact that it is a coloured substance. It will be noticed that in this formula the carbon atoms are united partly as they are in the paraffins (on the one side of the central ring), partly as in ethene, partly as in benzene. Attention



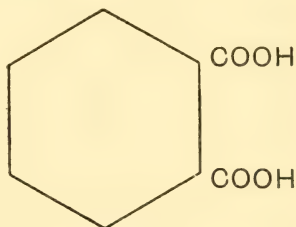
is called to these various formulæ in order that it may be clear that when a number of carbon atoms become associated they are necessarily arranged in a variety of ways.

Amorphous carbon itself is presumably but the last stage in a series of transformations, the end result being a complex in which a considerable, if not a very large, number of ring systems are so interlocked that the various affinities are all engaged. But to account for the properties of amorphous carbon, it is necessary to assume that ethenoid affinities—the unsatisfied conjoined pairs of affinities formed by the union of two carbon atoms by the partial saturation of two affinities of the one by two affinities of the other—preponderate in the molecule and come freely to the surface.

It may be pointed out that, when oxidised, naphthalene yields phthalic acid—an acid derived from benzene, thus—

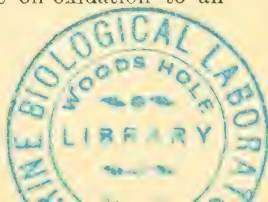


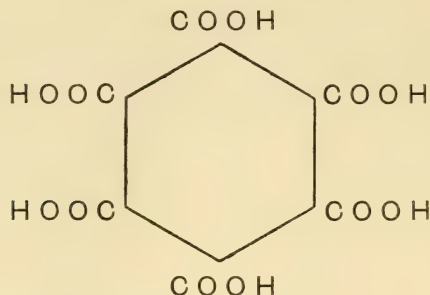
Naphthalene



Phthalic acid

Anthracene also gives rise to this acid on oxidation. Obviously, if two other systems such as that attached to one pair of carbon atoms of the centric nucleus in anthracene were attached to each of the remaining two pairs of carbon atoms in the same nucleus, a hydrocarbon would be formed capable of giving rise on oxidation to an acid of the formula—





The acid of this composition is known as *mellitic acid*; as the acid can be obtained by oxidising amorphous carbon, there is every reason to suppose that the carbon molecule in some way corresponds in structure to a hydrocarbon such as is here thought of.

Colour of Amorphous Carbon.—This also is a property which may be interpreted as proof that the molecule has a complex ethenoid structure.

In writing to Schönbein in 1852 Faraday remarks:—

“Your letter quite excites me and I trust you will establish undeniably your point. It would be a great thing to trace the state of combined oxygen by the colour of its compound, not only because it would show that the oxygen had a special state which could in the compound produce a special result but also because it would, as you say, make the optical effect come within the category of scientific appliances and serve the purpose of a philosophic indication and means of research, whereas it is now simply a thing to be looked at. Believing that there is nothing superfluous or deficient or accidental or indifferent in nature, I agree with you in believing that colour is essentially connected with the physical condition and nature of the body possessing it; and you will be doing a very great service to philosophy, if you give us a hint, however small it may seem at first, in the development or, as I may even say, in the perception of this connection.”

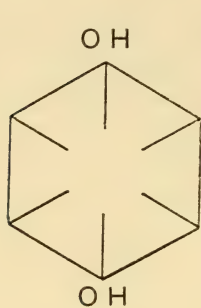
What Faraday foresaw is since come to pass. Light, it is well known, is always more or less refracted by passage through a transparent medium. Among carbon compounds the paraffins are the least refractive substances known. If certain values be taken as atomic refraction constants (comp. Brühl, these Proceedings, vol. xviii. p. 122), it is easy to deduce the molecular refractive power of any paraffin hydrocarbon and the value thus found is in close approximation to that calculated. Carbon in the ethenoid state, however, produces a greater effect; it has the peculiarity, moreover, that when two or more pairs of ethenoid carbon atoms occur in *close conjunction* the refractive power is not merely the sum

of the refractive powers which such systems would exercise separately but always greater. The system of carbon atoms forming benzene also has a peculiar influence on light and when such systems are combined the effect they exercise is in excess of the sum of their influences apart.

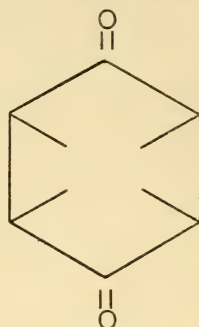
Colour may even arise by the superposition of the effects produced at ethenoid junctions and in benzenoid systems—in fact, it is now beyond question that colour is conditioned by structure and it is doubtful if any really simple molecule be coloured. Although benzene and naphthalene are colourless, anthracene, as already mentioned, is coloured (pale yellowish-green); moreover it is fluorescent. It will be noticed, on reference to the formula on p. 375 that anthracene is represented as containing four ethenoid systems and a centric benzenoid system: its colour is doubtless due to the co-operation of these, each system acting as a special “light-absorbing” or resonating centre but the effect produced in this case, judging from the intensity of the colour, is not very considerable. Yellow and red hydrocarbons are also known: thus the red colouring matter of the carrot is a hydrocarbon; there is reason to suppose that the number of ethenoid and centric systems in these compounds co-operating in the production of colour is larger than in anthracene.

Relatively few coloured hydrocarbons are known, however. The majority of coloured organic substances are hydrocarbon derivatives containing either oxygen or nitrogen or both these elements in certain definite forms of combination. To give an example, Faraday's benzene (C_6H_6), which although highly refractive, is colourless, is easily converted into a coloured substance by means of oxygen. By introducing in place of two of its hydrogen atoms two hydroxyl groups—that is to say, the fundamental molecule of water, OH_2 , minus one of the atoms of hydrogen—*hydroquinone* or *quinol* is produced, the substance well known among photographers, who use it as a developing agent. Quinol is a non-volatile, odourless, colourless crystalline substance which dissolves easily in water. It is very sensitive to the action of oxidising agents, whereby it is at once deprived of the two atoms of hydrogen contained in the two hydroxyl groups and converted into quinone, which is so called as it was first obtained by oxidising quinic acid from cinchona bark. The change may be pictured in the manner shown on next page.

Thus represented, the change involves a striking alteration in the contexture of the molecule, in fact, the passage of the oxygen into a special state such as Faraday contemplated. Corresponding to this change, an extraordinary alteration in properties is noticeable, quinone being a deep yellow coloured, highly volatile solid, of marked odour, scarcely soluble in water. Its colour may be ascribed to the presence in the one molecule of the two ethenoid (oxygen-carbon) junctions and the benzenoid centric junction, which together



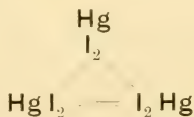
Quinol



Quinone

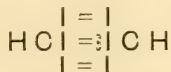
form an unsymmetrical light absorbing system. Most coloured substances appear to contain such a "quinonoid" system; even a simple substance such as iodoform, CHI_3 , which has a yellow colour, may be regarded as a compound of quinonoid type, since iodine has a tendency to function as a tervalent element, so that each iodine atom in iodoform has two affinities potentially free. Normally, the single quinonoid system has a pale colour—a more or less pronounced yellow—but if additional "absorbing centres" are developed in the molecule, the colour grows in intensity and is especially intense in cases in which two or more such systems are associated.

Mercuric iodide is another case in point: in the solid state this compound is either yellow or scarlet; in the gaseous state, however, it is said to be colourless and when dissolved in certain solvents it forms colourless solutions. According to the quinonoid hypothesis, a molecule of the formula HgI_2 should be colourless, like that of methylene iodide, CH_2I_2 ; but if several such molecules were conjoined, thus



a system corresponding to that of quinone would be formed.

It is not improbable that the yellow colour of solid iodoform is that of a complex such as



Moreover, the blue colour of water, of oxygen and even that of ozone, in which Faraday displayed such special interest, is not improbably conditioned by the presence of molecules of a more

complex character than those represented by the conventional formulæ OH_2 , O_2 and O_3 .

True blacks have not yet been produced by artificial means but a number of artificial dye stuffs are known the colour of which borders on black. The almost complete absorption of light by such materials appears to be conditioned by the conjunction of a number of light-absorbing systems and the superposition of their individual absorptive effects, as they are all substances of complex benzenoid structure, containing several systems each of which taken singly would be more or less intensely coloured—usually yellow or red.

It is therefore probable that the blackness of the amorphous forms of carbon is due to its complex atomic structure and that it is composed of ethenoid-benzenoid systems similar to those which are met with in the more complex coal-tar hydrocarbons. A similar argument would lead us to attribute a complex paraffinoid structure to the diamond: it cannot well be supposed that six atoms of carbon can form a saturated system in the manner represented by Brühl (*loc. cit.*), unless, being formed under great pressure, the affinities in the diamond are forced to act in unusual directions, as in Tammann's ice of greater density than water. The fact that two atoms of nitrogen can form a saturated molecule shows the difficulty of arriving at positive conclusions in such cases, however.

Sir James Dewar's observations on the marvellous absorptive power of charcoal at low temperatures appear, in some measure, to meet with an explanation from the point of view above advocated; they at least afford strong presumptive proof that charcoal is possessed of properties such as are characteristic of ethenoid compounds.

CONDUCTIVITY IN HIGH CHARCOAL VACUA IN RELATION TO THE THEORY OF CHEMICAL CHANGE AND INDUCTION PHENOMENA.

Among the observations recorded by Sir James Dewar during the Septenate under discussion, there are not a few of which apparently the full significance has yet to be appreciated. Reference may first be made to a recent series of experiments in which a novel use has been made of the Crookes radiometer in determining small gas pressures. The observations show that when helium is the residuary material filling the instrument, an attached charcoal condenser cooled in liquid hydrogen is unable to absorb the gas sufficiently to diminish the pressure to such an extent that the radiometer will not rotate when the concentrated beam of an electric

arc-lamp is focused upon the black surface of the inner vanes. Even when the charcoal condenser is cooled in solid hydrogen to a temperature of about 15° absolute, the vanes of the instrument still rotate when exposed to the beam. If, however, hydrogen be the residuary gas, on cooling the charcoal condenser in liquid hydrogen the radiometer can no longer be excited into action. Tested in the ordinary way, by means of an induction coil, the bulb charged with helium, in which the radiometer vanes still rotate, appears to be "vacuous," as no discharge will pass through it.

The crucial importance of these observations lies in the fact that the method of purification adopted is so complete that all gases other than helium are removed from the sphere of action.

It is thus proved that a relatively considerable amount of gas may be present and yet no electric discharge will take place—in other words, that the passage of an electric current of high potential through a gaseous atmosphere is dependent, apparently, not on the mere presence of *one particular kind* but on that of appropriate *systems* of molecules, since it can only be supposed that the ordinary conductivity of "helium" in a tube prepared without using the very special precautions taken by Sir James Dewar is conditioned by its association with a minute proportion of some other substance—perhaps impure vapour of water.

A similar observation, which has always seemed to me to afford proof of the same kind, has been brought under notice on more than one occasion in the Royal Institution lectures—namely, the observation that when one of Sir William Crookes's tubes, containing an earth which phosphoresces on passing an electric discharge through the tube, is cooled locally by means of a wad soaked in liquid air, the discharge will no longer pass and the earth cannot be excited into phosphorescence. The ease with which the passage of the discharge is prevented is such as to leave no doubt that some quite volatile substance is present and becomes condensed on cooling the tube locally. This explanation will be the more easily accepted by those who have witnessed the striking demonstration often given by Sir James Dewar of the efficiency of liquid air as a cooling agent, which consists in placing a few cubic centimetres of the refrigerant in a depression in a flask charged with gaseous bromine at a low pressure. As the air boils away, the gaseous molecules within the flask can be visualised as rushing towards the cold surface and, as it were, falling asleep in the solid state as soon as they reach it, for all colour soon disappears from the interior of the flask, the bromine forming a dark solid patch on its side where it is cooled.

No observations which have been made of late years appear to me to be of more consequence than these two, in connection with the general problem as to the nature of the conditions determinative of the passage of an electric discharge through a gaseous atmosphere

at low pressures. In this, probably, is involved the whole question as to the existence, as actual separate entities, of the mysterious units termed electrons, to which so much is now attributed—but on the basis of experiments that, for the most part, have never involved an approach to the care taken in the inquiry now under discussion.

In discussing the problems of the atmosphere in 1902, Sir James drew attention to the auroral discharges in its upper regions and to the partial identification of the lines in the spectrum of the discharge with those given by some of the newly discovered gases, especially neon; at the same time he pointed out that we have still to account for the appearance of some of the rays of these gases and for the absence of others, particularly of all the rays of nitrogen. To quote one of his statements:—

“If we cannot give the reason of this, it is because we do not know the mechanism of luminescence—nor even when the particles which carry the electricity are themselves luminous or whether they only produce stresses causing other particles which encounter them to vibrate; yet we are certain that an electric discharge in a highly rarefied mixture of gases lights one element and not another, in a way which, to our ignorance, seems capricious.”

It may be that in the intensely cold upper regions of the atmosphere precisely those substances are absent—such as water vapour—which necessarily accompany the gases under ordinary laboratory conditions, a degree of purification being effected such as Sir James himself has demonstrated to be an effective means of stopping the discharge.

It may be added that the results under discussion are in harmony with those arrived at by Dr. H. B. Baker; in fact, the refined experiments on the influence of conducting moisture on the occurrence of chemical change in gases which Dr. Baker has carried out with such exceptional skill afford a body of evidence which, taken together with that elicited in the laboratory of the Royal Institution, should compel attention to the need of greater precaution in practice and, meanwhile, of greater caution in speculation.

Sir James Dewar has pointed out that it would be interesting to repeat light repulsion experiments in the highest attainable charcoal vacuum. Perhaps, in making this suggestion, he has in mind the possibility that the measured effects may be in part attributable to secondary causes such as are brought to light in his radiometer observations.

A similar question may be asked also with reference to the difference of electric potential which is set up when two metals are brought into contact—a subject of much controversy in the past. The one

side has held that the effect is due to "atmospheric" influences and that it is produced at the expense of some amount of chemical change; the other has regarded it as a purely inductive effect, although on this point again there is a difference of opinion as to whether or no the metals alone are concerned.

His suggestion may also be extended to a subject which occupied a large share of Faraday's thoughts—that of *inductive capacity*. The so-called specific inductive capacity of a vacuum was measured by a Committee of the British Association in 1880, soon after the introduction of the Sprengel pump had led to an improvement in the means of securing a high degree of exhaustion. The value was but slightly lower than that deduced previously from measurements in ordinary vacua (B.A. Report, 1880, p. 197). The question is whether in the highest attainable charcoal vacua the value would differ from that obtained under more ordinary conditions.

Faraday thought of induction "as in all cases an action of contiguous particles"; the power of propagation possessed by so-called insulators he called their Specific Inductive Capacity. But, as Tyndall has insisted, Faraday regarded insulators and conductors as merely the opposite terms in a series. The insulators in common use, we now know, are merely electrolytic conductors of very high resistance and the values of their Specific Inductive Capacity may be regarded as indicating their relative conductivities: if, as argued above, the passage of electricity through gases be dependent on the formation of complex conducting systems comparable with those which there is reason to believe condition the conductivity of liquid electrolytes, the probability that vacua such as Sir James Dewar has obtained would afford results different from those previously obtained is considerable: in any case it is desirable to examine into their behaviour. The whole subject is in need of further investigation, not in any dogmatic spirit but rather in that which Faraday had in mind when recommending the science of electricity as a fine and ready field of discovery—"to those philosophers who pursue the inquiry zealously yet cautiously, combining experiment with analogy, suspicious of their preconceived notions, paying more respect to a fact than a theory, not too hasty to generalise and, above all things, willing at every step to cross-examine their own opinions both by reasoning and experiment."

Although of late years electrical considerations have been introduced into chemical discussions, this has been done in so narrow a spirit that the security of our position is but little greater than when the connexion was first established by Faraday himself in his earliest researches.

The refined and laborious determinations previously made (1895-97) in the Royal Institution Laboratory of the dielectric constants of various liquids and frozen electrolytes at the low temperatures afforded

by liquid air* have furnished results of extraordinary interest and importance. Professors Dewar and Fleming were led by their observations to the conclusion that, in all probability, at temperatures not far from the absolute zero, in the case of substances generally (other than metallic conductors), the value of the dielectric constant would be only two to three times that of a vacuum. Taking into account the very considerable difference in the results obtained on using specially purified distilled water instead of ordinary distilled water,† however, it is clear that even a minute proportion of impurity may affect the results; moreover the effect of reducing the temperature to the degree possible with liquefied hydrogen has to be ascertained. If induction through dielectrics such as water be at least mainly due to the propagation of an "electrolytic impulse," at temperatures at which all "fluidity" is destroyed and the molecules are firmly locked in their positions, no such action should be possible: the specific inductive capacity should fall at least to that of a vacuum.

It was clear to Faraday that a vacuum might have inductive capacity—that a charge might be communicated across a space devoid of conducting, contiguous particles; but he awaited proof before deciding and we cannot do otherwise than follow his example; probably we shall do well to suspend our judgment also in the case of dielectrics.

It is matter for congratulation that the laboratory in which such transcendent problems were first stated should be that in which the closest approach is being made to means of solving them.

Chemical Interactions under Reduced Pressure.—Sir James Dewar's observations on the interaction of sulphur and mercury and those on phosphorescence also deserve special notice from the point of view now under consideration.

If sulphur be placed at the one end and mercury at the other of a \cap -shaped tube (Fig. 9) and the tube be exhausted and sealed up, keeping the two ends in liquid air during the time required to reach a high vacuum either by means of the pump or by cooled charcoal, after several hours at the ordinary temperature the surface of the mercury appears tarnished, owing to the formation of a film of the sulphide. As the vapour pressure of mercury exceeds that of sulphur considerably, it was to be expected that the formation of sulphide would have taken place on the sulphur side; and, in fact, if the \cap tube be constricted at the bend (Fig. 10), sulphide is deposited at the bend. The molecule of sulphur is known to be complex even at temperatures considerably above its boiling-point but it is entirely resolved into simple diatomic molecules (S_2) at high temperatures. Sir James Dewar's experiments would indicate that such simple

* Roy. Soc. Proc., vols. lxi. and lxii.

† Ibid., 1897, lxi. 319.

molecules are probably given off even at ordinary temperatures in a vacuum. It cannot be decided whether "moisture" was in any way concerned with the occurrence of change in these experiments; there must have been some moisture present, as cooling the tube during the exhaustion would have the effect of retaining a certain amount condensed on the cold surfaces.

An even more striking experiment described by Sir James is that with phosphorus. A bulb A of the shape shown (Fig. 11), to which is attached a chamber D containing charcoal covered with a layer of phosphoric anhydride "to absorb all traces of moisture," also a small mercury gauge G and a short capillary branch P containing a little phosphorus, having been thoroughly exhausted, is

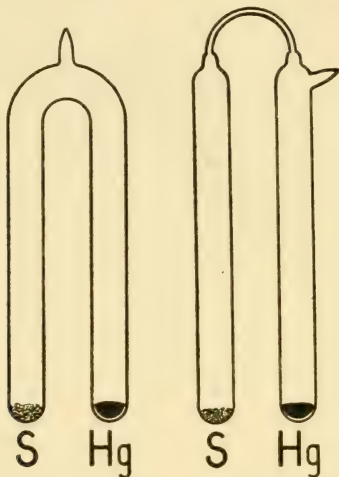


FIG. 9.

FIG. 10.

filled at atmospheric pressure with oxygen and then sealed. On immersing the charcoal chamber in liquid air the pressure soon falls to a fraction of a millimetre; then suddenly the chamber A becomes filled with a phosphorescent glow, which is clear indication of the occurrence of chemical change. The pressure having fallen to a point at which it can no longer be measured by the gauge, a stage is reached when the glow disappears and only phosphorus distills into the charcoal condenser. When the charcoal is no longer cooled and oxygen escapes from it, the phenomena are reversed: as the gas meets the phosphorus in the bulb A, the occurrence of an interaction is marked by oscillating flashes; soon all is dark again. The demonstration is a most fascinating one, as I can testify, having witnessed it several times. It may be supposed either that some particular

form of phosphorus molecule is the origin of the glow or that it marks a stage at which some special oxide is formed. The experiments of Jungfleisch lead to the conclusion that the cause of the phosphorescence is the oxidation of phosphorus anhydride, but in this apparatus no luminosity was observed when this oxide replaced the phosphorus.

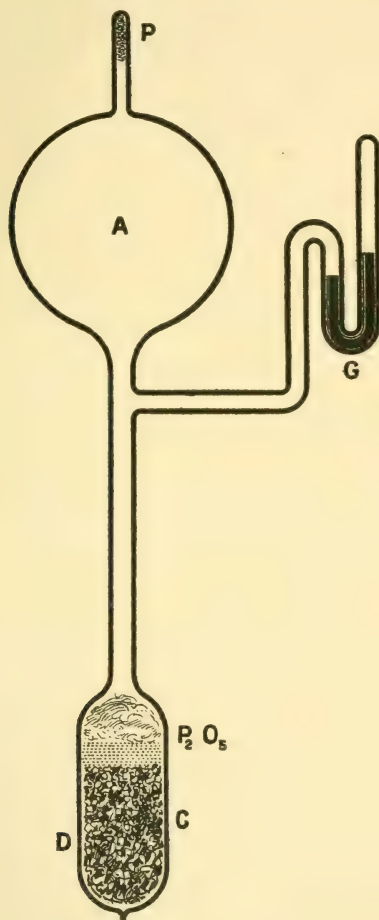


FIG. 11.

In any case, the demonstration affords a proof of the narrow range of conditions within which a particular kind of change may occur. I doubt if it afford any proof that oxidation of phosphorus can be effected in the entire absence of "moisture." Phosphoric

anhydride cannot be credited with absolute dehydrating power; and although water vapour cannot well have remained in the bulb, it must be granted that a certain proportion of simple molecules of hydrone (OH_2) may have been present together with the phosphorus molecules. We are perhaps too much in the habit of associating the properties of water with those of the hydrone molecule: being lighter than that of most gases, the simple molecule of water must be highly volatile, although more prone than most molecules to adhere to surfaces with which it may come in contact.

Photographic Action at Low Temperatures.—In the Bakerian lecture delivered to the Royal Society in 1901, Sir James states that "Photographic action is still active at the temperature of boiling hydrogen although it is reduced to about half the intensity it bears at the temperature of liquid air." This apparently is a confirmation of his previous conclusion based on experiments made with liquid air, that such action can take place at low temperatures and in absence of moisture. The subject is a difficult one to discuss, owing to our ignorance not merely of the actual nature of the effect produced by light on the sensitive silver salt but also of the nature of the active "system." Even assuming, however, that it be the silver haloid alone, although this is *per se* an electrolyte in the viscous or fluid state, it is probably not one in the rigid state; and the same argument would apply to any system composed of the haloid and a sensitiser: therefore it should not be sensitive to light at low temperatures. It is perhaps not improbable that the photographic effect is the result of autoexcitation consequent on the phosphorescence which Sir James has shown to be conditioned in so many substances by exposure to light at low temperatures.

In calling attention to the bearing of all these different results on the one problem, I am led by the desire to lay all possible emphasis on their importance and the need of further inquiry.

Solid Fluorine.—The liquefaction of fluorine during the earlier period has now been followed by its solidification by means of liquid hydrogen. The yellow liquid is converted into a white solid,* melting at about 40° absolute, a temperature a little below that at which solid oxygen melts. When the point of a tube containing solid fluorine plunged in liquid hydrogen was broken off by means of steel pincers a violent explosion took place.

A further series of observations on the interactions of liquid fluorine with substances previously cooled in liquid air have been

* Chlorine also becomes white when it solidifies. In the account given to the French Academy of these experiments (*Comptes Rendus*, cxxxvi. 642), it is implied that bromine is colourless when solid. This is incorrect. I am informed by Sir James Dewar that it only becomes somewhat paler in colour.

recorded. Sulphur, selenium, red phosphorus and arsenic are all violently acted upon but tellurium and antimony are not affected. Sodium is slowly attacked, potassium violently after an interval; lime is decomposed with violence and the hydrocarbon anthracene is at once acted upon. It has been argued that these observations indicate the persistence of a high degree of affinity at low temperatures but it may be questioned whether such is the case: the least change would cause the temperature to rise locally and once started might take place under "spheroidal state" conditions.

MODIFICATIONS IN THE PROPERTIES OF MATTER AT LOW TEMPERATURES.

Effect of extreme Refrigeration on Alloys of Iron.—In continuation of previous work on the strength of metals at the temperature of liquid air, Sir James Dewar and Mr. (now Sir) R. A. Hadfield have carried out a long series of observations with iron and allied metals. The results corroborate the inference previously drawn that all common metals and alloys increase in tenacity at low temperatures whether their ductility increase or decrease, the change persisting only during the period of cooling.

In the case of Swedish charcoal-iron, the tenacity rose from 20 to 38 tons per square inch, there being substantially no elongation. Steels behaved similarly.

Nickel was improved not only in tenacity (its tensile strength being increased from 29 to 46 tons) but also in ductility (from 43 to 51 per cent.). Moreover, the ductility of all the nickel-iron alloys examined was diminished to only a moderate extent by cooling.

Era manganese steel (containing C 1·23, Mn 12·64 per cent.), which is remarkable as being non-magnetic and on account of its tenacity (56 tons) and ductility (30 per cent. elongation), when cooled to -185° , although but slightly more tenacious was almost entirely detoughened, being elongated only to the extent of $2\frac{1}{2}$ per cent.

The most remarkable are the iron-manganese-nickel alloys containing about 6 per cent. of manganese and from 14 to 24 per cent. of nickel. These alloys are non-magnetic and possess the highest electric resistance of any known alloys; they are also the most ductile alloys yet made. The ductility of an alloy containing about 14 per cent. of nickel was reduced by cooling from 75 to 25 per cent.; one containing about 18 per cent. decreased in ductility only from 57 to 42 per cent., whilst a third containing 24 per cent. actually rose in ductility from 60 to 67 per cent.—this being the first alloy met with which increased in ductility on cooling.

Such results are very remarkable. The magnetic properties of iron are probably not, as was at one time supposed, inherent in the

simple molecules but are characteristic of particular structural arrangements of such simple molecules; in fact the remarkable variation in properties met with among the iron alloys may well be traceable, at least in large part, to structural differences. Unfortunately such problems cannot be dealt with at present except inferentially; on this account the insight afforded by the experiments carried on at low temperatures is of very special value and interest.

Ice at Low Temperatures.—Water is undoubtedly one of if not the most remarkable of known substances, both in its physical and its chemical behaviour. On cooling the liquid, it ceases to contract at 4°C. and expands slightly until the freezing-point (0°C.) is reached, when it becomes ice; the conversion of the liquid into solid is attended by a great increase in bulk, the density of ice at 0°C. being only 0.91599 grams per cubic centimetre. There can be no doubt that ice molecules are present in solution in the liquid, it may almost be said, long before solidification sets in, the temperature at which the density of water is at a maximum (4°C.) being that at which the contraction which the liquid undergoes on cooling is just balanced by the expansion consequent on a certain proportion of the molecules becoming arranged in the manner in which they are present in ice.

Liquid water doubtless consists only to a limited extent of the simple molecules which constitute vaporous water, that is to say, of molecules of the composition represented by the chemical formula OH_2 (hydrone); in addition to these, it probably contains complexes of several kinds formed by the association of the simple molecules of hydrone. Ice presumably is formed from some one kind of the complex molecules. In the case of oxygen as in that of carbon, there is every reason to suppose that the force of chemical affinity is exerted in certain specific directions and that when the affinities come into operation the molecules necessarily tend to assume certain relative positions. The oxygen in hydrone has a considerable amount of residual affinity, which is the cause of its activity: when owing to the reduction of temperature the vibrations of the molecules become sufficiently damped to allow the residual affinity to act, the affinities become, as it were, interlocked in certain directions and crystallisation is the consequence. Water, near to the ice point, may be likened to a pile of bricks placed one upon the other in close contact and therefore occupying minimum volume: ice may be likened to the same bricks arranged in open triangular form; so placed they enclose a hollow space and—inclusive of this space—occupy a greater volume than when placed directly one upon the other. But the formation of ice is an incomplete process of solidification; apparently ice is not a homogeneous substance but an equilibrated mixture, in certain proportions, of solid and liquid

molecules and it is to this circumstance, it may be supposed, that ice owes its peculiar viscosity and plasticity. As the temperature is lowered, the liquid molecules gradually give place to solid molecules and the ice becomes more and more nearly rigid; consequently the rate at which the volume alters diminishes as the temperature is lowered. Sir James Dewar's observations show that the density of ice at -185° is 0.92999, so that the mean cubical coefficient of change in volume between this temperature and 0° is 0.00008099. This mean value is only about half that observed between 0° and -20° , namely 0.0001551. Although clear pieces of ordinary ice dropped into liquid air crack in all directions, owing to the sudden cooling, when clear pieces that have been cooled slowly in liquid air are introduced into liquid hydrogen they do not crack—this behaviour is again proof that the expansibility diminishes at very low temperatures.

As the formation of ice from liquid water involves expansion, the freezing-point is necessarily lowered by pressure; in other words, the molecules of the solid are decomposed by pressure and forced to take on some other contexture. Various calculations have been made as to what would be the behaviour of water at very low temperatures and pressures, assuming that its properties change in the manner observed under more ordinary conditions; the conclusions arrived at, however, have all been upset by Tamman's remarkable observation* that under high pressure two solid forms of water exist which are probably both denser than water: one of these melts at -15.8° under a pressure of 5040 atmospheres. This conclusion is completely confirmed by Sir James Dewar's observation, that if water be frozen in a steel cylinder in successive portions and lead shot be included in the middle and upper portions, on cooling such ice to -80° and subjecting it to pressure, the lead shot do not fall through the ice even under a pressure of 100 tons per square inch: it is clear that pressure does not operate to the extent formerly supposed in depressing the freezing-point of water.

It may therefore be regarded as established that chemical affinity can operate in more than one direction between the units of the water complexes and that, provided the temperature be low enough, pressure alone may be effective in curbing the tendency of affinity to act in one particular direction or set of directions and even of compelling it or enabling it to act in some other direction which is less preferred under conditions of greater freedom. The complete investigation of the optical properties of the solid forms of water at low temperatures and under various pressures would be of great interest. There is some reason to suppose that colour is more highly developed in ice than in water but I am not aware that the comparison has been made in any satisfactory manner. The molecules of hydrone *per se* should be colourless, if the explanation of the origin of colour previously

* Ann. der Physik., 1900, ii. 1.

given be in any way correct; colour might arise, however, if several such molecules were so juxtaposed as to give rise to a system of junctions similar to those which appear to condition the appearance of colour in hydrocarbons and other compounds. The manner in which the colour of water changes as its state is varied should therefore afford some insight at least into the direction in which changes in structure are proceeding.

The alteration in colour from scarlet to yellow in the case of vermilion and of mercuric iodide and from yellow to white in the case of uranic nitrate and of ammonium platinum chloride, which is effected on cooling these substances in liquid air, may be regarded as affording proof of molecular simplification consequent on the suspension of unions of the residual affinities such as are referred to on p. 378. In the case of organic colouring matters, in which colour is conditioned by peculiarities of structure within the fundamental molecules, cooling has little if any influence on the colour.

It is when these various applications of the knowledge which is being gained of the properties of matter at low temperatures are appreciated, that the work done in the Royal Institution Laboratory is seen to be of such exceptional importance.

The determinations which have been made of the densities of oxygen, nitrogen and hydrogen in the solid state show that in these cases the density increases as the liquid becomes solid. It would be a matter of great interest if the comparison which can be made in the case of water between the solid and liquid states could be extended to other substances which are condensed only at low temperatures; at present the density of the solid in comparison with that of the liquid is known in but a few instances.

Sir James Dewar has calculated from his determinations of their densities at low temperatures that the molecular volume in the solid state—the volume occupied by the molecular proportion in grammes—of oxygen, nitrogen and hydrogen would be 21·2, 25·5 and 24·2 cubic centimetres at the absolute zero. It is interesting to note that the molecular volume of liquid hydrogen at its boiling point is 28·6, whereas that of liquid helium is 26·6. It is remarkable that the values should be so nearly alike, taking into account the great difference in the masses of the molecules.

India-rubber at Low Temperatures.—Perhaps the most striking case of alteration in properties effected by extreme refrigeration is afforded by india-rubber, which is one of the most elastic of substances at ordinary temperatures: when cooled in liquid air, however, it becomes so rigid and brittle that it is easily pulverised. Films 1/50 mm. thick are no longer permeable by liquid oxygen, or even liquid hydrogen. The main constituent of india-rubber is a complex hydrocarbon of the turpentine class, containing a number of ethenoid junctions; these junctions presumably determine its peculiar physical

properties and also render it a solvent of oxygen: probably these junctions become localised in their action at low temperatures, owing to some rearrangement of the molecules more or less akin to that involved in the formation of ice from liquid water. The effect produced by cold on india-rubber, it may be pointed out, is very different from that noticeable in the case of charcoal, the activity of which is also attributable to its polyethenoid structure; but in the latter case, being rigid, the ordinary molecule is probably insusceptible of rearrangement, so that the ethenoid junctions remain operative even at low temperatures. The power of condensing gases—more-over of acting as a selective “solvent”—which is exhibited by charcoal in so marked a degree, may be regarded as incipient in india-rubber, since this is a solvent of oxygen but not of other gases to any marked extent.

The manifestation of so high a degree of attraction for charcoal by hydrogen gas, as pointed out above, is proof that although it is a univalent element the biatomic molecules of the gas are possessed of residual affinity, which comes into operation when their rate of motion is sufficiently reduced by cooling.

A similar argument is applicable to helium.

CALORIMETRIC STUDIES.

When a volatile liquid is evaporated by passing a current of air through it, the temperature falls owing to the escape of heat in the vapour. Sir James Dewar has made a most interesting application of this principle by utilising it in solidifying liquefied nitrogen and hydrogen. If, as he has pointed out, the limit of temperature reached on evaporating a volatile liquid be contrasted with the critical temperature of the substance on the absolute scale, its value is about half the critical temperature, as shown in the following table:—

	Temperature of Evaporation	Boiling- point	Critical Temperature	Evaporation Temperature in terms of Absolute Critical Temperature
Ether	-34°	+35°	194°	·51
Sulphur dioxide	-50	-10	155	·52
Methyl chloride	-55	-24	141	·53
Ammonia	-87	-39	130	·46
Ethylene	-132	-103	10	·50

It therefore follows that if hydrogen be bubbled through liquid nitrogen, the critical temperature of which is - 146° C. or 127° absolute, the temperature should be reduced to about 273 - 63 = - 210°C. and consequently the nitrogen should freeze, as it melts

at about this temperature. Actually the solidification of liquid nitrogen is easily effected by bubbling hydrogen through it. A similar argument shows that liquefied hydrogen should be solidified by passing the more volatile helium through it, and this was found to take place when mixtures of hydrogen and helium, cooled to the temperature of exhausted liquid air, circulated through regenerator coils.

Liquid Air and liquid Hydrogen Calorimeters.—An achievement of great importance which comes within the period under review is the use of liquefied air and hydrogen as calorimetric substances by the evaporation of which the heat given out on cooling various substances through a known range of temperature—their heat capacities in fact—can be accurately determined.

In the case of air, the apparatus used is that shown in Fig. 12, in which B is the calorimeter—a small vacuum flask some 25–50 c.c. in

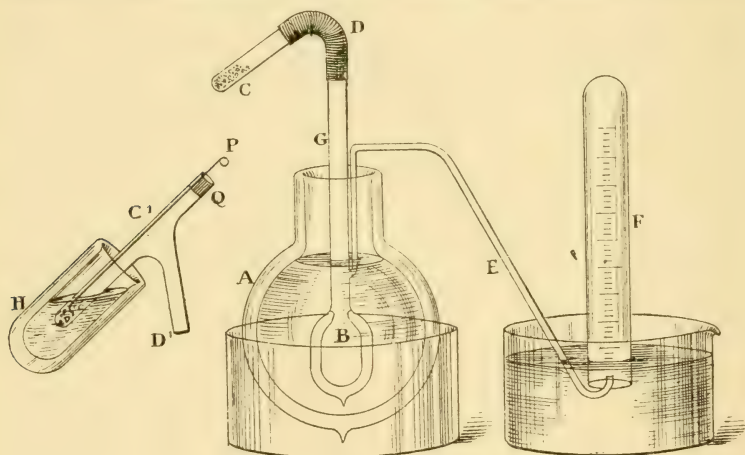


FIG. 12.

capacity—placed inside a larger vacuum vessel A, C being a tube connected with B by the flexible joint D and containing the substance to be dropped into B. The arrangement at the side represents an alternative method of introducing the substance in single pieces at a time into the calorimeter. By heating or cooling C or C', it is possible to determine the heat capacity of the substance between some particular temperature and that at which the liquid in the calorimeter boils. In practice, A is charged with a couple of litres of old liquid air rich in oxygen and some of the same liquid is introduced into B; in this way it is possible to maintain a fairly constant temperature throughout an experiment. The instrument is standardised by

dropping in a known weight of a substance of high atomic weight, such as lead, the specific heat of which varies but slightly with the temperature; the volume of gas given off is measured in the tube F.

The data in the following table illustrate the degree of sensitiveness of various liquefied gases as calorimetric agents when used in such an apparatus.

	Boiling-point	Liquid Volume in c.c. of 1 gramme at the Boiling-point	Latent Heat in gramme Calories	Volume of Gas in c.c. at 0° and 760 mm. per gramme Calorie
Sulphur dioxide . . .	+ 10·0	0·7	97·0	3·6
Carbon dioxide . . .	- 78·0	0·65 (solid)	142·4	3·6
Ethylene	-103·0	1·7	119·0	7·0
Oxygen	-182·5	0·9	53·0	13·2
Nitrogen	-195·6	1·3	50·0	15·9
Hydrogen	-252·5	14·3	125·0	88·9

It is obvious that oxygen is about twice as sensitive as ethylene, whilst hydrogen is between five and six times as sensitive as oxygen. In practice, there is an advantage in using liquefied air, as air is the substance surrounding us; when liquefied hydrogen is used, it is essential to prevent access of air to the apparatus, which may be modified for the purpose in the manner shown in Fig. 13.

Among the most interesting results obtained with these instruments are those relating to carbon—in the forms of graphite and diamond—and ice. In the case of carbon (diamond) previous determinations pointed to the disappearance of all heat capacity at about -90° . Sir James Dewar's observations show that although the heat capacity diminishes greatly as the temperature falls, it is still perceptible in amount even at -220° . The same is true of ice, thus:

	18° to -78°	-78° to -188°	-188° to -252°
Diamond	0·0794	0·0190	0·0043
Graphite	0·1341	0·0599	0·0133
Ice	0·463	0·285	0·146

To judge from the determinations which have been made with various substances, including those of the earlier period relating to metals, it is clear that as the absolute zero is approached the intermolecular vibrations become more and more damped and perhaps cease altogether at the zero. In the case of metals, electrical conductivity is then at its maximum but at its minimum in the case of non-metals and compound substances. To use a rough analogy in

illustration of the changes which attend the cooling, the metal molecules may be pictured as tubes provided with wide flanges,

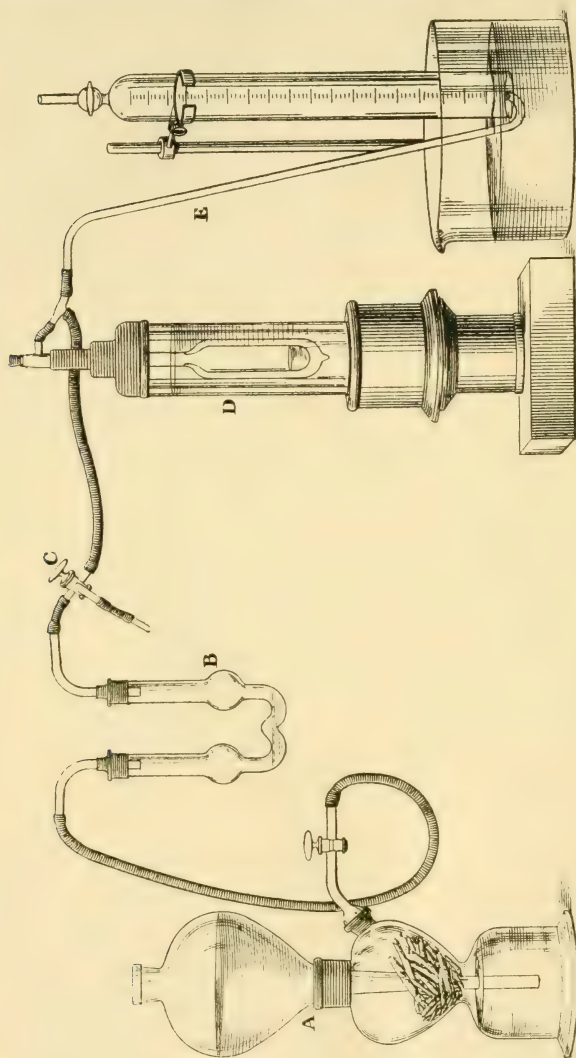


Fig. 13.

flange sliding on flange in such a manner that the passage of fluid through a series of connected tubes is more or less interfered with

by the oscillatory movement at each junction; as the temperature falls, the flanges come more nearly into apposition and ultimately coincide. Conductivity is then at its maximum, the rate of flow depending alone on the sectional area of the tube and the skin friction, being no longer checked by the oscillations of the tubes.

THERMOMETRY AT LOW TEMPERATURES.

Owing to the fact that the rate of change of resistance of platinum becomes gradually smaller at very low temperatures, the accurate determination of low degrees of temperature cannot well be effected by means of resistance thermometers.

An elaborate investigation of a number of constant pressure gas thermometers has been made from which it appears, that either a simple gas such as helium, hydrogen or oxygen or a compound gas such as carbon dioxide, at an initial pressure somewhat less than one atmosphere, may be made use of as the thermometric substance in determining temperatures near to but above that at which it boils.

The average value of the boiling point given by these experiments in the case of oxygen is $-182^{\circ}\cdot5$ and in the case of hydrogen $-252^{\circ}\cdot5$ or $20^{\circ}\cdot5$ * absolute. The value for oxygen is in agreement with the mean results obtained by Wroblewski, Olzewski and others.

The melting-point of hydrogen, determined by a helium thermometer, is 15° absolute.

A careful study of a large number of resistance thermometers has shown that at low temperatures these give variable results and that no thermometer of the kind will afford accurate values up to and below the boiling-point of hydrogen. At the boiling-point of

* In the interest of historical accuracy, it should be pointed out that this value was first given in the Bakerian lecture delivered to the Royal Society on February 7, 1901, published in their Proceedings, vol. lxxviii., pp. 44-54.

If reference be made to the third English edition of Ernst von Meyer's *History of Chemistry from the earliest times to the present day* (Macmillan), an authoritative work published in 1906, the following passage will be found (p. 523):—

"Dewar was the first to succeed in obtaining a measurable amount of liquid hydrogen (about 50 c.c. at one time) and he has since then been able to solidify it. An apparatus has also been designed by Travers by means of which liquid hydrogen can be obtained in quantity.

Liquid hydrogen is clear and colourless; it shows no absorption spectrum and the meniscus is as well defined as in the case of liquid air. The boiling-point was first determined by Dewar with a platinum resistance thermometer to be -238° C. but more recent determinations by Travers, using a helium thermometer, have given $-252^{\circ}\cdot5^{\circ}$ C., a number now accepted by Dewar."

As a matter of fact, Travers and Jaquerod make the following statement (p. 489) in a communication read to the Royal Society on June 19, 1902 Proceedings, vol. lxx., pp. 484):—

hydrogen, the reduction of the electric resistance of some metals is very remarkable—copper dropping to only 1/105th, gold 1/30th, platinum 1/35th to 1/17th, silver 1/24th of the resistance which it has at 0°. The resistance of an unalloyed metal diminishes

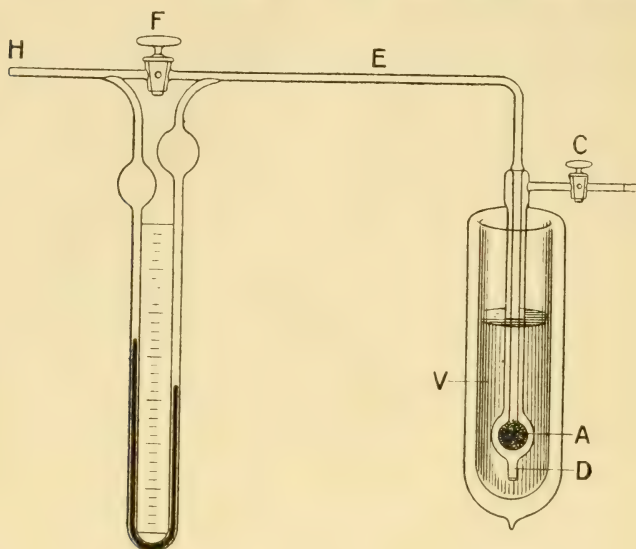


FIG. 14.

continually as the temperature falls and in each case appears to approach to a definite asymptotic limit. Gold and silver give the best measures of low temperatures. When considered in detail, the observations are full of interest as throwing light on the properties of individual metallic elements: obviously the last word on the subject has not yet been said and it is important that the inquiry should be extended to metals purified with every degree of refinement which chemical skill can devise: this, however, will be a task of great difficulty and must entail a vast amount of labour.

One of Sir James Dewar's most beautiful applications of charcoal consists in saturating it with air or hydrogen or helium and using it when thus charged as a thermometric substance. An apparatus for

"Dewar has obtained the following values for the boiling-point of hydrogen on the constant-volume hydrogen scale: $-253^{\circ}\cdot03$, $-253^{\circ}\cdot37$, $-252^{\circ}\cdot81$, $-250^{\circ}\cdot35$; the pressure on the gas at the ice-point being 287 mm., 270 mm., 739 mm. and 127 mm., respectively. On the scale of a thermometer filled with helium containing 7 per cent. of neon, at a pressure corresponding to 728 mm. of mercury at the ice-point, he found the temperature to be $252^{\circ}\cdot68$ and $252^{\circ}\cdot84$ C."

this purpose is depicted in fig. 14. In this arrangement A is a bulb containing charcoal saturated with air or hydrogen or helium at any desired pressure; V represents liquid air or hydrogen; the space between the bulb A and D is filled with the vapour of the liquid in V. On flashing even a feeble beam of light on the bulb, although the rays must penetrate through the vessels and the liquid in V, the level of the liquid in the gauge is at once altered. The special value of such an instrument is that it becomes more sensitive as the temperature falls.

GASES OF THE ATMOSPHERE.

The new field opened up within recent years by Lord Rayleigh's classical discovery of argon in our atmosphere has been cultivated with such brilliant success, particularly by Sir William Ramsay and Professor Travers, that we now know that air contains besides this gas four other elementary substances: helium, neon, krypton and xenon, all of which belong to the class of inert elements—that is to say, elements which are apparently destitute of chemical properties and incapable, so far as we are at present aware, of forming compounds with other elements—unless, as may well prove to be the case, radium be a compound of helium. These gases are at present the riddles of chemistry but it may well be that their true character is not yet appreciated: indeed, presumptive proof that they are not in reality inert is afforded by the fact that they give brilliant spectra, if the argument be correct which is put forward on p. 32 that electric discharges take place in gases within complex systems. That even the helium molecule has some degree of chemical activity is shown by the fact that it is powerfully attracted by charcoal at very low temperatures and that its heat of absorption is by no means inconsiderable. Lastly, it may be pointed out that the argument which has led to the assumption being made that the molecules of these gases consist of single atoms is based on the slenderest of foundations and is entirely inconclusive—atoms possessed of an extraordinary high degree of affinity might well form molecules which would be exceptionally sluggish in chemical behaviour: nitrogen, in fact, is an illustration of the force of this argument, being almost inert in the form of diatomic molecules, N_2 , although an extraordinarily active substance in the elementary state.

For these reasons the investigation of the properties of the gases of the helium group is of exceptional interest and importance.

Reference was made by Miss Clerke to the account given in 1901 by Liveing and Dewar of the separation of krypton and xenon from liquefied air by a single process of fractional distillation and of their observations on the spectra of these gases. Later observations on the separation of the various gases by means of charcoal have been referred to in an earlier section of this essay (p. 362).

In discussing the problems of the atmosphere in his Friday evening lecture in 1902, Sir James Dewar drew attention to the fact that the proportion of hydrogen constantly present in our atmosphere has been over-estimated and that there is not more than 1/900,000th of this gas in town air that has not come in contact with metal tubes.

In connection with the problem of the distribution of the minor constituents of the atmosphere, attention has been called to the conditions which affect the recognition of the various gases and to their bearing on the study of auroral discharges. The problems involving experimental study in this field are shown to be very numerous. Very striking conclusions have been arrived at as to the composition of the atmosphere in regions in which the temperature and pressure conditions are such as could not lead to the condensation of the oxygen and nitrogen.

It is probable that above fifty-six miles the atmosphere would consist substantially of the lighter gases, hydrogen and helium and neon. The effect which gradual elimination of oxygen especially would have on the colour of the sky has yet to be taken into account.

Whether other gases remain to be discovered in air is at present an open question: a considerable number of the lines seen in the spectra obtained on passing electric discharges through samples of the least condensable gases from the air are at present unidentified.

The subject of the Upper Air and Auroras is discussed very fully in Sir James Dewar's Presidential Address to the British Association at Belfast in 1902.

PROPERTIES OF RADIUM.

The properties of radium have been made the subject of study whenever opportunity has offered.

The heat evolved has been measured by means of the liquid oxygen and liquid hydrogen calorimeters.

It has been shown that a Blende screen, such as Sir William Crookes uses in his Spinhariscope, is not caused to scintillate by radium if the screen be cooled in liquid air; on cooling the radium, however, both screen and radium being *in vacuo*, the scintillations are as vigorous as at ordinary temperatures. This result is at once a confirmation of previous observations that phosphorescent substances generally lose their activity when cooled to a very low temperature and a proof that radium is an entirely exceptional material.

A series of observations of striking importance has been made recently on the rate at which helium is produced from radium: these not only afford a verification of Soddy and Ramsay's discovery but also a most remarkable confirmation of the correctness of the theoretical predictions of Professor Rutherford. The experiments derive exceptional value from the fact that they were carried out with the

material purified under the direction of Dr. Thorpe, which he has shown to have an "atomic weight" practically the same as that originally deduced for radium by Madame Curie. This material was prepared at the expense of a grant of 1000*l.* made to the Royal Society by the Goldsmiths' Company specially for the purpose of promoting the study of the properties of radium—of all substances known to us the most mysterious and exceptional in its behaviour.

MAINTENANCE OF VITALITY AT LOW TEMPERATURES.

Dr. Macfadyen's earlier experiments on this subject were referred to by Miss Clerke. He has stated in a later communication that no impairment of the vitality of bacteria is conditioned by their continued exposure to the intense cold of liquid air even for a period of six months. That all chemical action should come to an end at such temperatures was to be expected; Dr. Macfadyen's remarkable observations, however, establish the important fact that bacteria may be frozen without being ruptured or their minute mechanism dislocated—that, in fact, they resemble in behaviour a watch, which stops when frozen, because the oil used as a lubricant is congealed, although when the temperature is raised and the oil melts it is easily started into activity from its state of suspended animation. In the organism, water is the lubricant, as it were; the once congealed organism starts into life again so soon as the water is liquid and food materials can circulate in it and gain access to the enzymic centres at which they undergo constructive and oxidative changes.

A further use has been made of liquid air by Dr. Macfadyen which may well prove to be of great importance. He has shown that by triturating micro-organisms in the hardened condition to which they are reduced by cooling in liquid air, it is possible to liberate the cell contents by purely mechanical means.

The fluid thus obtained from typhoid organisms is found to be effective as an immunising and curative agent against typhoid fever when injected into the monkey.

Moreover, it is clear that the luminosity of phosphorescent organisms referred to above is due to a vital process, as the effect is no longer noticeable after disintegrating the organisms at the temperature of liquid air.

THE LIQUEFACTION OF HELIUM.

The liquefaction of helium was effected on July 9th 1908, in the Physical Laboratory of the University of Leiden, by Professor Kammerlingh Onnes, who thereby achieved the conspicuous success his systematic studies of the properties of gases, carried on with great skill and unremitting perseverance during several years past, so richly deserved. In describing his results, Professor Onnes has most generously recognised the debt he is under to the Royal Institution :—

"In the execution I have availed myself of different means which Dewar has taught us to use. I have set forth the great importance of his work in the region of low temperatures in general elsewhere; here, however, I gladly avail myself of the opportunity of pointing out that his ingenious discoveries, the use of silvered vacuum glasses, the liquefaction of hydrogen, the absorption of gases in charcoal at low temperatures, together with the theory of Van der Waals, have had an important share in the liquefaction of helium."

Professor Onnes used helium extracted from Monazite sand, from which the thorium now used so largely in making the mantles for incandescent lighting is prepared. He operated with a quantity of 200 litres (160 litres being held in reserve) but it was necessary to pass this through the condensing circuit twenty times before liquefaction was observed. As showing the magnitude of his operations it may be mentioned that, at the commencement of the experiment, at 5.45 a.m., 75 litres of liquid air were available and that at 1.30 p.m. 20 litres of liquid hydrogen were ready for the final cooling operation. Nothing had been observed when the last flask of liquid hydrogen was connected with the apparatus: liquid helium was just perceived at 7.30 p.m.

In his Presidential Address to the British Association in 1902, Sir James Dewar gave a forecast of the properties of liquid helium based on his studies of the properties of the gas and the application of Van der Waal's doctrine of corresponding states, a generalisation which has played a most important part in his experiments as well as in those of Professor Onnes.

The boiling-point was estimated to be about 5° absolute; he inferred that the liquid would be twice as dense as liquid hydrogen (viz. 0.14), that it would possess a very feeble surface-tension and would be only seventeen times as dense as its vapour; also that it would be quite exceptional in its optical properties and very difficult to see.

Professor Onnes, in point of fact, had difficulty at first in realising that he had succeeded in effecting his purpose: the liquid looked, he says, at if it was almost at its critical temperature—in fact, he speaks of the peculiar appearance of the helium as being best compared with that of a meniscus of carbon dioxide in a Cagniard de la Tour tube. It has exceedingly slight capillarity. The boiling-point is at most 4.5° absolute. Liquid helium has a very low density, viz. 0.15, the ratio of the density of the vapour to that of the liquid being about 11 to 1.

The liquefaction of helium has brought us apparently to within about 3° of the absolute zero of temperature—a result nothing short of marvellous.

The history of Cold and of the Absolute Zero was discussed very fully by Sir James Dewar in his British Association Address.

He pointed out that the production of cold occupied Bacon's

thoughts and that it was made the subject of experimental inquiry by Boyle, who communicated his results to the Royal Society in 1682. Boyle's confession, that he "never handled any part of natural philosophy that was so troublesome and full of hardships," is one which probably is now thoroughly appreciated both at the Royal Institution and at Leiden.

The freezing-point and boiling-point of water were agreed upon as fixed points by the beginning of the eighteenth century. The air thermometer was first brought under notice in 1703-04 by Amontons, a French observer, whose work was not appreciated at the time. Amontons was the first to recognise that the use of air as a thermometric substance led to the inference of the existence of a zero of temperature; the value he arrived at was -240° . More refined observations made by Lambert in 1779 gave the value -270° , which is almost identical with -273° now accepted.

In recent years we have learnt to do almost what we will at temperatures not far removed from this presumed absolute zero. Liquid air is now dealt with as though it were water: and from it oxygen is prepared on a commercial scale by submitting liquid air to fractional distillation, this method having superseded all others.

A long series of researches has been required to advance our command over matter at low temperatures to its present remarkable state of perfection. When the history of the subject comes to be dealt with, it will be difficult to over-estimate the importance of the contributions made from the laboratory of the Royal Institution.

THE FUTURE OF SCIENTIFIC RESEARCH AT THE ROYAL INSTITUTION.

Faraday, in 1813, in describing his work as chemical assistant under Sir H. Davy, spoke of himself as "constantly engaged in observing the works of Nature and tracing the manner in which she directs the arrangement and order of the world."

It may be surmised, that in giving the sum of 100,000 dollars to the Royal Institution to provide it with further means for the investigation of the relations and co-relations existing between man and his Creator, Mr. Thomas G. Hodgkins was mindful of the position in the history of scientific discovery which the Institution can claim to occupy, and was in some measure aware that it has contributed, through the work done by its professors and in its laboratories, in an altogether remarkable manner, to our knowledge of natural forces.

Nowhere else can the feeling arise in the same way in the mind of the scientific worker of being in a Holy of Holies: but how many have such a feeling? The vast import of the discoveries made within the narrow precincts of the Institution is realised probably only by very few.

Within its walls, just a century ago, Davy made the first real use of electricity as an analytical agent and discovered the alkali metals, potassium and sodium—not by any chance act, but as the result of the most careful deductive reasoning. Not content with this cardinal achievement, he subsequently demonstrated the elementary nature of chlorine and later on that of iodine, determining the properties of the latter with incredible swiftness and acumen; to crown all, he invented the safety-lamp, at the same time making no inconsiderable contribution to the theory of combustion.

Faraday—who from being a bookbinder's apprentice straightway became chemical assistant to Davy, entering at once on the path of inquiry as though to the manner born—although at first a chemist, in the course of his life, by a marvellous intuitive process, acquired mastery of the cognate subject electricity, and practically created the science. The pioneer systematic worker on the liquefaction of gases, he was also the discoverer of benzene among the products of the decomposition of oil at a red heat—the great lawgiver who defined the conditions which determine electrolytic and chemical change—the originator of new fundamental conceptions in electrical science too numerous to mention, but such that the Institution may claim to be the focus-point of the marvellous developments of pure and applied electricity witnessed during the past few decades.

That electricity owes much to the Royal Institution is generally admitted, but that the foundations of the coal-tar colour industry, with all its marvellous ramifications, as well as of a dominant section of organic chemistry—that comprising the host of descendants to which benzene has given rise—are sprung from the same Minerva head is less commonly understood; and it is yet to be recognised that the foundations of chemical belief—the conditions of chemical change—were laid down in the marvellous fifth, sixth, seventh and eighth series of Faraday's researches in electricity.

Faraday's was too simple a nature; modern conditions require more pretentious treatment than that which he inclined to adopt; his modest presentation of the facts seems no longer to catch the attention: perhaps a new generation will be more sympathetic and exploit the stores of wisdom which still lie untouched in his memoirs, when it has learnt to appreciate the spirit in which he worked—the reverent and philosophical manner in which he conducted his inquiries.

The birthplace of discoveries and inventions which have revolutionised our civilisation, it may be said of the work accomplished in the Institution that it has been the labour of the highest genius—nowhere else, indeed, can the worth of genius to the world be so clearly demonstrated. Davy and Faraday shine forth in its history and in the history of science as stars of the first magnitude—yet both came to their work untrained, with minds uninfluenced by dogmatic teaching. At the present day it is worth our while to remember this,

and to ask ourselves whether genius be not too often maimed and not made by "education." Taking into account the large number of workers now engaged in scientific inquiry, the comparatively low average of quality attained to in the output is somewhat surprising; the majority appear to lack not only originality and breadth, but also the critical faculty and that sense of proportion which is so eminently characteristic of Faraday's writings. Some occult influence is at work tending to depress the value of human effort; the distance between those who are really pioneer workers and the general body appears to be widening, not diminishing, as it should be if effective means were being taken to inform and instruct the public.

It would almost seem that, as Wordsworth has it—

. . . for everything we are out of tune;

It moves us not . . .

in the sense, that we are not yet attuned to the complexities of modern knowledge; that in our attempt to grasp facts we lose sight of relations and co-relations; and that men like Mr. Hodgkins, looking on from a distance, have seen this and desired to assist in bringing about a better understanding.

His history appears to have been a remarkable one. Born in this country, he emigrated while young to America and established a confectionery business in New York; having made a fortune, he retired when about sixty years old and took to farming on the coast of Long Island. He died in December 1892, shortly after he had transferred to the Royal Institution the sum of 100,000 dollars United States currency, the income whereof was to be devoted (at his demise) to the purposes of scientific investigation. He was also the donor of 40,000*l.* to the Smithsonian Institution at Washington.

It would be interesting if we knew how he was moved to take so great an interest in science. Liebig, who visited Faraday in 1844, wrote to him on his return to Germany:—

"What struck me most in England was the perception that only those works which have a practical tendency awake attention and command respect; while the purely scientific, which possess far greater merit, are almost unknown. And yet the latter are the proper and true source from which the others flow. Practice alone can never lead to the discovery of a truth or a principle. In Germany it is quite the contrary. Here, in the eyes of scientific men, no value or at least but a trifling one is placed on the practical results. The enrichment of science is alone considered worthy of attention. I do not mean to say that this is better; for both nations the *golden medium* would certainly be a real good fortune."

Very largely owing to his influence and more particularly in consequence of the introduction into the University system of experimental research work such as Liebig first made possible by

training competent teachers in his laboratory at Giessen, Germany long ago changed her attitude and soon attained to the "golden medium;" the result has been that she now holds a leading position both as a cultured and as an industrial nation. But although inspired primarily by Liebig, German success cannot be regarded otherwise than as resting largely upon Faraday's work—especially upon his discovery of benzene, the study of which has profited both science and industry in a manner without parallel in any other branch of natural knowledge.

Here it is still true that only those works which have a practical tendency or sensational discoveries awaken attention and command respect; the purely scientific achievements are almost unknown. In fact, the Royal Institution is still the only establishment within whose audience chamber due public recognition is accorded to scientific work.

In giving evidence before the Public School Commission in 1862, Faraday made the weighty statement:—

"that the natural knowledge which has been given to the world during the last fifty years, I may say, should remain untouched and that no sufficient attempt should be made to convey it to the young mind growing up and obtaining its first views of these things is to me a matter so strange that I find it difficult to understand; though I think I see the opposition breaking away, it is yet a very hard one to be overcome. That it ought to be overcome I have not the least doubt in the world."

Now, nearly fifty years later, we are more than ever constrained to admit that the opposition is a very hard one to be overcome; it is breaking away perhaps a little less slowly than in Faraday's time, but much too slowly in view of past delay and our urgent needs. And the outlook at present is far from encouraging.

"Science," it is true, has been introduced into a large number of schools, but in too many cases, it is to be feared, the teaching is of a more or less perfunctory character and both poor as discipline and barren of interest. The Universities have not yet thought it well to treat training in scientific method as a necessary element of preliminary education. And at the Universities themselves mere knowledge has been cultivated at the expense of appreciation and the power of applying knowledge. Even scientific workers seem rarely to have been actuated by the spirit of self-abnegation in the public interest, having done little to overcome the resistance which the conservative elements of society oppose to progress. The effect of scientific training and work in broadening sympathies has been strangely disappointing in this respect. But Faraday appears to have foreseen that such would be the case. One of the most striking addresses he delivered of which we have cognizance was that on the "Inertia of the Mind," written in his youthful days: in this he

draws a parallel between the inertia of the mind and the inertia of matter :* the dual state of mind which he pictures is still with us and doubtless will ever remain : it is at bottom both the cause of our difficulty and our hope. In fact, in saying that—

“the man who has once turned his mind to an art goes on more and more improving in it; the man who once begins to observe rapidly improves in the faculty . . . every little delay illustrates more or less the inertia of the passive mind; every new observation, every great discovery, that of the active mind,”

Faraday has himself pointed the way of improvement.

In 1854, when lecturing on Mental Education before His Royal Highness the Prince Consort, he drew attention to what appeared to him to be the great deficiency in the exercise of the mental powers in every direction. These words he said would express it, “*deficiency of judgment.*”

“I know,” he added, “that multitudes are ready to draw conclusions who have little or no power of judgment in the cases; that the same is true of other departments of knowledge; and that generally mankind is willing to leave the faculties which relate to judgment almost entirely uneducated and their decisions at the mercy of ignorance, prepossessions, the passions or even accident.”

He laid down what a man may and ought to do for himself in the following words :—

“It is necessary that a man *examine himself* and *that* not carelessly. On the contrary, as he advances, he should become more and more strict, till he ultimately prove a sharper critic to himself than anyone else can be; and he ought to intend this, for so far as he consciously falls short of it he acknowledges that others may have reason on their side when they criticise him. A first result of this habit of mind will be an internal conviction of *ignorance in many things respecting which his neighbours are taught* and that his opinions and conclusions on such matters ought to be advanced with reservation. A mind so disciplined will be open to correction upon good grounds in all things—even to those it is best acquainted with and should familiarise itself with the idea of such being the case: for though it sees no reason to suppose itself in error, yet the possibility exists. The mind is not enfeebled by this internal admission but strengthened: for if it cannot distinguish proportionately between the probable right and wrong of things known imperfectly, it will tend either to be rash or hesitate; whilst that which admits the due amount of probability is likely to be justified in the end. It is right that we should stand by and act on our principles but not right to hold them in obstinate blindness or retain them when proved to be erroneous.”

It is a sad reflection that so little attention has been paid to the lament to which he gave utterance at the conclusion of his address :—

* Life of Faraday, by Bence Jones, vol. i. p. 261.

"It is an extraordinary thing, that man with a mind so wonderful that there is nothing to compare with it elsewhere in the known creation should leave it to run wild in respect of its highest elements and qualities. He has powers of comparison and judgment by which his final resolves and all those acts of his material system which distinguish him from the brutes are guided :—shall he omit to educate and improve them when education can do much? Is it towards the very principles and privileges that distinguish him above other creatures, he should feel indifference? Because the education is internal, it is not the less needful; nor is it more the duty of a man that he should cause his child to be taught than that he should teach himself. Indolence may tempt him to neglect the self-examination and experience which form his school and weariness may induce the evasion of the necessary practices but surely a thought of the prize should suffice to stimulate him to the requisite exertion; and to those who reflect upon the many hours and days devoted by a lover of sweet sounds to gain a moderate facility upon a mere mechanical instrument, it ought to bring a correcting blush of shame, if they feel convicted of neglecting the beautiful living instrument wherein play all the powers of the mind."

The two essays to which I have referred—that written in 1823, almost at the beginning of his career and the second written thirty years later towards the close of his wonderful activity, during which period Faraday had worked with a logical clearness of purpose and a degree of constancy and consistency almost unparalleled in history—embody a complete doctrine of education: but they remain practically unheeded.

Yet such matters are of consequence, especially to the Royal Institution. Nowhere else has Faraday's doctrine been exemplified more often or more successfully. Surely some effort should be made to bring home to a larger public the mine of wealth which has been opened out by its officers, and to render it of permanent avail in the service of our nation. It needs members and it needs means: the great work done by the Institution in the past and that which, if only properly supported, it is obviously destined by its past to accomplish in the future should be more clearly and generally recognised; something more should be done to overcome public apathy to the progress of science.

It cannot but be supposed that if the interest attaching to scientific discovery were appreciated by the educated classes at large there would be a strong general desire both to be improved and to promote and subsidise inquiry. And workers should be attracted. It was, I believe, in the expectation that the attention which, he supposed, was being given in the public schools to science would lead not a few, who had both leisure and means, to desire to continue their studies and eventually devote themselves to research work, that Dr. Mond was led to establish the Davy-Faraday laboratory. Unfortunately, his expectation has in no way been justified.

In point of fact, the scientific amateur, who has been the glory of our country in the past, seems to be in danger of submergence. Some other influence than mere inertia is clearly at work, affecting intelligence and actively degrading if not destroying it. There can be little doubt that this is to be sought in two directions: in our continued belief in classical study as an effective means of education and in our insensate worship of examinations. The combined effect of these two influences is undoubtedly to develop the passive habit of mind and the belief in precedent—to cultivate the worst form of mental inertia.

The case was stated very clearly to the members of the Institution on January 31, 1868, in a Friday evening discourse, by the Rev. F. W. Farrar, M.A., F.R.S., then a classical master in Harrow School, afterwards Dean Farrar. His words are so pregnant with meaning that I feel impelled to quote them:—

“So far from being half finished, the real battle for educational reform has hardly begun. Latin and Greek still continue to be the all but exclusive staple of our education and though a classical training conducted on wise principles and with reasonable methods is of the highest value, yet the many and serious evils which our present system of it involves have been resolutely ignored. The yoke of the Greek and Latin languages have been made needlessly humiliating and needlessly heavy; taken alone, it is doubtful whether they furnish the best mental discipline for any but certain that they do not furnish even a good discipline for all; and they remain to this day entrenched behind a mountain-heap of fallacies of which no small number ought to have been banished ignominiously to the region of the most exploded errors.

“But even if all the arguments in favour of a purely classical education were as tenable as half of them are fantastic, our present system of it is a complete and disastrous failure; and that it is so may be largely demonstrated alike by the criticism of its enemies and the repeated confessions of its friends. And if this be so, it is our clear duty as Englishmen, as patriots, nay even as mere honest men, to make that system more worthy of its immense importance and of our national prestige.

“It would be easy to adduce the testimony of many eminent scholars to the humiliating ignorance on a multitude of subjects which has been the inevitable result of years exclusively devoted to two dead languages; but the case of the vast majority of boys who do not become scholars in any sense of the word is still more to be deplored. People read glowing estimates of Greek and Roman literature and take them for a defence of classical education. There could not be a greater delusion. Hundreds of boys after years of expensive training know far less and have far less culture than their sisters who have only had the modest aid of a single governess. They know nothing except, perhaps, the merest and most useless smattering of modern languages, of history, of mathematics or of science; and if they want to pass in a competitive examination they must be hastily sent to some professional tutor to have their minds crammed for the purpose like a hurriedly-packed portmanteau. The parent comforts himself that their education has been purely literary,

but this purely literary education has somehow left them with a bad handwriting, with very vague notions of spelling and with minds that can find satisfaction in nothing higher than sensational novels. The parent takes refuge in the belief that at least their boys know Latin and Greek; but this is infinitely far from being the case; of the vocabulary they possibly know a little but of the grammar less and of the literature nothing at all. It is certain that they will never open a Greek or Latin book again; and for these paltry and miserable results they have all but sacrificed the happy seed time during which so much might have been accomplished. The evidence of these facts, evidence given by most friendly witnesses, stands in the Commission Reports undisputed and undisputable. It shows that for many a boy the years of school life are wasted. It is as though he stood in the middle of a boundless plain, waving on every side with golden corn, in the midst of which, trained to despise the sickle as vulgar and the harvest as utilitarian, he had been taught for years to occupy his time in plucking a few petals of the scarlet poppies, which are crumpled as he gathers them and which grow rank and flaccid even during the few moments he holds them in his hand.

"The question then is, not whether the education is to be literary or scientific but whether it is to be scientific or nil; the struggle is not between science and literature but between something and nothing, between science and no science, between intellectual culture and its almost total absence. It is a melancholy fact but it is a fact, that at present we struggle almost in vain against the two potent elements of intellectual progress—extravagant athleticism on the one hand and promiscuous sensation reading on the other, of which the one poisons and effeminates the mind, the other often tasks and overstrains the body; the one absorbs the strenuous ambition which might have been devoted to nobler objects, the other wastes the inestimable leisure which might else have been rich in mental and moral benefits for our country and for mankind.

"What, then, is to be done? Some would say, 'Substitute for your simulacrum of Greek and Latin an education which, if less pretentious, shall at least be real and sound, in modern languages, in literature and, above all, in science.' But it would be a great disaster if there were supposed to be any antagonism between science and literature—both are indispensable, each of them is an absolutely essential factor in an education pretending to be liberal. Yet our present system is neither literary nor scientific, whereas it is perfectly certain that it might be both."

Those who are conversant with our educational system are only too well aware that the indictment laid by the late Dean forty years ago accurately represents the position at the present day. Our "system" is still neither literary nor scientific—the fact is, we have no system: yet we have experience enough to frame one, if we could only agree to work together and to utilise our knowledge—if we could only establish the necessary organisation. Our present tendency, however, is to cast experience to the winds.

"The Greeks were themselves illiterate," said the Rev. Mr. Farrar; "they knew little of *words*, but they made up for it by *thought*—by that power of deep reflection which makes facts luminous with

meaning." We teach *words* in our schools but not *thought*. School education is a bane rather than a blessing. In consequence of lack of training in method, even scientific workers, for the most part, are content to be advocates, and too many make no attempt to exercise judicial functions or to follow in the wake of Faraday and Darwin by striving to be philosophical.

The origin of our difficulty, perhaps, is not far to seek—indeed, it is practically certain that it lies in the circumstance that our country is dominated by the literary and not by the practical type of mind, by men of narrow purview. This is especially true of our ancient Universities, by whom all affairs educational are controlled.

There can be little doubt that until the literati are deposed from their position of almost exclusive control general progress will be impossible: they will never see eye to eye with those trained on the scientific side, not from any unwillingness or ill-feeling but from actual inability to understand and appreciate their work and aspirations: the methods of the scientific worker make no appeal to the literary student: he has no conception of an experiment, and even results rarely have any significance in his eyes. Unless, therefore, it can be recognised by a preponderant party that a change must be made in the attitude of our schools and that change be enforced, nothing effective will be done. We need to put men in control who are gifted with broader sympathies than those can be who have been selected on the narrow basis of a limited literary proficiency.

In an article by Carl Snyder entitled "America's Inferior Place in the Scientific World," published in the January, 1902, number of the *North American Review*, the statement is made that, "It would be hardly too much to say that during the hundred years of its existence the Royal Institution alone has done more for English science than all of the English Universities put together. This is certainly true with regard to British industry, for it was here that the discoveries of Faraday were made."

Sir James Dewar was led by this to inquire into the total expenditure of the Institution on experimental inquiry and public demonstrations during the whole of the nineteenth century, and in his Presidential Address at the British Association at Belfast in 1902, he published the following statement of the items:—

	£
Professors' Salaries—Physics and Chemistry ..	54,600
Laboratory Expenditure	24,430
Assistants' Salaries	21,590
Total for one hundred years ..	<u>£100,620</u>

In addition, he said, the members and friends of the Institution have contributed to a fund for exceptional expenditure for Experimental Research the sum of £9580. It should also be mentioned that a

Civil List pension of £300 was granted to Faraday in 1853 and was continued during twenty-seven years of active work and five years of retirement. Thirty-two years in all, at £300 a year, make a sum of £9600, representing the national donation; added to the amount of expenditure just stated, this brings up the total cost of a century of scientific work in the laboratories of the Royal Institution, together with public demonstrations, to £119,800; an average of £1200 per annum.

Such expenditure has only been made possible by frequent donations from members and friends of the Institution.

Up to the present time, the work has been done by men who have been prominent as philosophers; the problem of the future will be to maintain continuity with the past, which must prove very difficult in face of an ever-growing outside competition and the increasing cost of experimental inquiry.

Future workers should not only have the necessary working space and adequate means of meeting the expenses of the position and of all inquiries which it may appear desirable to undertake; they will also need the assistance and society of competent associate workers. It should not be forgotten that the Institution will probably afford the one haven of rest open to an inquirer in our country, where original work can be done under proper conditions, undisturbed by outside influences. In all our Colleges the burden of teaching now cast upon the Professoriate is such as to render the task of research very difficult, the necessary leisure for quiet reflection and study being secured, if at all, with utmost difficulty; and student assistants are no sooner trained to the point of efficiency than they are tempted away to some remunerative post, usually before they have developed sufficient judgment and independence to become original inquirers themselves.

It is to be hoped that in the near future a sufficient staff of student assistants may be at the disposal of the Professor in charge of the Institution laboratories to aid him in the promotion of inquiry, by association with whom he will be able also to secure scientific companionship and the inspiration which naturally springs from intercourse with active youthful minds anxious to exploit the genius of their teacher. It is more than unfortunate that such assistance has not been at the disposal of the Professor of late years, both to enable him to utilise more fully the invaluable experience which he has accumulated and to have preserved this for the use of future workers.

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- 1900 "On the Spectrum of the more Volatile Gases of Atmospheric Air which are not condensed at the Temperature of Liquid Hydrogen." (With Professor Liveing.) Proc. Roy. Soc. vol. lxvii. p. 467; *Annales de Chimie*, 7 s. vol. xxii. p. 482.
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- 1902 "Coefficients of Cubical Expansion of Ice, Hydrated Salts, Solid Carbonic Acid, and other Substances at Low Temperatures." Proc. Roy. Soc. vol. lxx. p. 237; *Nature*, vol. lxvi. p. 88; *Chemical News*, vol. lxxxv. pp. 277, 289.
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- 1903 "Sur la Solidification du fluor et sur la Combinaison à -252° , 5 du Fluor solide et de l'Hydrogène liquide." (With Professor H. Moissan.) *Comptes Rendus*, vol. cxxxvi. p. 641; *Chemical News*, vol. lxxxvii. p. 178; *Nature*, vol. lxvii. p. 497; *Pharmaceutical Journal*, 4 s. vol. xvi. p. 671.
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- 1904 "Effect of Liquid Air Temperatures on the Mechanical Properties of Steel and its Alloys." (With Mr. R. A. Hadfield.) Proc. Roy. Soc. vol. lxxiv. p. 326; *Chemical News*, vol. xci. p. 13; *Annales de Chimie*, 8 s. vol. iv. p. 556.
- 1904 "Absorption and Thermal Evolution of Gases occluded in Charcoal at Low Temperatures." Proc. Roy. Soc. vol. lxxiv. p. 122; *Annales de Chimie*, 8 s. vol. iii. p. 5; Comptes Rendus, vol. cxxxix. p. 261.
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- 1906 "Liquid Air and Charcoal at Low Temperatures." Proc. Roy. Inst. vol. xviii. p. 433; *Engineering*, June 15. (With Summary of Work.)
- 1907 "Studies in High Vacua and Helium at Low Temperatures." Proc. Roy. Inst. vol. xviii.; *Engineering*, June 14.
- 1907 "Notes on the Use of the Radiometer in observing small Gas Pressures; Application to the Detection of the Gaseous Products Produced by Radio-active Bodies." Proc. Roy. Soc. vol. lxxix. A. p. 529; *Chemical News*, vol. xcvi. p. 97; Comptes Rendus, vol. cxlv. p. 110.
- 1908 "Notes on the Application of Low Temperatures to some Chemical Problems: (1) Use of Charcoal in Vapour Density Determination; (2) Rotatory Power of Organic Substances." (With Dr. H. Owen Jones.) Proc. Roy. Soc. vol. lxxx. A. p. 229.
- 1908 "Rate of Production of Helium from Radium." Proc. Roy. Soc., vol. lxxxi. A. p. 280; *Chemical News*, vol. xcvi. p. 188.
- 1908 "The Nadir of Temperature and Allied Problems." Proc. Roy. Inst. vol. xix.; *Engineering*, June 12.

WEEKLY EVENING MEETING,

Friday, June 5, 1908.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L.
LL.D. F.R.S., President, in the Chair.

PROFESSOR SIR JAMES DEWAR, M.A. LL.D. D.Sc. F.R.S. *M.R.I.*,
Fullerian Professor of Chemistry R.I.

The Nadir of Temperature and Allied Problems.

[ABSTRACT.]

MANY years ago the zenith of temperature and the intensity of solar heat were favourite subjects of mine, but since that time my investigations have led me to attack problems related to the nadir of temperature, and some of these I propose to consider in this discourse. Our absolute scale of temperature depends on gas thermometry; but for demonstration purposes thermojunctions coupled with reflecting galvanometers are more convenient.

On lowering a thermojunction into a vessel of liquid air, the galvanometer you observe registers 88° on the scale fixed on the wall in degrees absolute. On blowing air through the liquid scarcely any disturbance of the galvanometer is produced; when, however, hydrogen is bubbled through the liquid air, the galvanometer at once shows a lowering of temperature amounting to about 5° . On exhausting the liquid air, the temperature quickly falls and 70° absolute is registered, and in a short time the limit of 65° is reached, while the liquid air is boiling under a pressure of about 2 cm. mercury pressure.

If the liquid air is now replaced by liquid hydrogen, a temperature of 20° absolute is registered by the thermojunction, this being about the boiling point of the liquid. When the liquid hydrogen is rapidly exhausted, a fall is indicated to 14° or 15° absolute. The hydrogen has now solidified under a pressure of about 55 mm. Continuing the exhaustion on the solid hydrogen a temperature of $13\frac{1}{2}^{\circ}$ is indicated. On admitting pure hydrogen at atmospheric pressure to the vacuum vessel, the solid hydrogen melts, and a temperature of 20° is once more registered.

The temperature of 13° absolute is the practical nadir; or the lowest temperature we can conveniently permanently maintain by the use of solid hydrogen.

Now hydrogen boils at 20° and air at 88° , which is more than four times as high. The temperature of the lecture room is about 300° absolute, that is fifteen times warmer than boiling hydrogen. Fifteen times higher than 300° gives a temperature of 4500° , which is nearly the temperature of the sun. We thus produce in the

laboratory a degree of cold relative to the average temperature which is exactly comparable to the intensely high temperature of the electric furnace or even that of the sun.

When liquid air is dropped on to the surface of fluids at the ordinary temperature, the liquid air at once assumes the spheroidal state, and rushes about on the warmer liquid which has a temperature three times higher. Projected by the lantern the liquid air drops look like tadpoles or nuclei with cometary tails formed of the condensing mist of the vapour of the warmer liquid. This experiment

was first shown some two years ago. If, now, liquid hydrogen is poured on to the surface of liquid air, the temperature ratio of the liquids being as great as 1 to 4, we anticipate the liquid hydrogen would equally float about in the spheroidal state. The experiment is quite successful although somewhat less striking than that of liquid air on the surface of carbon tetrachloride, because it all passes in a few seconds owing to the small latent heat of liquid hydrogen, although 10 c.c. of liquid hydrogen is used in each operation. The boiling liquid air surface is clouded over when the liquid hydrogen is poured on to it, owing to the formation of a snow of solid air, and all boiling of the liquid air is arrested. The rapidity of the passage of the phenomena is suggestive to some extent of the difficulties which surround experimental work when attempting to approach the nadir of temperature.

As an illustration of the importance of questions of radiation in low temperature research, two vacuum-jacketed vessels, alike in all respects except that one is coated inside with silver and the other with a film of highly reflecting lead sulphide (Fig. 1), are connected to the same liquid-air cooled charcoal exhaust, thereby ensuring and maintaining an identical high vacuum in each; and both afterwards charged with the same volume of liquid air. The air is allowed to distil from each vessel, and the rates of distillation compared. It takes thirty-five seconds for the silvered vessel to boil off a volume of air measured at the ordinary temperature of 50 c.c., while the lead-sulphide

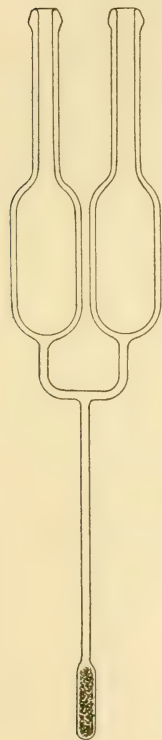


FIG. 1.

coated vessel only takes seven seconds to evaporate a similar amount. Thus the influx of heat from the sulphide of lead coated vessel is just five times greater than that entering by the silver covered surface. A film of nickel deposited from Mond's Nickel carbonyl is nearly as good as silver. Of course the influx of heat is not merely a question of radiation, but also involves gas convection; which again

depends on the perfection of the vacuum ; as well as heat conduction down the necks of the vessels.

All the oxygen used in London is now obtained from the fractionation of liquid air by the Linde method, and consequently is much purer than the old commercial oxygen, while substantially pure nitrogen can be obtained at the same time if wanted, along with the rare gases.

The cooling of air by adiabatic expansion is easily shown with an apparatus of the old Cailletet type, a thermojunction in the expanding air is connected to the reflecting galvanometer which shows the alterations of temperature on the scale fixed on the wall. The gas is compressed by the pump, cooled to absorb the heat of compression, and then suddenly expanded. Wroblewski in 1888 first saw a mist of hydrogen produced by this instantaneous method of cooling. With helium, I in 1901, and Olszewski, of Cracow, in 1905, both failed to observe any condensation by adiabatic expansion. The following table summarises the results of these experiments :—

ADIABATIC EXPANSION OF HELIUM AT TEMPERATURE OF
SOLID HYDROGEN.

	Initial Pressure. Atmospheres.	Final Pressure. Atmospheres.	Temperature. Degrees Cent. Absolute.	
			Olszewski, 1905.	Dewar, 1901.
Olszewski . . .	180	40	7·6	11·7
Dewar . . .	80 to 100	20	5·8	8·9
„	10	4·4	6·7
„	5	3·3	5·1
„	1	1·7	2·7

Thus Olszewski thought he had reduced his temperature down to 1·7 degree above absolute zero, yet neither he nor I saw any indications of liquefaction. Olszewski, in fact, seemed to regard helium as possibly non-condensable, and such an inference, if true, would be an important addition to our knowledge. There are, however, several facts that would help to account for the failures to observe any condensation. The refractive index of helium and its latent heat are both very small, the latter probably only one-seventh that of hydrogen, so that it would be very difficult to see and isolate as a mist. When apparatus was arranged for helium circulation, similar to that used in the liquefaction of hydrogen, two difficulties arose, viz. the use of a sufficient amount of pure helium gas, and the supply of enough liquid hydrogen to maintain the cooling until regenerative action on the helium began. For the rapid liquefaction of hydrogen it is necessary to start from a temperature of 65°, and regenerate down to 20°. Assuming the boiling point of helium to

be about 5° (to which several observations of my own point) we must start from 15° to have the same temperature fall of 3 to 1. But that would require a large and elaborate plant, and with the mass of gases in circulation and the time required, the fight against any influx of heat from the outside becomes harder.

There is no intermediate fixed point between 65° and 20° , so that a descent of 15° has to be effected by regenerative methods in the liquefaction of hydrogen, and a similar rate of descent would have to be achieved before helium could be liquefied. In order to keep the circulation of helium up for four minutes in my apparatus, about 6 litres of hydrogen disappeared, and during all the time of the operation the impurities in the gas were accumulating at the valves.

There are other difficulties to be overcome before the apparatus could be worked with success. Hydrogen is cheap enough, helium very costly. For years our supply has come from the King's Well at Bath, which gives off a gas (largely nitrogen) containing $\frac{1}{2000}$ of its volume of impure helium. We have to deal with very large volumes of gas, and after years of work I lost all my treasured store of helium. We know now that helium is more common than we had at first thought. Our atmosphere contains $\frac{1}{250000}$ of helium, and in several springs it is more abundant than at Bath. The gases given off by certain springs in France contain more than 2 per cent. of helium. A similar amount has also been found in the natural gas of a North American town. With 100 or 200 litres of helium at the experimenter's disposal, it would be easy to prove the success or failure of the regenerative method.*

For the present I have made further experiments on helium absorption by charcoal. With helium, the absorption only begins at the lowest realisable temperatures, but when we come to the boiling-point of hydrogen the charcoal absorbs 200 times its volume of helium, and the curve shows that helium at its own boiling-point of about 5° would most probably be absorbed to the same extent as hydrogen at its boiling-point. The absorption of gases by charcoal was accompanied by evolution of heat shown in the following table :

HEAT EVOLUTION DETERMINED BY CHANGE OF GAS TENSIONS AT DIFFERENT TEMPERATURES (CONCENTRATION CONSTANT) IN EACH EXPERIMENT.

	Molecular Latent Heat.	Temperature Absolute. Deg. Cent.
Helium . . .	4.6×105	18
Hydrogen . . .	4.6×114	18
„ . . .	4.6×436	78
Nitrogen . . .	4.6×665	82
Oxygen . . .	4.6×684	82
Carbon dioxide .	4.6×1326	180

* Helium was liquefied by Professor Dr. Kamerlingh Onnes, of Leiden University, on July 9, 1908.

The direct value of the thermal evolution for hydrogen, about 85° , measured in the liquid air calorimeter described in last year's discourse, was found to be 4.6×426 .

I showed in the last discourse the high exhaustions obtainable by the use of charcoal at low temperatures. The condensing action is still effective against considerable dynamic heats.

The following experiments illustrate this :

A very thin india-rubber membrane is stretched across the open end of a piece of glass tube 1 inch wide (being securely fixed by the use of melted rubber and thread to the outside wall of the tube) and connected to a discharge tube and a bulb containing charcoal which

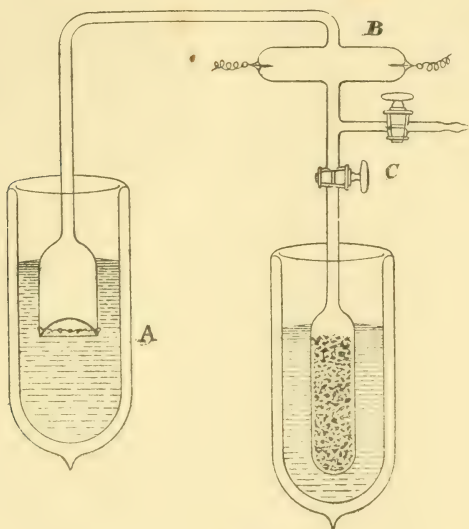


FIG. 2.

can be shut off by a stopcock (see A, B and C, Fig. 2). The apparatus is exhausted down to a few mm. pressure, thus forcing the rubber to be drawn out to a spherical shape, and the end of the tube is immersed in liquid air. A discharge is passed and the charcoal, already immersed in liquid air, is brought into play to increase the vacuum. The rate of diffusion through the membrane is so greatly diminished, that a vacuum is very soon produced by the charcoal which is so high that no discharge will pass. If the liquid air is taken away from A, a continuous discharge takes place in B owing to rapid diffusion through the rubber film at the ordinary temperature.

Measures on the rates of leakage across such membranes for a given surface in a given time were carried out by observing the increase of pressure in a McLeod gauge. The values are given in terms of the number of cubic millimetres of gas which passes per minute across an area of 1 square centimetre. Both air and hydrogen diffusion was examined. At ordinary temperatures the figures obtained were approximately 1.5 in air and 4.9 in hydrogen. At -80°C . the corresponding values were 0.0009 and 0.0044, while at -185°

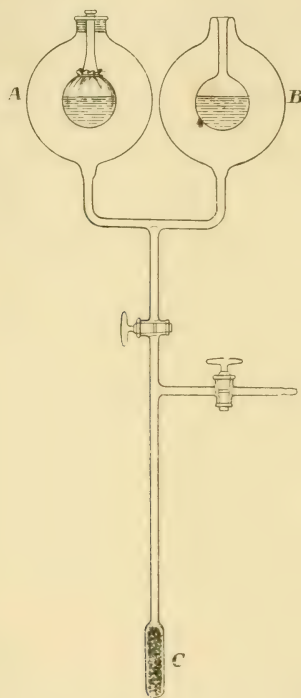


FIG. 3.

the figures were 0.00012 and 0.00013 respectively. One membrane was also tested while immersed in liquid hydrogen and was found to be quite tight.

An india-rubber vacuum vessel can be formed on a similar principle. Fig. 3 shows two little balloons, one of glass B, the other of a thin india-rubber membrane A, both within larger bulbs, which are connected together, the space within being maintained exhausted

by a charcoal bulb C. The vacuum so obtained is such that both little bulbs will hold liquid air without appreciable ebullition.

RELATIVE VISCOSITY OF AIR AT THE ORDINARY AND LIQUID AIR TEMPERATURES.

Referring to Fig. 4, A is a U-shaped quill tube filled with cotton wool and connected by stopcock D to B, which is a very fine capillary tube, drawn out in the blowpipe. C is an ordinary aluminium electrode

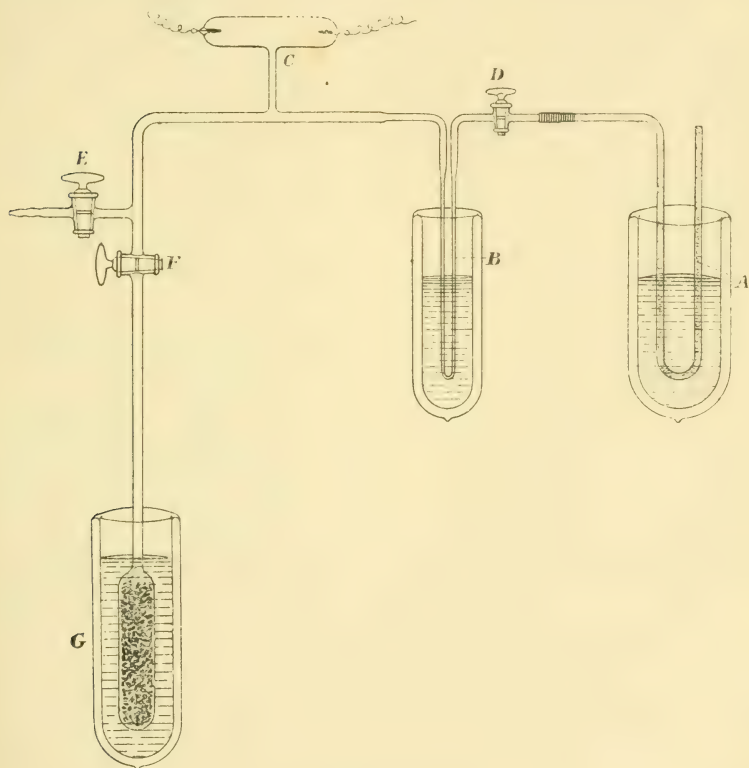


FIG. 4.

sparkling tube, used as manometer for indicating pressure in the apparatus by the alteration in the appearance of the electric discharge. E and F are stopcocks for exhausting the apparatus and connecting it to the charcoal condenser G. A is kept immersed in liquid air to

purify the entering gases. After the charcoal condenser is cooled, the vacuum in C becomes so high that the coil discharges across a 1-inch spark gap outside rather than through C. The stopcock D is turned to give a charge of dry air to fill the quill tube between D and the capillary B. The conductivity of C now becomes high, the outside spark gap stops acting, and the discharge again passes in C. The vacuum, however, rapidly becomes high until C begins to show a good glass phosphorescence. B is now cooled in liquid air, when a rapid leak of gas through B into C becomes apparent by the red glow of nitrogen round the positive pole through the viscosity of the air being reduced by the cooling, thus increasing the amount of leakage.

The converse experiment can be shown thus: when the vacuum is not low, e.g. just after turning D to admit a charge of air, we heat B with a spirit-lamp. The resistance in C now rapidly rises until sparks pass across the outside gap due to the high exhaust in C. When air is replaced by dried hydrogen the same phenomena can be shown.

The expression for the viscosity μ of a gas at absolute temperature T is given by $\mu = nT^a$ where n and a are constants. Rayleigh's value of a for air = 0.75, so that a change from 15° C. to -183° C. would diminish the viscosity $2\frac{1}{2}$ times in any case. A series of quantitative experiments must be undertaken on the viscosity of hydrogen down to 20° absolute.

INCANDESCENCE OF A THORIA MANTLE PRODUCED BY CROOKES RAYS IN A CHARCOAL VACUUM.

A small piece of an ordinary gas mantle about 1 cm. square is supported by platinum wire near the focus of the concave aluminium cathode of a vacuum tube 15 cm. in length and 6 cm. diameter (Fig. 5). The platinum wire carrying the piece of Thoria mantle is fused into a glass rod B which can be pushed into any position by means of the indiarubber joint fitting B. As gases diffuse through the rubber tube at B a slight leak is maintained into the vacuum tube. The final exhaust is controlled by means of a side tube provided with a stop-cock and bulb containing 20 grm. of charcoal (G) placed in liquid air.

Starting with a few mm. pressure in the tube, the cooled charcoal is put into action by opening the stopcock. The pressure consequently falls, as indicated by the discharge. In a short time a dull red spot shows in the centre of the square of the mantle; this rapidly increases in intensity until a brilliant incandescence is obtained. Very soon however, as the pressure continues to fall by the action of the cooled charcoal, the incandescence again diminishes to a dull red spot and soon goes right out.

Now by closing the charcoal cock, the slight leak round the indiarubber joint causes the pressure again to rise in the tube and the

incandescence again establishes itself. The pressure still increasing, the incandescence again dies down and disappears. On opening the cock to the charcoal the same phenomena are again repeated.

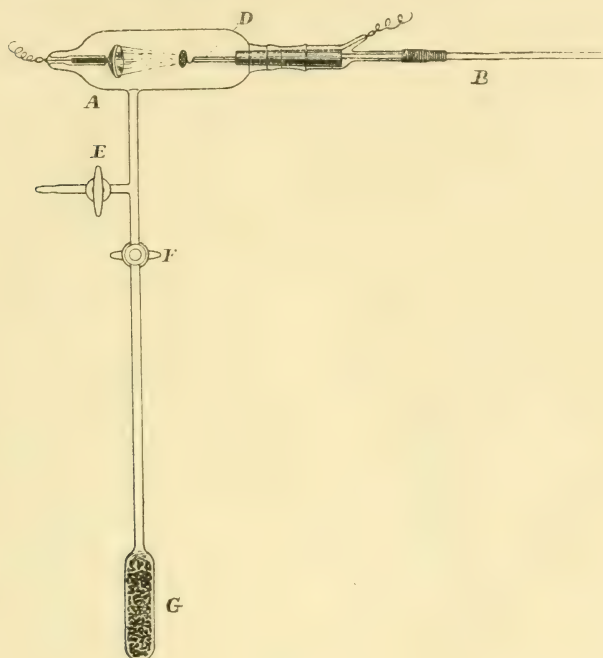


FIG. 5.

The limits of pressure, as measured by a McLeod gauge, within which this incandescence occurs range from 0.03 mm. to 0.003 mm.

SOLID HYDROGEN BY CHARCOAL EXHAUST.

A liquid may be solidified by the aid of a charcoal condenser maintained at four times its absolute boiling point. A vacuum tube (Fig. 6) containing liquid hydrogen A is surrounded by another vessel of liquid air B, arranged for projection, care being taken to guard against heat radiation from the electric lamp by the use of a water cell. A large bulb of charcoal C, cooled in a separate vessel of liquid air F, is connected to a bent tube provided with a three-way stopcock D, passing through an india-rubber cork E which fits the liquid hydrogen vacuum tube A. The cork is now rapidly fitted

into the neck of the liquid hydrogen tube, and the stopcock opened to the charcoal condenser. The liquid hydrogen is thus subjected to the exhaust of the large cooled charcoal bulb, and is seen to boil up rapidly, and very soon opaque solid hydrogen appears in ever-growing amount, until the whole mass of hydrogen is frozen to a snowy froth.

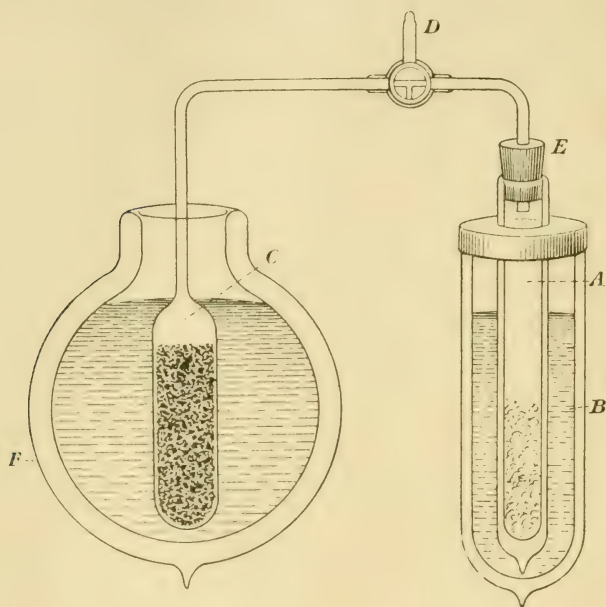


FIG. 6.

In this connection it may be pointed out that assuming the temperature of boiling helium to be about 5° absolute, then charcoal in liquid hydrogen under exhaustion, along with proper isolation of the helium from influx of heat, would very probably be able in a similar manner to bring about the production of solid helium, and we may be certain that if the charcoal could be cooled in liquid helium, then it could be solidified even if its solid tension was exceedingly small.

[J. D.]

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 22, 1909.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L.
Sc.D. F.R.S., President, in the Chair.

ALFRED RUSSEL WALLACE, ESQ., O.M. D.C.L. LL.D. F.R.S.

*The World of Life: as Visualised and Interpreted
by Darwinism.*

[ABSTRACT.]

THE lecturer began by stating that, although the theory of Darwinism is one of the most simple of comprehension in the whole range of science, there is none that is so widely and persistently misunderstood. This is the more remarkable, on account of its being founded upon common and universally admitted facts of nature, more or less familiar to all who take any interest in living things: and this misunderstanding is not confined to the ignorant or unscientific, but prevails among the educated classes, and is even found among eminent students and professors of various departments of biology.

Darwinism is almost entirely based upon those external facts of nature, the close observation and description of which constituted the old fashioned "Naturalists," and it is the specialisation in modern science that has led to the misunderstanding referred to. Those who have devoted years to the almost exclusive study of anatomy, physiology, or embryology, and that equally large class, who make the lower forms of life (mostly aquatic) the subject of microscopical investigation, are naturally disposed to think that a theory which can dispense with all their work (though often strikingly supported by it) cannot be so important and far-reaching as it is found to be.

Numbers, Variety and Intermingling of Life-forms.

Coming to the first great group of facts upon which Darwinism rests, the lecturer called attention to the great number of distinct species both of vegetable and animal life found even in our own very limited and rather impoverished islands, as compared with more extensive areas. Great Britain possessed somewhat less than 2000 species of flowering plants, while many equal areas on the Continent of Europe have twice the number. The whole of Europe contains 9000 species, and the World 136,000 species already described: but the total number, if the whole earth were as well known as Europe, would be almost certainly more than double that number or about a

quarter of a million species. The following table, showing how much more crowded are the species in small than in large areas, was exhibited on the wall. It affords an excellent illustration of the fact of the great intermingling of species, so that large numbers are able to live in close contact with other, usually very distinct, species.

*Numbers of Flowering Plants.**

	Square Miles.	Species.
The County of Surrey . . .	760	840
A portion containing . . .	60	660
" " . . .	10	600
" " . . .	1	400

The above figures were given by the late Mr. H. C. Watson, one of our most eminent British botanists, and as he lived most of his life in the county, they are probably the results of his personal observation, and are therefore quite trustworthy.

Continuing the above enquiry to still smaller areas, one perch equalling $\frac{1}{160}$ acre, or less than the $\frac{1}{100,000}$ of a square mile, has been found to have about 40 distinct species, while on a patch 4 ft. by 3 ft. in Kent (or about $\frac{1}{25,000,000}$ of a square mile) Mr. Darwin found 20 species.

The same law of increase of numbers in proportion to areas applies to the animal world, if we count all the species that visit a garden or field during the year, though those that can continuously live there are not perhaps so numerous in very small areas.

The Increase of Plants and Animals.

The powers of increase of plants and animals were next discussed, and were shown to be enormously great. An oak tree may produce some millions of acorns in a good year, but only one of these becomes a tree in several hundred years, to replace the parent. Kerner states that a common weed, *Sisymbrium Sophia*, produces about three-quarters of a million of seeds: and if all these grew and multiplied for three years, the plants produced would cover the whole land surface of the globe.

Equally striking is the possible increase in the animal world. Darwin calculated that the slowest breeding of all animals, the elephant, would in 750 years, from a single pair produce nineteen millions. Rabbits, which have several litters a year would produce a million from a single pair in four or five years, as they have probably done in Australia, where they have become a national calamity. As illustrative of this part of the subject, the lecturer referred at some

* Other tables illustrating similar facts in other parts of the world were prepared, but not exhibited, as being likely to distract attention from the lecture itself.

length to the cases of the bison and the passenger pigeon in North America, and the lemmings of Scandinavia. In the insect tribes still more rapid powers of increase exist. The common flesh-fly goes through its complete transformations from egg to perfect insect in two weeks; and Linnæus estimated that three of these flies could eat up a dead horse as quickly as a lion.

It is these enormous powers of rapid increase that have ensured the continuance of the various types of existing life from the earliest geological ages in unbroken succession; while it has also been an important factor in the production of new forms which have successively occupied every vacant station with specially adapted species.

Inheritance and Variation.

The vitally important facts of inheritance with variation was next discussed, and their exact nature and universal application pointed out. The laws of the *frequency* and the *amount* of variations, and their occurrence in all the various parts and external organs of the higher animals, was illustrated by a series of diagrams. These showed the actual facts of variation in adult animals of the same sex obtained at the same time and place, which had been carefully measured in numbers varying from twenty to several thousand individuals.

The general result deduced from hundreds of such measurements and comparisons, was, that the individuals of all species varied around a mean value—that the numbers became less and less as we receded from that mean, and that the limit of variation in each direction was soon reached. Thus, when the heights of 2600 men, taken at random, were measured, those about 5 ft. 8 in. in height were found to be far the most numerous. About half the total number had heights between 5 ft. 6 in. and 5 ft. 10 in., while only 10 reached 6 ft. 6 in., or were so little as 4 ft. 10 in., and at 6 ft. 8 in. and 4 ft. 8 in. there were only one of each.

The diagrams from the measurements of various species of birds and mammals were shown to agree exactly in general character; and the further fact was exhibited by all of them, that the parts and organs varied more or less independently, so that the wings, tails, toes, or bills of birds were often very long, while the body, or some other part was very short, a point of extreme importance, as supplying ample materials for adaptation through natural selection.

The Law of Natural Selection.

The next subject discussed was the nature and mode of action of natural selection. It was pointed out that since the glacial epoch no decided change of species had occurred. This showed us

that the adaptation of every existing species to its environment was not only special but general. The seasons changed from year to year, but the extremes of change only occurred at long intervals, perhaps of many centuries, with lesser, but still very considerable variations twice or thrice in a century. It was by the action of these seasons of extreme severity at long intervals, whether of arctic winters, or summer droughts, that the very existence of species was endangered; and it was at such times that the enormous population of most species and their wide range over whole continents, always secured the preservation of considerable numbers of the best adapted in the most favoured localities. Then the rapidity of multiplication came into play, so that in two or three years the population of each species became as great as ever: while, as all the least favourable variations had been destroyed, the species as a whole had become better adapted to its environment than before the almost catastrophic destruction of such a large proportion of them.

It is the fact of the adaptation of almost all existing species to a continually fluctuating environment—fluctuating between periodical extremes of great severity—that has produced an amount of adaptation that in ordinary seasons is superfluously complete. This is shown by the well-known fact that large numbers of adult animals that have not only reached maturity but have also produced offspring and successfully reared them, continue to live and breed for many years in succession, although varying considerably from the mean, while almost the whole of the inexperienced young fall victims to the various causes of destruction that surround them.

The Nature of Adaptation.

The next subject discussed was the complex nature of adaptations in many cases, and probably in all; a subject of great extent and difficulty. The lecturer directed special attention to the relations between the superabundance of vegetation in spring and summer, the enormous, but, to us, mostly invisible, hosts of the insect tribes which devour this vegetation, and the great multitudes of our smaller birds whose young are fed almost exclusively on these insects. Without these hosts of insects the birds would soon become extinct; while without the birds, the insects would increase so enormously as to destroy a considerable amount of vegetable life, which would, in its turn, lead to the destruction of much of the insect, and even of the highest animal groups, leaving the world greatly impoverished in its forms of life.

The vast numbers of insects required daily and hourly to feed each brood of young birds was next referred to, and the wonderful adaptation of each kind of parent bird which enables it to discover and to capture a sufficient quantity immediately around its nest, in competition with many others engaged in the same task in every

copse and garden, was next pointed out. The facts were shown to involve specialities of structure, agility of motions, and acuteness of the senses, which could only have been attained by the preservation of each successive slight variation of a beneficial character throughout geological time; while the emotions of parental love must also have been continuously increased, this being the great motive power of the strenuous activity exhibited by these charming little creatures.

Lord Salisbury on Natural Selection.

As illustrating the strange and almost incredible misconceptions prevailing as to the mode of action of natural selection, the lecturer quoted the following passage from the late Lord Salisbury's presidential address to the British Association at Oxford in 1894. After describing how the diverse races of domestic animals have been produced by artificial selection, Lord Salisbury continued thus:—

"But in natural selection, who is to supply the breeder's place? Unless the crossing is properly arranged the new breed will never come into being. What is to secure that the two individuals of opposite sexes in the primeval forest, who have been both accidentally blessed with the same advantageous variation, shall meet, and transmit by inheritance that variation to their successors? Unless this step is made good the modification will never get a start; and yet there is nothing to ensure that step but pure chance. The law of chance takes the place of the cattle-breeder or the pigeon-fancier. The biologists do well to ask for an immeasurable expanse of time, if the occasional meetings of advantageously varied couples, from age to age, are to provide the pedigree of modifications which unite us to our ancestors, the jelly-fish."

Here we have the extraordinary misconception presented to a scientific audience as actual fact, that advantageous variations occur singly, at long intervals, and remote from each other; each statement being, as is well known, the absolute reverse of what is really the case. It totally ignores the fact, that every abundant species consists of tens or hundreds of millions of individuals, and that as regards any faculty or quality whatever, this vast host may be divided into two portions—the *less* and the *more* adapted—not very unequal in amount. It follows that at any given time, in any given country, the advantageous variations always present are not to be counted by ones and twos, as stated by Lord Salisbury, but by scores of millions; and not in individuals widely apart from each other, but constituting in every locality or country somewhere about one half of the whole population of the species.

The facts of nature being what they are, it is impossible to imagine any slow change of environment to which the more populous species would not become automatically adjusted under the laws of multipli-

cation, variation, and survival of the fittest. Almost every objection that has been made to Darwinism assumes conditions of nature very unlike those which actually exist, and which must, under the same general laws of life, always have existed.

Protective Colour and Mimicry.

The phenomena of protective coloration and mimicry were very briefly alluded to, both because they are comparatively well known and had formed the subject of previous lectures; while they are very easily explained on the general principles now set forth. The explanation is the more easy and complete, because of all the characters of living organisms, colour is that which varies most, is most distinctive of the different species, and is almost universally utilised for concealment, for warning, or for recognition. And further, its useful results are clear and unmistakable, and have never been attempted to be accounted for in detail by any other theory than that of the continuous selection of beneficial variations.

The Dispersal of Seeds.

The subject of the dispersal of seeds through the agency of the wind, or of carriage by birds or mammals in a variety of ways, and often by most curious and varied arrangements, of hooks, spines, or sticky exudations almost infinitely varied in the different species, was also briefly treated, since they are all readily explicable by the laws of variation and selection, while no other rational explanation of their formation has ever been given.

Conclusion.

In concluding, the lecturer called attention to a series of cases which had shown us the actual working of natural selection at the present time. He also explained that these cases were at present few in number, first, because they had not been searched for; but perhaps, mainly, because they only occur on a large scale at rather long intervals, when some great and rather rapid modification of the environment is taking place.

In the following paragraph he endeavoured to summarise the entire problem and its solution: "It is only by continually keeping in our minds all the facts of nature which I have endeavoured, however imperfectly, to set before you, that we can possibly realise and comprehend the great problems presented by the 'World of Life'—its persistence in ever-changing but unchecked development throughout the geological ages, the exact adaptations of every species to its actual environment both inorganic and organic, and the ex-

quisite forms of beauty and harmony in flower and fruit, in mammal and bird, in mollusc and in the infinitude of the insect-tribes; all of which have been brought into existence through the unknown but supremely marvellous powers of Life, in strict relation to that great law of Usefulness, which constitutes the fundamental principle of Darwinism."

[A. R. W.]



WEEKLY EVENING MEETING,

Friday, January 29, 1909.

SIR WILLIAM CROOKES, D.Sc. F.R.S., Honorary Secretary and Vice-President, in the Chair.

COLONEL SIR FREDERIC L. NATHAN, R.A. *M.R.I.*

Improvements in Production and Application of Guncotton and Nitroglycerine.

THE subject, on which I have been asked to lecture to-night, is one that has often been dealt with in this theatre by such great authorities as the late Sir Frederick Abel and Sir Andrew Noble. I feel it a great honour therefore to have been invited to deliver this discourse, but I realise that it is a very difficult task I am attempting. Sir Frederick Abel was one of the greatest authorities on the chemistry of explosives, and used to deal with them mainly from the chemical point of view; Sir Andrew Noble, on the other hand, is the greatest living authority on all that concerns artillery and ballistics, and he has considered explosives mainly from the ballistic standpoint. I am neither a chemist nor an artillerist, but occupy the much more humble position of a manufacturer of explosives, and I must ask your indulgence whilst I endeavour to describe the main features connected with the manufacture of guncotton and nitroglycerine, and to say a few words about cordite, essentially a combination of the two.

For centuries the only explosive known to the world was that mechanical mixture of saltpetre, charcoal and sulphur, called gunpowder. Chemical explosives may be said to date from the discovery of guncotton by Schönbein, and it is a fact worth noting on this occasion, that the first sample of guncotton in this country was one which accompanied a letter of Schönbein from Basle, dated the 18th March 1846, and addressed to Michael Faraday at the Royal Institution. Schönbein referred to guncotton in this letter as follows:—

“There is another point about which I take the liberty to ask your kind advice. I am enabled to prepare in any quantity a matter which, next to gunpowder, must be regarded as the most combustible substance known. So inflammable is that matter that on being brought in contact with the slightest spark, it will instantly be set on fire, leaving hardly any trace of ashes, and if the combustion be caused within closed vessels, a violent explosion takes place. That combustible substance is, as I will confidently tell you, raw cotton, prepared in a simple manner, which I shall describe you hereafter. I must not omit to mention that water has not the least action upon my matter, that is, that it may be immersed ever so long in that fluid, without

losing its inflammability after having been dried again. A substance of that description seems to be applicable to many purposes of daily life, and I should think that it might advantageously be used as a powerful means of defence and attack. Indeed, the Congreveian rockets can hardly be more combustible than my prepared cotton is. What shall I do with that matter? Shall I offer it to your Government? I have enclosed a little bit of that really frightful body, and you may easily convince yourself of the correctness of my statements regarding its properties."

In a subsequent letter he gave this body the name of guncotton.

Attempts to manufacture guncotton in accordance with the method devised by Schönbein were made both in this country and abroad. Accidents which occurred, however, both in Great Britain and France in the early days of manufacture, led to the abandonment of attempts to produce it in these countries: it was only in Austria that its production was persevered with, and a system of manufacture worked out there by Baron von Lenk. Having succeeded in producing guncotton on the manufacturing scale, von Lenk turned his attention to adapting it for propulsive purposes, and although at one time his efforts appeared to have met with a certain amount of success, and batteries of field artillery in Austria were actually equipped with guncotton cartridges, the difficulty of moderating its rate of combustion was never satisfactorily overcome. While this question was still the subject of experiments, serious accidents, due to the spontaneous combustion of guncotton in store, led to its production being given up even in Austria.

In 1863, Sir Frederick Abel took up the study of the manufacture of guncotton in this country with a view to adapting it for propulsive purposes, and, at the same time, to improving its stability, so that its spontaneous combustion in store might be prevented.

He was not successful in the first object, but as regards the production of guncotton of good stability, the modifications that he introduced into the von Lenk system of manufacture resulted in the production of stable guncotton.

The process of manufacture devised by von Lenk was briefly as follows:—

Skeins of long staple cotton yarn were immersed in a mixture of strong nitric acid of 1.52 sp.gr., one part, and sulphuric acid of 1.84 sp.gr., three parts, contained in iron pans. The skeins were stirred about in the acid bath for a few minutes, removed to a grating above it, and some of the acid squeezed out with a suitable iron tool. The cotton, while still thoroughly wetted with acid, was transferred to earthenware pots, in which it remained for 48 hours. The pots stood in cold water to prevent decomposition of their contents. At the end of two days the conversion of the cotton into guncotton was complete: the skeins were removed from the pots, and as much as possible of the acid removed in centrifugal wringing machines. After centri-

fugalling, the skeins were drowned as rapidly as possible in a cascade of water, the object being to remove the rest of the free acid. The final purification was effected by immersing the skeins for about three weeks in running water, boiling for a few minutes in an alkaline solution, and finally washing for a few days in flowing water.

In all that concerned the actual process of nitration, Abel followed von Lenk, but instead of using skeins of long staple cotton, he introduced the use of cotton waste from the spinning mills, suitably cleaned : and after the free acid had been removed in the preliminary drowning, the guncotton, still in the same physical condition as the cotton waste from which it had been produced, was reduced to a fine state of division in a beating-engine. The effect of this important modification was to remove the last traces of "free acid," and of unstable bodies, so that the prolonged washing in cold water could be dispensed with, and at the same time, a much more stable product be obtained.

Cotton fibre is of a tubular structure, and as long as these tubes exist in long lengths, the impurities in the interior of the tubes, derived from the evaporated juices of the cotton plant, and more or less affected by the nitration process, are extremely difficult of removal. Not only is the cotton in the form of long tubular fibres, but these fibres are themselves matted and entwined to such an extent that the former process of washing in running water even failed to remove impurities from amongst the bundles of fibre.

The operation of pulping introduced by Abel, breaks up both the bundles of fibre and the fibres themselves, reducing the latter to short lengths or destroying them altogether by crushing. In this fine state of division the removal of impurities is much more readily effected by washing.

The manufacture of guncotton by the von Lenk-Abel process was commenced in this country about 1865. Foreign countries took it up in quick succession, and the process was the one universally followed for the next 40 years. Some modifications of the nitration process were made towards the end of that period, in one case in the direction of dipping larger charges of cotton waste, and of allowing them to remain in the original acid mixture until nitration was completed, and then transferring the whole contents of the nitrating pan into the acid centrifugal : in another case the nitration process was actually carried out in the centrifugal itself.

In 1905, however, an entirely new system of nitration, hereafter referred to as the "displacement process," was invented by Messrs. Thomson, of the Royal Gunpowder Factory, and this process has been perfected and has entirely replaced the pot system of nitration there, and at Nobel's Explosives Factory at Ardeer, in Scotland. It is also being adopted at other factories both in this country and abroad.

The nitration of the cotton waste is carried out in shallow circular

earthenware pans. These pans are grouped together and worked in sets of four. The bottom of the pan slopes downwards to a central hole, connected by suitable pipes and cocks to a pipe supplying the nitrating acid, and to other pipes through which the waste acid is removed on completion of nitration. The pans are covered with aluminium hoods connected to an exhaust-fan, for carrying off fumes.

Nitrating acid is then run in up to a definite mark, and a charge of 20 lb. of dry cotton waste is immersed in the acid in each pan in small quantities at a time. An aluminium fork is used for the purpose. When the charge of cotton waste has been dipped, perforated earthenware plates are placed on the top of it, to keep it all under the surface of the acid; a film of cold water is run on to the surface of the plates and serves as a seal to prevent fumes getting into the room, and the aluminium hoods are removed. The cotton waste remains soaking in the acid for two and a half hours; at the expiration of that time its conversion to guncotton is complete. The cock leading to the waste acid pipe is then opened and the waste acid allowed to flow away from the guncotton at a definite rate, whilst cold water is allowed to flow on to the top of the perforated plates at an equal rate. The water follows up the acid through the guncotton without any appreciable mixing of the water and the acid taking place, and when the whole of the acid has been displaced in this way, the water is allowed to drain away from the gun-cotton, which is then ready for the final purification process. This system of manufacture possesses many advantages over the systems which it is superseding. Foremost among them are:—

1. Decreased cost of manufacture, due to the facts that for a given output very much less labour is required: that the plant is both very cheap and very durable: that no power is required to work it: that less acid is lost in the washing processes; and that, owing to the absence of fumes and spilt acid, the cost of maintenance of the buildings is reduced.

2. Increased safety as far as personnel is concerned, because there is no escape or splashing about of acid, which in the old processes was a fruitful source of acid burns, and also because decompositions, which used to take place both in the digesting pots and in the acid centrifugals, with the consequent evolution of poisonous oxides of nitrogen, no longer occur.

3. A better guncotton is obtained. It is freer from unconverted cotton, and as the whole of the nitration and preliminary washing operations are carried out in earthenware receptacles, it is freer from mineral impurities.

4. An increased yield to the extent of about 7 per cent. is realised.

The manufacture of guncotton was not commenced at the Royal Gunpowder Factory, Waltham Abbey, until the year 1872. Shortly after that date an improvement was made in the purification pro-

cess. It consisted in subjecting the guncotton, while still in the waste form, to a series of steam boilings in large wooden vats. In the early days of this process boilings of long duration were used throughout. Later, a system was introduced in which a large number of short boilings at the commencement, was followed by a couple of final long boilings. With the introduction of the displacement process of nitration, a thorough investigation of the chemistry of the boiling process was undertaken at Waltham Abbey, and as a result it was ascertained that a more rapid purification was effected by means of two long boilings, each of twelve hours duration, followed by a series of very much shorter ones.

It is very probable that the displacement system of nitration is itself responsible for the reduction in the amount of boiling required to produce a stable guncotton. Although there is no appreciable amount of mixing taking place between the displacing water and the waste acid, still mixing at the surface of contact does occur to a slight extent, sufficient to produce a distinct rise of temperature. The zone of warm acid liquid produced passes very slowly through the whole of the guncotton, removing in its course various impurities. The purifying action of this liquid is no doubt due to the fact that it possesses strong oxidising and solvent properties.

The pulping process introduced by Abel is still universally employed, and although its value from a purification point of view is no longer of such great importance now that guncotton is boiled, as it was in the early days of cold water washing, it is, undoubtedly, still of use in effecting a final purification of the guncotton.

In the beating engine, the mechanical process of reducing the guncotton to a pulp is effected, but no actual removal of impurities takes place, because the water is not changed during the operation. The impurities still present in the guncotton at this stage are both mechanical and chemical. The mechanical impurities consist chiefly of particles of metal of various kinds, sand and fine grit, wood and similar substances, introduced originally in the cotton waste and during the processes of manufacture. The chemical impurities are bodies produced by the action of the nitrating acid on bodies other than cellulose; they are not entirely removed in the boiling process, but are set free in the pulping. To remove the mechanical impurities the guncotton pulp in a large volume of water is at Waltham Abbey run from the beaters over flannel laid on long shallow troughs, the troughs having pockets with baffle plates at intervals. The rough surface of the flannel retains the fine particles of grit, etc., and the larger particles settle in the pockets or grit-traps. In the last pocket an electro-magnet is inserted to remove iron or steel particles, which may have escaped retention in the grit-traps.

The guncotton thus freed from mechanical impurities runs into large oval iron tanks, called "poachers," where it receives several cold water washings. The contents of the poacher are agitated by

means of a power-driven wooden paddle-wheel, and then allowed to settle. The washing water containing the impurities is drawn off from the surface of the guncotton by means of a large skimmer, in order that not only impurities in solution may be removed, but also any light solid impurities in suspension.

The finally purified pulp is passed to a moulding machine, where it is lightly compressed to remove the bulk of the water, and converted into a form in which it can be easily handled. If intended for use in mines or torpedoes, or for demolition purposes, the lightly compressed shapes are submitted to heavy hydraulic pressure, converting them into dense hard blocks.

The other high explosive of which I am to speak, viz., nitroglycerine, enters into the composition of many modern propellants. Nitroglycerine was discovered by Sobrero in 1847, but it remained for a long time a chemical curiosity only.

Alfred Nobel commenced its manufacture as a blasting agent, about 1868, for which purpose he absorbed nitroglycerine with an infusorial earth, known as *kieselguhr*, and gave the compound the name of dynamite. But, prior to this, nitroglycerine had been made on a large scale in America, where it was frozen after manufacture, for purposes of transport, and used for blasting.

Nitroglycerine is a liquid, and is a much more violent explosive than guncotton, and whereas the manufacture of guncotton is absolutely safe throughout, that of nitroglycerine is dangerous. The risks attendant on the manufacture of nitroglycerine are due to the facts that the temperature resulting from the chemical reaction is not so easily controlled, and that nitroglycerine, being a liquid insoluble in water, the processes after nitration have to be carried out with a substance not rendered inert, as guncotton is, by admixture with water. For these reasons the nitration of glycerine in the early days of the production of nitroglycerine on a manufacturing scale was carried out in very small quantities.

With the introduction of dynamite, the small pots used for the nitration of glycerine, standing in vessels full of ice water, were replaced by lead tanks in which considerable quantities of glycerine, amounting to several hundred pounds, were nitrated at one operation. In these vessels the temperature was controlled by means of cold water circulating through lead coils fixed in the tank, and the whole contents of the tank were kept in agitation during the nitration by means of mechanical stirrers, or by compressed air escaping through small holes in lead pipes situated at the bottom of the nitrating vessel. On completion of the nitration it was the practice in the early days to drown the whole of the charge of nitroglycerine and waste acid in a large bulk of water, from which the nitroglycerine separated out and was removed for subsequent purification by washing with alkaline solutions in lead tanks. This system entailed the loss of the waste acid, and was superseded by a process in which

the nitroglycerine and waste acid were run from the nitrating vessel into another vessel termed a separator, and allowed to separate in it. The nitroglycerine, being lighter than the waste acid, came to the top and was run off into a third tank for preliminary purification consisting of several water washings.

This preliminary purification removes most of the free acid adhering to and dissolved in the nitroglycerine, but in order to obtain a stable product a further and prolonged purification is necessary, as in the case of guncotton. This is effected in lead tanks, by repeated washings with warm dilute sodium carbonate solution. The alkali remaining in the nitroglycerine after this treatment is thoroughly removed by washing with purified warm water. As nitroglycerine is a somewhat viscous liquid, special care has to be taken that the washing solutions are brought into very intimate contact with every portion of the charge of nitroglycerine. For this purpose, the method universally employed is to agitate the contents of the washing tanks by means of the escape of air under compression through small holes in the bottom of the tank. As a result of this very thorough agitation, the nitroglycerine, even after the removal, by skimming, of as much as possible of the washing liquid, still contains a small proportion of water suspended in it in a very fine state of division. It also contains small quantities of flocculent impurities and mineral matter derived from the glycerine and acids. To get rid of these bodies, filtration is resorted to ; coarse crystalline salt is very usually employed as a medium, but at Waltham Abbey it has been found that a filter in the form of a mat of sponges is more efficacious and free from the objections salt filters possess.

After the removal of the nitroglycerine from the waste acid, which takes place in a comparatively short space of time, the waste acid was run out of the separator into large lead vessels, where it remained for days, in order to allow of the formation and removal of the last traces of nitroglycerine. This process is known as after-separation, and was necessary to enable the waste acid to be dealt with without risk, because as long as it contained any traces of nitroglycerine, it could not be stored or handled, without risk of violent decompositions, or even of explosions, taking place..

This system of manufacture, comprising nitration, separation, preliminary washing, final washing and after-separation, all carried out in different vessels and in different houses, was the one which, with slight modifications in detail, was followed almost universally, and is still in use in many of the older factories in this country and abroad. Its disadvantages are several. In the first place, owing to the fact that it is unsafe to transport or to carry liquid nitroglycerine about, factories are always designed so that it may flow from process to process by gravity. The result, obviously, is that nitroglycerine houses must be built on the side of a hill, or, as this is not always possible, the alternative of building a nitrating house and also, prob-

ably, a separating house, on artificial mounds, has to be resorted to, entailing a very considerable expense.

In the next place, owing to the corrosive nature of the mixture of nitroglycerine and waste acid, and to the acid nature of the nitroglycerine even when separated from the waste acid, the only material which can be used for the cocks necessary to allow of the nitroglycerine and acid to run from vessel to vessel, is earthenware. The use of earthenware cocks is attended with considerable risk, owing to the fact that there is friction in them between the key and the body of the cock, and there is always the risk in moderately cold weather of the nitroglycerine freezing and fixing the key; force, if used under these circumstances, would be very liable to cause accident. Again, the necessity of storing the waste acid under observation for long periods is a costly one, both as regards labour and plant required.

It was to overcome these disadvantages that the whole system in current use for the manufacture of nitroglycerine received very careful consideration at the Royal Gunpowder Factory some years ago.

The first step that was taken to improve matters, was to abolish the use of earthenware cocks in the preliminary and final washing tanks. As the nitroglycerine when it was ready to leave these tanks was thoroughly free from acid, it was possible to get rid of the cocks on these tanks, and to replace them by rubber tubes. During the washing operations this tube is secured to a nozzle fixed to the outside of the tank at a point above the level of the liquid. To run off the nitroglycerine it is only necessary to slip the rubber tube off the nozzle and direct it into another vessel or into a lead gutter used to convey the nitroglycerine to the next operation.

Rubber, however, could not be used in the case of the separator or the nitrator, where either acid nitroglycerine, or a mixture of nitroglycerine and acid had to be drawn off. To overcome the difficulty in this case an entirely new system was invented at Waltham Abbey. Instead of running the nitroglycerine and waste acid on completion of the nitration process into the separator, the separation is allowed to take place in the nitrating vessel itself. Nitroglycerine as it separates from the waste acid comes to the top, it being the lighter of the two liquids, and to remove it from the nitrator all that is necessary is to raise the liquid contents of the vessel gradually until the nitroglycerine reaches the top of the nitrator, where a pipe or gutter is fixed to lead the nitroglycerine away into the preliminary washing tank. This raising of the charge is effected by introducing into the bottom of the nitrator, through the same pipe by which the nitrating acid is admitted, the waste acid from a previous charge. The rate of inflow of the waste acid is regulated so that the nitroglycerine displaced is as free as possible from acid in suspension.

The waste acid has still to be dealt with. It was discovered that the addition of a small percentage of water to this acid, after the nitroglycerine has been separated from it in the nitrator-separator,

entirely prevents the further formation and separation of the small traces of the nitroglycerine, which the after-separating bottles were required to deal with.

The advantages of the Waltham Abbey plant and system of manufacture over others are briefly as follows :—

1. *Increased Safety.*—By the abolition of all cocks through which nitroglycerine had to pass, the risks attendant on their use have disappeared. By the presence of cooling coils in the one and only vessel in which nitroglycerine and acids are in contact, any undue rise in temperature, always a possibility in the circumstances, can be at once checked. It was not usual to have cooling coils in the separator and after-separating bottles.

2. *Reduction in Total Elevation for, and Area of a Factory.*—The abolition of the separator, and the running off of the nitroglycerine from the top of nitrator effect a very material saving in the height required.

The after-separating house being no longer necessary, nor the separator house when one existed as distinct from the nitrating house, the number of buildings, and therefore the ground area, is substantially reduced.

3. *Reduced Cost of Production.*—This results from the fact that the capital outlay for a factory is much less, that fewer men are required for a given output, that there is less plant and fewer buildings to maintain, and that the plant itself suffers slower deterioration. Finally, the yield of nitroglycerine is increased by at least 5 parts for every 100 parts of glycerine nitrated.

The substitution, recently, of Nordhausen for ordinary sulphuric acid, has further improved the yield of nitroglycerine, and whereas a few years ago a yield of 210 parts of nitroglycerine for every 100 parts of glycerine nitrated was considered excellent, the average yield at Waltham Abbey is now 230 per cent., a very high figure in view of the fact that the theoretical yield is 246·74 per cent. The use of Nordhausen sulphuric acid also permits of a considerable reduction in the proportion of nitrating acid to glycerine, so that a larger output is obtainable for any given sized plant.

In the year 1846, Schönbein discovered guncotton. In the year 1886, that is, 40 years later, the French chemist Vieille invented his smokeless powder for military purposes. This explosive, which was primarily designed for use in the small calibre Lebel rifle, consisted essentially of guncotton, and the secret of its success lay in the fact that Vieille so altered its physical state that its rate of combustion, when confined, was under complete control. This condition was arrived at by treating the fibrous guncotton with suitable solvents which entirely destroyed the fibre and converted it into a colloidal horny substance quite devoid of all porosity. The gelatinised guncotton resulting from this treatment burnt, when ignited, from the surface inwards, and by varying the surface area, any

required rate of combustion could be obtained. The use of smokeless powders, manufactured in this way, was very soon extended to all natures of ordnance.

The next step in the development of smokeless powders was the combination of nitroglycerine with nitrocellulose. The first powder of this type was the "ballistite" of Alfred Nobel, patented by him in the year 1888. The original ballistite was composed of equal parts of nitroglycerine and of soluble nitrocellulose, a variety of guncotton soluble in nitroglycerine, and no solvent was therefore required in its preparation, although a certain proportion of camphor was used to promote the solution of the nitrocellulose. Another form of nitroglycerine-nitrocellulose explosive is the British service powder, cordite, which originally consisted of nitroglycerine 58 parts, guncotton, insoluble in nitroglycerine, 37 parts, and mineral jelly, a product of the distillation of crude petroleum, 5 parts. To effect the gelatinisation of the guncotton, the solvent acetone, obtained indirectly from the destructive distillation of wood, is employed. The result of subjecting nitrocellulose, in suitable machines, to the action of nitroglycerine or of solvents, of which there are several suitable ones besides acetone, is to destroy its fibre and convert it into a gelatinous mass, in which condition it can be formed into any desired shape. Where solvents are used to produce this result, they remain in the mass during subsequent operations, and are finally driven off by means of heat. The resulting products, somewhat incorrectly termed "powders," which are manufactured in a variety of forms, such as grains and flakes of different shapes, ribbons or strips, solid cords, tubes, etc., vary in consistence with the quantity of nitroglycerine they contain. The more nitroglycerine present, the softer the powder; pure nitrocellulose powders being hard to brittleness.

For practical purposes modern smokeless powders are of two types:—

1. Those consisting entirely of nitrocellulose, and termed "nitrocellulose powders."

2. Those consisting of a mixture of nitrocellulose and nitroglycerine, known as "nitroglycerine powders."

Opinions differ somewhat as to the relative merits of these two types; in this country the latter type is preferred. Their characteristic features are, briefly, as follows:—

A nitroglycerine powder is more powerful than a nitrocellulose powder, and the more nitroglycerine present the more powerful the explosive. Therefore, for equal ballistics, a smaller charge of the former than of the latter is required, and, consequently, the chamber capacity and the size and weight of the breech mechanism is reduced; on the other hand, the higher the proportion of nitroglycerine the higher is the temperature of combustion, and the greater the erosive effects on the surface of the bore of the gun.

The presence of nitroglycerine in an explosive allows of the more

easy and rapid elimination of the solvent used in manufacture, and of moisture, a small quantity of which is always present in nitroglycerine and guncotton. The sooner this is attained the better, because the longer the time that the powder is being heated in order to dry it, the more likely is its chemical stability to be affected. Moreover, it is a well-established fact that with nitrocellulose powders it is impossible to remove the volatile matter with anything like the same completeness as can be done in the case of nitroglycerine powders. The consequence is that the slow evaporation from nitrocellulose powders of the residual volatile matter which takes place in store tends to produce changes in their physical character, and renders them in course of time liable to alter in ballistic properties, and even to develop dangerous pressures in the gun.

Nitroglycerine powders are cheaper than nitrocellulose powders, weight for weight, and even more so for equal ballistic effects.

The original cordite, the manufacture of which commenced in 1890, contained a high proportion of nitroglycerine, 58 per cent., and the erosion produced, especially in large guns, was considerable. This led to experiments being carried out at Waltham Abbey, with a view to the production of a less erosive explosive, and the final result was the introduction into the service, in 1901, of a modified cordite known as "cordite M.D." in which the percentage of nitroglycerine is reduced to 30 per cent., so that the composition becomes: nitroglycerine 30 per cent., guncotton 65 per cent., and mineral jelly 5 per cent.

The constants of explosion of cordite and cordite M.D., determined at the Royal Gunpowder Factory, some little time ago, are as follows:—

Explosive.	Density of Loading.	Heat of Explosion at Constant Volume, Water Gaseous.	Total Gases, Water Gaseous at 0° C., 760 mm.	Temperature of Explosion.
		Calories per Gram.	cc. per Gram.	°C.
Cordite . . .	0·2	1156	871	2663
Cordite M.D. . .	0·2	965	920	2374

An inspection of these figures shows that the alteration in proportions of the explosive ingredients results in a decrease in the heat of explosion of about $16\frac{1}{2}$ per cent., and an increase in the volume of gases of about $5\frac{1}{2}$ per cent., whilst there is a decrease of 289° C. in the temperature of explosion.

As would therefore be expected the erosion produced by cordite M.D. is very much less than that produced by the original cordite

for the same ballistics, and is certainly not greater, if as great as that produced by the best forms of nitrocellulose explosives.

Although of minor importance to smokelessness, flamelessness is a desirable quality for propulsive explosives to possess. In this respect cordite M.D. is superior to cordite in the case of rifles and machine guns; unfortunately a suitable ingredient has not yet been discovered which will render smokeless powders flameless in large guns.

A third ingredient in both natures of cordite, viz., mineral jelly, although present in a comparatively small proportion, is a very important constituent.

Cordite in the advanced experimental stage consisted of nitroglycerine and gun-cotton alone, and as their combustion produced no solid residue of any kind, the surface of the bore of the magazine rifle in which the early experiments took place was not fouled in any way. The result was that the cupro-nickel coated bullets, propelled in succession at high velocity through a clean barrel, deposited some of the cupro-nickel in the bore. In order to prevent this a number of substances were incorporated with the nitroglycerine and gun-cotton, with the object of producing a deposit in the bore, which it was hoped would get rid of the difficulty of metallic fouling. Of all these various substances the one which appeared to answer the purpose most satisfactorily was refined vaseline, and this material became the third ingredient of cordite as eventually introduced into the British Service. When the manufacture was commenced on a large scale, vaseline, which is the proprietary name of one of the refined products of the distillation of petroleum, was replaced by mineral jelly, the same material, but in a cruder form.

The original object with which mineral jelly was introduced was of no importance when cordite was substituted for the black and brown powders used in large guns, but in order to have but one nature of smokeless powder in the service, mineral jelly was added to all cordite whether for use in small arms or artillery. Subsequent experience has demonstrated how very fortunate was the selection of this material for rifle cordite, and the extension of its use to all sizes of cordite.

Mineral jelly is one of the best ingredients it is possible to have in smokeless powders from the point of view of their chemical stability. This important fact, not recognised originally, was brought out in the following way: In order to facilitate the explosion of cordite in blank ammunition for the rifle, it was cut into very thin flakes and the non-explosive mineral jelly was omitted from its composition. After a comparatively short storage in a hot climate, the stability of the smokeless blank, as it was called, was found to have suffered seriously, whereas the stability of normal cordite containing mineral jelly was not appreciably affected. These facts led to a thorough investigation at Waltham Abbey of the action of mineral

jelly in preserving the stability of cordite, and it was discovered that mineral jelly contained constituents which had the valuable property of combining with the decomposition products (the result of prolonged storage of cordite at high temperatures) to form stable bodies, thus removing these decomposition products, which undoubtedly exert a deteriorating influence on the cordite, from their sphere of action.

When Abel was engaged on his researches in connection with the production and properties of guncotton, it was obvious to him that some test of a chemical nature was required, in order to ascertain whether or not the finished guncotton had been thoroughly purified in manufacture. It will be remembered that accidents occurred in the early days of its production because this purification had not been carried sufficiently far. The test which he devised was based on the principle that if guncotton be subjected to an elevated temperature, traces of oxides of nitrogen will be given off, and will reveal their presence by acting on a suitable reagent.

The test is carried out by heating guncotton in a test tube placed in a water bath, and suspending over it a strip of moistened filter paper impregnated with potassium iodide and starch. If the purification of the guncotton has not been sufficient, the discoloration of the test paper takes place early; as the result of experience Abel fixed a time before which no reaction should take place. This test, known as the Abel heat test, is a test for the purity of guncotton, and of nitroglycerine, and of freshly made explosives containing either one or both of these ingredients. For this purpose no test has yet been devised which equals it. But it was never intended to be, and is not, a quantitative test, and is therefore only a rough guide, though a very useful one, as to the stability of an explosive which has been in store for more or less prolonged periods, or under more or less adverse conditions.

Smokeless powders of the types dealt with are all subject to deterioration, and there is very little doubt that this deterioration is for any given explosive a function of the temperature of storage. The higher the temperature, the more rapid the deterioration.

The necessity, therefore, of some quantitative test which would enable a judgment to be formed as to the extent of deterioration suffered by any given sample of cordite is obviously of great importance, because such a test would afford the means of determining how much longer it would be safe to store any given batch of cartridges or lot of cordite at any given temperature. Any such test must be a heating test, and it must be possible to co-relate the temperature and duration of the test with any given temperature and duration of storage. The rate of deterioration as a function of the temperature was determined by Dr. Will for guncotton, and later by Dr. Robertson at Waltham Abbey for nitroglycerine. From these and other experiments carried out at Waltham Abbey, a factor of increase in rate of deterioration of cordite with increase of tempera-

ture was deduced. This factor having been determined, what is known as the "silvered vessel test" was worked out at the Royal Gunpowder Factory. In this test, of which the details will be described presently, cordite is heated in a specially designed vessel at $80^{\circ}\text{C}.$, a temperature not too far removed from those to be met with when cordite is stored under the worst service conditions, and the number of hours heating at this temperature any given sample will stand before it shows signs of active decomposition are ascertained. Then, by means of an equation, containing the factor connecting rate of increase of deterioration with rise in temperature, a calculation can be made converting the hours of heating at $80^{\circ}\text{C}.$ the sample withstood, to years and fractions of a year it would stand at any given temperature of storage, and therefore a knowledge is obtained of how much longer it would be safe to store this cordite at any given temperature.

This test was applied to a considerable number of samples of known age and thermal history. From these data and knowing the number of hours at $80^{\circ}\text{C}.$ that newly made cordite of good stability will stand before showing signs of decomposition, the number of hours that the different samples should stand the test were calculated. When the samples were actually tested, the number of hours heating at $80^{\circ}\text{C}.$ they withstood, were in close agreement with the number of hours it was calculated they should stand.

The form of vessel in which the heating is carried out is the well-known vacuum vessel of Sir James Dewar. A glass bulb silvered externally, is enclosed in an outer bulb, silvered internally. The space between the two is highly evacuated for the purpose of limiting the dissipation of any heat evolved by exothermic changes on the one hand, and on the other for the purpose of minimising the effect of accidental slight changes in temperature of environment.

In the centre of the inner bulb is situated the bulb of a thermometer, the stem of which passes through a cork in the neck of the vessel. A side tube is attached for the purpose of making observations on the colour of the gases evolved. For heating the vessel, a bath is provided, with cylinders closed at the bottom, and wide enough to admit the vessel to such a depth as the side tube will permit. The bath is surrounded by insulating materials. The vessels are packed in the cylinders with wool yarn, and the tops of the cylinders are closed with felt discs, to exclude draughts.

The bath is fitted with a gas regulator or other means for securing that the temperature of the explosive is kept constant.

The cordite is coarsely ground, and 50 grammes are used.

Readings of the thermometer are taken at intervals, and the time is noted when a rise of $2^{\circ}\text{C}.$ in the temperature of the explosive above the temperature of $80^{\circ}\text{C}.$ occurs. At the same time, visual observations are made as to the colour of the column of gas in the side tube, since it is found that previous to the rise in temperature occur-

ring orange-coloured fumes of nitric peroxide are evolved. When the temperature exceeds 82°C. , the test is complete and the flask is withdrawn. The number of hours which have elapsed since the start of the test is the measure of the stability of the cordite.

Until about 60 years ago, the only explosive known, for all purposes, was gunpowder. With the discovery of guncotton and nitroglycerine, gunpowder was gradually replaced by them for blasting purposes. In their early days the two explosives were used singly, guncotton as guncotton, nitroglycerine—first of all alone—and then as dynamite. Later on, the two were combined as blasting gelatine, and explosives of a similar nature, but it was quite 40 years after their discovery before either became of practical use for propulsive purposes.

The invention of "Poudre B" by Vieille marked the commencement of a new era in connection with the science of artillery, and it was not long before smokeless powders made from the violent guncotton or of guncotton combined with the still more violent nitroglycerine, entirely superseded the centuries-old gunpowder. Modern explosives are characterised by very greatly increased power, giving enormously greater range to projectiles fired from both rifles and artillery, thus altering entirely the conditions of both land and naval warfare.

It is at present not easy to forecast in what direction further improvements in propellants will take place. It is also difficult to conceive what the explosive of the future will be which shall produce a change as revolutionary as that which took place when smokeless powders superseded the old-fashioned black powders. For some time to come, probably, the manufacturer of explosives will have to content himself with endeavours to improve them as far as he can both from a ballistic and from a stability point of view, with the ingredients now at his disposal.

[F. L. N.]

GENERAL MONTHLY MEETING.

Monday, February 1, 1909.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

William Edward Lake, Esq.
Archibald Liversidge, Esq., M.A. LL.D. F.R.S.
K. C. E. von Martius, Esq., Ph.D.
Francis Joseph Sharpe, Esq.

were elected Members of the Royal Institution.

The Treasurer announced that the Managers had received an Anonymous Gift of £10,000 from a Lady: and the following Resolution, passed by the Managers at their Meeting held this day, was read and unanimously adopted:—

Resolved, That the Managers of the Royal Institution of Great Britain desire to express to the Lady who has Anonymously and Unconditionally placed at their disposal, for the purposes of the Institution, the sum of £10,000, their most grateful appreciation of her munificence and discernment. They accept the Gift as a timely and noble recognition of the good public work the Institution has done in the past, and is still doing, in the acquisition and diffusion of Scientific Knowledge, and as an incitement to maintain and extend its usefulness in the unique position which it has for more than a century occupied.

The Honorary Secretary announced the decease of Francis Elgar, Esq., LL.D. F.R.S. F.R.S.E. M.Inst.C.E., on January 16, and the following Resolution of Condolence, passed by the Managers at their Meeting held this day, was read and unanimously adopted:—

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their sense of the loss the Institution has sustained in the decease of Dr. Francis Elgar, LL.D. F.R.S. M.Inst.C.E., the first Professor of Naval Architecture in this country at the University of Glasgow, and late Manager of the Royal Institution, whose valuable and original contributions to the study of Naval Architecture are of world-wide reputation.

Dr. Elgar was elected a Member of the Royal Institution in 1892, became a Visitor in 1901, and a Manager in 1905, which office he held until 1907. He was a regular attendant at the Managers' Meetings and the Lectures, and manifested a keen interest in aiding the work of the Institution by the willing support he gave to the Research Fund.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with Mrs. Elgar and the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

- The Secretary of State for India*—Memoirs of Department of Agriculture :
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American Journal of Science for Dec.-Jan. 1908-9. 8vo.
Analyst for Dec.-Jan. 1908-9. 8vo.
Astrophysical Journal for Dec. 1908. 8vo.
Athenæum for Dec.-Jan. 1908-9. 4to.
Author for Jan. 1909. 8vo.
British Homœopathic Review for Dec.-Jan. 1908-9. 8vo.
Chemical News for Dec.-Jan. 1908-9. 4to.
Chemist and Druggist for Dec.-Jan. 1908-9. 8vo.
Concrete for Dec.-Jan. 1908-9. 8vo.

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- Dioptric Review for Dec. 1908. 8vo.
 Dyer and Calico Printer for Dec.-Jan. 1908-9. 4to.
 Electrical Contractor for Dec.-Jan. 1908-9. 8vo.
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WEEKLY EVENING MEETING,

Friday, February 5, 1909.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.

PROFESSOR JAMES GEORGE FRAZER, D.C.L. LL.D. Litt.D.

*The Influence of Superstition on the Growth of
Institutions.*

WE are apt to think of Superstition as an unmitigated evil, false in itself, and pernicious in its consequences. That it has done much harm in the world cannot be denied. It has sacrificed countless lives, wasted untold treasures, embroiled nations, severed friends, parted husbands and wives, parents and children, putting swords and worse than swords between them: it has filled gaols and madhouses with its innocent or deluded victims; it has broken many hearts; embittered the whole of many a life: and, not content with persecuting the living, it has pursued the dead into the grave and beyond it, gloating over the horrors which its foul imagination has conjured up to appal and torture the survivors. It has done all that and more. Yet the case of superstition, like that of Mr. Pickwick after the revelations of poor Mr. Winkle in the witness-box, can perhaps afford to be placed in a rather better light; and without posing as the Devil's Advocate, or appearing before you in a blue flame and sulphureous fumes, I do profess to make out what the charitable might call a plausible plea for a very dubious client. For I propose to prove, or at least to make probable by examples, that among certain races and at certain times some social institutions which we all, or most of us, believe to be beneficial, have partially rested on a basis of superstition. The institutions to which I refer are purely secular or civil: of religious or ecclesiastical institutions I shall say nothing. It might perhaps be possible to show that even religion has not wholly escaped the taint or dispensed with the aid of superstition; but I prefer for to-night to confine myself to those civil institutions which people commonly imagine to be bottomed on nothing but hard common sense and the nature of things. While the institutions with which I shall deal have all survived into civilised society, and can no doubt be defended by solid and weighty arguments, it is practically certain that among savages, and even among peoples who have risen above the level of savagery, these very same institutions have derived much of their strength from beliefs which

nowadays we should condemn unreservedly as superstitious and absurd. The institutions in regard to which I shall attempt to prove this are four, namely, government, private property, marriage, and the respect for human life. And what I have to say may be summed up in four propositions, as follows :—

I. Among certain races and at certain times superstition has strengthened the respect for government, especially monarchical government, and has thereby contributed to the establishment and maintenance of civil order.

II. Among certain races and at certain times superstition has strengthened the respect for private property, and has thereby contributed to the security of its enjoyment.

III. Among certain races and at certain times superstition has strengthened the respect for marriage, and has thereby contributed to a stricter observance of the rules of sexual morality, both among the married and the unmarried.

IV. Among certain races and at certain times superstition has strengthened the respect for human life, and has thereby contributed to the security of its enjoyment.

Before proceeding to deal with these four propositions separately, I wish to make two remarks, which I beg you to bear in mind. First, in what I have to say I shall confine myself to certain races of men and to certain ages of history, because neither my knowledge nor my time permits me to speak of all races of men and all ages of history. How far the conclusions which I shall draw for some races are applicable to others must be left to further inquiries to determine. That is my first remark. My second is this. If it can be proved that in certain races the institutions in question have been based partly on superstition, it by no means follows that even among these races they have never been based on anything else. On the contrary, as all the institutions which I shall consider have proved themselves stable and permanent, there is a strong presumption that they rest mainly on something much more solid than superstition. No institution founded wholly on superstition, that is, on falsehood, can be permanent. If it does not answer to some real human need, if its foundations are not laid broad and deep in the nature of things, it must perish, and the sooner the better. That is my second remark.

I. With these two cautions I address myself to my first proposition, which is, that among certain races and at certain times superstition has strengthened the respect for government, especially monarchical government, and has thereby contributed to the establishment and maintenance of civil order.

Among many peoples the task of government has been greatly facilitated by a superstition that the governors belong to a superior order of beings, or possess certain magical or supernatural powers to which the governed can make no claim and can offer no resistance. Thus Dr. Codrington tells us that among the Melanesians “the

power of chiefs has hitherto rested upon the belief in their supernatural power derived from the spirits or ghosts with which they had intercourse. As this belief has failed, in the Banks' Islands for example some time ago, the position of a chief has tended to become obscure: and as this belief is now being generally undermined, a new kind of chief must needs arise, unless a time of anarchy is to begin." It is thus that in Melanesia, as perhaps in other parts of the world, religious scepticism tends to undermine the foundations of civil society.

In Polynesia the state of things was similar. There, too, the power of chiefs depended largely on a belief in their supernatural powers, in their relation to ancestral spirits, and in the magical virtue of taboo, which pervaded their person and interposed between them and common folk an invisible but formidable barrier, to pass which was death. In New Zealand the Maori chiefs were deemed to be living *atuas* or gods. "Think not," said one of them to a missionary, "that I am a man, that my origin is of the earth. I come from the heavens: my ancestors are all there: they are gods, and I shall return to them." So sacred was the person of a Maori chief that it was not lawful to touch him, even to save his life. A chief has been seen at the point of suffocation and in great agony with a fish bone sticking in his throat, and yet not one of his people, who were lamenting around him, dared to touch or even approach him, for it would have been as much as their own life was worth to do so. Not only the person of a Maori chief, but everything that had come into contact with it, was sacred and would kill, so at least the Maoris thought, any sacrilegious person who dared to meddle with it. Cases have been known of Maoris dying of sheer fright on learning that they had unwittingly eaten the remains of a chief's dinner or handled something that belonged to him. For example, a chief's tinder-box has proved fatal to several men: for having found it and lighted their pipes with it, they actually expired of terror on learning to whom it belonged. Thus the divinity which hedged a Maori chief was a devouring flame which shrivelled up and consumed whatever it touched. No wonder that such men were implicitly obeyed.

Throughout the rest of Polynesia the state of things was similar. In Africa we meet with a superstition of the same sort. The Cazembes of Angola regarded their king as so holy that no one could touch him without being killed by the magical power which emanated from his sacred person. Similar beliefs are current in the Malay region, where the theory of the king as the Divine Man is strongly held. Not only is the king's person considered sacred, but the sanctity of his body is believed to communicate itself to his regalia and to slay those who break the royal taboos. Thus it is firmly believed that any one who seriously offends the royal person will be struck dead by a quasi-electric discharge of that Divine Power which the Malays suppose to reside in the king's body. Further, the Malays firmly believe that

the king possesses a personal influence over the works of nature, such as the growth of the crops and the bearing of fruit-trees.

Similarly in Africa kings are commonly supposed to be endowed with a magical power of making the rain to fall and the crops to grow. In times of drought and dearth they are entreated by their subjects to exert their supernatural powers for the good of the country; and if no rain falls and vegetation perishes, the king is liable to be deposed or put to death. This widespread African superstition culminated long ago in ancient Egypt, where the kings were treated as gods both in life and in death, temples being dedicated to their worship and priests appointed to conduct it. And when the harvests failed the ancient Egyptians, like the modern negroes, laid the blame of the failure on the reigning monarch.

Nor have such superstitions been confined to savages and other peoples of alien race in remote parts of the world. They seem to have been shared by the ancestors of all the Aryan peoples from India to Ireland. Thus an ancient Indian law-book tells us that "even an infant king must not be despised from an idea that he is a mere mortal; for he is a great deity in human form." Similarly in Homeric Greece kings were called sacred and divine, and it was thought that the reign of a good king caused the earth to bring forth wheat and barley, the trees to be loaded with fruit, the flocks to multiply, and the sea to yield fish. In ancient Ireland it was also believed that when kings observed the customs of their ancestors, the seasons were mild, the crops plentiful, the cattle fruitful, the waters abounded with fish, and the fruit-trees had to be propped up on account of the weight of their produce.

Perhaps the last relic of such superstitions which lingered about our English kings was the belief that they could heal scrofula by their touch. The disease was accordingly known as the king's evil. The superstition survived into the eighteenth century. In his childhood Dr. Johnson was touched for scrofula by Queen Anne.

The foregoing evidence, summary as it is, may suffice to show that many peoples have regarded their rulers, and especially their kings, with superstitious awe as beings of a higher order and endowed with mightier powers than common folk. Imbued with such a profound veneration for their governors, and with such an exaggerated conception of their power, they cannot but have yielded them a prompter and more implicit obedience than if they had known them to be men just like themselves. If that is so, I may claim to have proved my first proposition, which is, that among certain races and at certain times superstition has strengthened the respect for government, especially monarchical government, and has thereby contributed to the establishment and maintenance of civil order.

II. I now pass to my second proposition, which is, that among certain races and at certain times superstition has strengthened the

respect for private property, and has thereby contributed to the security of its enjoyment.

Nowhere perhaps does this appear more plainly than in Polynesia, where the system of taboo reached its highest development; for the effect of tabooing a thing was, in the opinion of the natives, to endow it with a supernatural or magical energy which rendered it practically unapproachable by any but the owner. Thus taboo became a powerful instrument for strengthening the ties—perhaps our socialist friends might say riveting the chains—of private property. Indeed, some good authorities have held that the system of taboo was originally devised for no other purpose. Thus, an Irishman who lived as a Maori with the Maoris, and knew them intimately, says that “the original object of the *tapu* seems to have been the preservation of private property. Of this nature in a great degree was the ordinary personal *tapu*. This form of *tapu* was permanent, and consisted in a certain sacred character which attached to the person of a chief and never left him. It was his birthright, a part of himself, of which he could not be divested. . . . The fighting men and petty chiefs, and every one who could by any means claim the title of. . . . gentleman, were all in some degree possessed of this mysterious quality. It extended or was communicated to their clothes, weapons, ornaments, and tools, and to everything in fact which they touched. This prevented their chattels being stolen or mislaid, or spoiled by children, or used or handled in any way by others. And as in the old times . . . every kind of property of this kind was precious in consequence of the great labour and time necessarily. . . . expended in the manufacture, this form of *tapu* was of great real service. An infringement of it subjected the offender to various dreadful imaginary punishments, of which deadly sickness was one.” Even when the offence had been committed unwittingly the offender not uncommonly died of fright on learning what he had done. So strong was the invisible barrier which hedged the private property of chiefs and gentlemen among the Maoris.

In other parts of Polynesia the system of taboo was the same, and everywhere it tightened, for good or evil, the ties of private property. Thus in the Marquesas Islands the system converted the tabooed or privileged classes into landed proprietors: the land belonged to them alone and to their heirs: common folk lived by the labour of their hands. Taboo was the bulwark of the landowners: it was that alone which elevated them by a sort of divine right into a position of affluence and luxury above the vulgar: it was that alone which insured their safety and protected them from the encroachments of their poor and envious neighbours. “Without doubt,” say the French writers from whom I borrow these observations, “without doubt the first mission of taboo was to establish property, the base of all society.”

In Melanesia also a system of taboo exists, and derives its sanction from a superstition that the chief or other person who imposes a

taboo has the support of a powerful ghost or spirit. Here, too, superstition is a powerful engine for the defence of private property. In New Britain plantations, cocoanut trees, and other possessions are protected against thieves by marks of taboo attached to them, and it is thought that whoever violates the taboo will be visited with sickness or other misfortunes.

A system of taboo based on superstition prevails also all over the Malay Archipelago, and here again superstition strongly enforces the rights of private property. Thus in the island of Timor, as we learn from Dr. Alfred Wallace, "a prevalent custom is the *pomali*, exactly equivalent to the 'taboo' of the Pacific islanders, and equally respected. It is used on the commonest occasions, and a few palm leaves stuck outside a garden as a sign of the *pomali* will preserve its produce from thieves as effectually as the threatening notice of man-traps, spring guns, or a savage dog would do with us."

Again, in Africa, superstition is a priceless ally in the defence of private property. On the coast of Guinea, for example, fetishes are often inaugurated for the express purpose of detecting and punishing certain kinds of theft, and not only the thief himself but any person who knows of his crime and fails to give information is liable to be punished by the fetish. Shadowy as the fetish may seem to us, its protection is most efficient. In this part of Africa solitary patches of corn in the depth of the forest and even articles of property left by the way-side are quite safe if they are protected by a fetish or charm. Similarly in Sierra Leone charms are often placed in plantations to deter people from stealing, and we are told that a few old rags placed on an orange tree will secure the fruit as effectually as if it were guarded by the dragons of the Hesperides. Superstitions of the same sort have been transported by the negroes to the West Indies, where the bravest blacks tremble at the very sight of the ragged bundle, the bottle, or the egg-shells which have been stuck in the thatch or over the door of a hut to deter marauders. If a negro has dared to steal from a house so protected, the owner applies to a magician, who gives out that he has set a charm to catch the thief. If the culprit hears of this and cannot find a more potent magician to take off the spell which the other has cast upon him, his terrified imagination begins to work, and he falls into a decline and dies. Superstition has killed him.

Many similar examples might be adduced, if time permitted. But perhaps I have said enough to show that among many peoples, and in many parts of the world, superstitious fear has operated as a powerful motive to deter men from stealing. If that is so, my second proposition is proved, which is, that among certain races and at certain times superstition has strengthened the respect for private property, and has thereby contributed to the security of its enjoyment.

III. I pass now to my third proposition, which is, that among certain races and at certain times superstition has strengthened the

respect for marriage, and has thereby contributed to a stricter observance of the rules of sexual morality both among the married and the unmarried. That this is true will appear, I think, from the following examples.

Among the Karens of Burma "adultery or fornication is supposed to have a powerful influence to injure the crops. Hence, if there have been bad crops in a village for a year or two, and the rains fail, the cause is attributed to sins of this character; and they say the God of Heaven and earth is angry with them on this account, and all the villagers unite in making an offering to appease Him." And when a case of adultery or fornication has come to light, "the elders decide that the transgressors must buy a hog and kill it. Then the woman takes one foot of the hog, and the man takes another, and they scrape out furrows in the ground with each foot, which they fill with the blood of the hog. They next scratch the ground with their hands and pray: 'God of heaven and earth, God of the mountains and hills, I have destroyed the productiveness of the country. Do not be angry with me. Now I repair the mountains, now I heal the hills, and the streams and the lands. May there be no failure of crops . . . Make Thy paddy fruitful, Thy rice abundant. Make the vegetables to flourish. . . .' After each has prayed thus, they return to the house and say they have repaired the earth." Thus, according to the Karens, adultery and fornication are not simply moral offences which concern only the culprits and their families; they physically affect the course of nature by blighting the fruits of the earth and destroying its fertility. Hence they are public crimes which threaten the existence of the whole community by cutting off its food supply at the root. But the physical injury which these offences do to the soil can be physically repaired by saturating it with pig's blood.

Again, the inhabitants of the hills near Rajamahā, in Bengal, imagine that adultery undetected and unexpiated causes the inhabitants of the village to be visited by a plague or destroyed by tigers or other ravenous beasts. To prevent these evils an adulteress generally makes a clean breast. Her paramour has then to furnish a hog, and he and she are sprinkled with its blood, which is supposed to wash away their sin and avert the Divine wrath. When a village suffers from plague or the ravages of wild beasts, the people religiously believe that the calamity is a punishment for secret immorality, and they resort to a curious form of divination to discover the culprits, in order that the crime may be duly expiated.

The Khasis of Assam are divided into a number of clans which are exogamous, that is, no man may marry a woman of his own clan. Should a man cohabit with a woman of his own clan, it is treated as incest, and is believed to cause great disasters: the people will be struck with lightning or killed by tigers, the women will die in child-bed, and so forth. The guilty couple is taken to a priest and obliged

to sacrifice a pig and a goat ; after that they are made outcasts, for their offence is inexpiable.

The Battas of Sumatra, in like manner, think that if an unmarried woman is found with child, she must be given in marriage at once, even to a man of lower rank ; for otherwise the people will be infested with tigers, and the crops in the fields will not be abundant. The crime of incest, in their opinion, would blast the whole harvest, if the wrong were not speedily repaired. Epidemics and other calamities which affect the whole people are almost always traced by them to incest, by which is to be understood any marriage that conflicts with their customs.

Similar views prevail among the tribes of Borneo. Thus in regard to the Sea Dyaks we are told that immorality among the unmarried is supposed to bring a plague of rain upon the earth as a punishment inflicted by the god Petara. It must be atoned for with sacrifice and fine, and the offenders are banished from their homes. The atonement consists in purifying the earth with pig's blood, which appears to many savages, as sheep's blood appeared to the ancient Hebrews, to possess the valuable property of atoning for moral guilt. Not long ago the offenders whose sin had thus brought the whole country into danger would have been punished with death or at least slavery.

The Bahau, another tribe in the interior of Borneo, believe that adultery is punished by the spirits, who visit the whole tribe with failure of the crops and other misfortunes. Hence, to avert these evil consequences from the innocent members of the tribe, the two culprits, with all their possessions, are placed on a gravel bank in the middle of the river. Then pigs and fowls are killed, and with the blood priestesses smear the property of the guilty pair in order to disinfect it. Finally the two are placed on a raft and set floating down the stream.

Among the Macassars and Bugineese of Southern Celebes incest is a capital crime ; but the blood of the guilty pair may not be shed, for the people think that, were the ground to be polluted by the blood of such criminals, the rivers would dry up, and the supply of fish would run short, the harvest and the produce of the gardens would miscarry, edible fruits would fail, sickness would be rife among cattle and horses, civil strife would break out, and the country would suffer from other widespread calamities. Hence the punishment of the guilty is such as to avoid the spilling of their blood. Usually they are tied up in a sack and drowned in the sea. Some tribes of Central Celebes believe that if the blood of persons guilty of incest were to fall upon the ground, the rice would never grow again. So they drown or throttle the culprits. When it rains in torrents, the Galelareese of Halmahera say that near kinsfolk are having illicit relations with each other, and that every human being must be informed of it, for then only will the rain cease to descend. The superstition has repeatedly caused blood relations to be accused of

incest. Further, the people think that alarming natural phenomena, such as a violent earthquake or the eruption of a volcano, are caused by crimes of this sort. Persons charged with these offences are brought to Ternate ; it is said that formerly they were often drowned or thrown into the volcano.

In some parts of Africa also it is believed that breaches of sexual morality disturb the course of nature, particularly by blighting the fruits of the earth. Thus, the negroes of Loango imagine that the commerce of a man with an immature girl is punished by God with drought and consequent famine, until the transgressors expiate their transgression by dancing naked before the king and an assembly of the people, who throw hot gravel and bits of glass at the pair as they run the gauntlet.

But in the opinion of many savages the effect of sexual immorality is not merely to disturb the balance of nature by blasting the crops, causing the earth to quake, and so forth ; the delinquents themselves, their offspring, or their innocent spouses are supposed to suffer in their own person for the sin that has been committed. Thus, the Baganda of Central Africa believe that if a wife who is with child by her husband commits adultery, she will either die in childbed or go mad and attempt to kill and devour her offspring. Further, they think that if, after the child is born and before it is named, either husband or wife proves unfaithful to the other, their child will die, unless the medicine-man saves its life by a magical ceremony. Again, it appears to be a common superstition that the infidelity of a wife prevents her husband from killing game, and even exposes him to the risk of being himself killed by wild beasts. Malagasy women suppose that if they are unfaithful to their husbands while the men are away at the wars, the absent spouse will be wounded or killed in action ; and this superstition is said to act as a restraint on the wives at home. The Zulus imagine that an unfaithful wife who touches her husband's furniture without first eating certain herbs causes him to be seized with a fit of coughing, of which he soon dies. These savages are also of opinion that the mere presence of an adulterer or adulteress in the sick chamber of the person whom they have wronged will cause the sufferer to die. This superstition also is said to act as a deterrent on Zulu women. Lastly, among the Sulka of New Britain unmarried persons who have been guilty of unchastity are believed to contract thereby a fatal pollution of which they will die, if they do not confess their fault and undergo a public ceremony of purification. Such persons are avoided : no one will take anything at their hands : parents point them out to their children and warn them not to go near them. The infection which they are supposed to spread is apparently conceived as physical rather than moral : for special care is taken to keep the paraphernalia of the dance out of their way, the mere presence of persons so polluted being thought to tarnish the paint on the instruments. But by a public ceremony of purification

the culprits can save their lives, which otherwise must have been destroyed by their unchastity.

These examples may serve to show that among many races sexual immorality, whether in the form of adultery, fornication, or incest, is believed in itself to entail, without the intervention of society, most serious consequences, not only on the culprits themselves, but on the community, often indeed to menace the very existence of the whole people by destroying the food supply. Wherever this superstition (for, of course, it is a pure superstition) has existed, it must have served as a powerful motive to deter men from adultery, fornication and incest. If that is so, then I think that I have proved my third proposition, which is, that among certain races and at certain times superstition has strengthened the respect for marriage, and has thereby contributed to a stricter observance of the rules of sexual morality, both among the married and the unmarried.

IV. I pass now to my fourth and last proposition, which is, that among certain races and at certain times superstition has strengthened the respect for human life, and has thereby contributed to the security of its enjoyment.

The particular superstition which has had this salutary effect is the fear of ghosts, especially the ghosts of the murdered. The fear of ghosts is widespread, perhaps universal, among savages: it is hardly extinct even among ourselves. If it were extinct, some learned societies might put up their shutters. Dead or alive, the fear has certainly not been an unmixed blessing. Indeed it might with some show of reason be maintained that no belief has done so much to retard the economic and thereby the social progress of mankind as the belief in the immortality of the soul: for this belief has led race after race, and generation after generation, to sacrifice the real wants of the living to the imaginary wants of the dead. The waste and destruction of life and property which this creed has entailed are enormous and incalculable. But I am not here concerned with the many disastrous and deplorable consequences which have flowed in practice from the theory of a future life: my business at present is with the more cheerful aspect of a gloomy subject—I mean with the wholesome, though groundless, terror which ghosts, apparitions, and spectres strike into the breasts of hardened ruffians and desperadoes. So far as such persons reflect at all, and regulate their passions by the dictates of prudence, it seems plain that a fear of ghostly retribution, of the angry spirit of their victim, must act as a salutary restraint upon their disorderly impulses: it must reinforce the dread of purely secular punishment, and supply the choleric and the malicious with a fresh motive for pausing before they inbrue their hands in blood. This is so obvious, and the fear of ghosts is so notorious, that both might perhaps be taken for granted, especially at this late hour of the evening. But for the sake of completeness I will give a few illustrations.

The ancient Greeks believed that the soul of any man who had just been killed was angry with his slayer and troubled him ; hence even an involuntary homicide had to depart from his country for a year until the wrath of the dead man had cooled down ; nor might the slayer return until sacrifice had been offered and ceremonies of purification performed. If his victim chanced to be a foreigner, the homicide had to shun the country of the dead man as well as his own. The legend of the matricide Orestes, how he roamed from place to place, pursued and maddened by the ghost of his murdered mother, reflects faithfully the ancient Greek conception of the fate which overtakes the murderer at the hands of the ghost. The Karens of Burma think that the ghosts of all who have died by violence remain on earth and roam about invisible, stealing the souls of men and so visiting the sufferer with mortal sickness. Accordingly these vampire-like beings are exceedingly dreaded by the people, who sacrifice to them and offer the most earnest prayers and supplications to avert their wrath and cruel assaults.

However, it is not always by fair words and propitiatory offerings that the community seeks to rid itself of these invisible but dangerous intruders. When the North American Indians had burned and tortured a prisoner to death, they used to run through the village, beating the walls, the furniture, and the roofs of the huts and yelling at the pitch of their voices to drive away the angry ghost of their victim, lest he should seek to avenge the injuries done to his scorched and mutilated body. Similarly, among the Papuans of Doreh, in Dutch New Guinea, when a murder has been committed in the village, the inhabitants assemble for several evenings successively and shriek and shout to frighten away the ghost, in case he should attempt to come back. The Yabim, a tribe in German New Guinea, believe that "the dead can both help and harm, but the fear of their harmful influence is predominant. Especially the people are of opinion that the ghost of a slain man haunts his murderer, and brings misfortune on him. Hence it is necessary to drive away the ghost with shrieks and the beating of drums. The model of a canoe laden with taro and tobacco is got ready to facilitate his departure." The Fijians used to bury the sick and aged alive, and having done so they always made a great uproar with bamboos, shell-trumpets, and so forth, in order to scare away the spirits of the buried people and prevent them from returning to their homes ; and by way of removing any temptation to hover about their former abodes, they dismantled the houses of the dead and hung them with everything that in their eyes seemed most repulsive.

It would be easy, but superfluous, to multiply evidence of the terror which a belief in ghosts has spread among mankind. The preceding examples may suffice for my present purpose, which is merely to indicate the probability that this widespread superstition has served a useful purpose by enhancing the sanctity of human life.

For it is reasonable to suppose that men are more loth to spill the blood of their fellows when they believe that by so doing they expose themselves to the vengeance of an angry and powerful spirit whom it is difficult either to evade or to deceive. Fortunately in this matter we are not left wholly to conjecture. In the vast empire of China, as we are assured by the best living authority on Chinese religion, the fear of ghosts has actually produced this salutary result. Among the Chinese the faith in the existence of the dead, in their power to reward kindness and punish injury, is universal and inveterate: it has been handed down from an immemorial past, and it is nourished in the experience, or rather in the mind, of everybody by hundreds of ghost-stories, all of which are devoutly accepted as true. Nobody doubts that ghosts may interfere at any moment in the conduct of life, in the regulation of human destiny. This faith of the Chinese in the existence and power of the dead, we are informed, "indubitably exercises a mighty and salutary influence upon morals. It enforces respect for human life, and a charitable treatment of the infirm, the aged and the sick, especially if they stand on the brink of the grave. Benevolence and humanity, thus based on fears and selfishness, may have little ethical value in our eye; but for all that, their existence in a country where culture has not yet taught man to cultivate good for the sake of good alone, may be greeted as a blessing. Those virtues are even extended to animals, for, in fact, these too have souls which may work vengeance, or bring reward. But the firm belief in ghosts and their retributive justice has still other effects. It deters from grievous and provoking injustice, because the wronged party, thoroughly sure of the avenging power of his own spirit when disembodied, will not always shrink from converting himself into a wrathful ghost by committing suicide," in order to wreak in death that vengeance which he could not exact in life. Cases of suicide committed with this intention are said to be far from rare in China. These beliefs, says Professor de Groot, put disrespect for human life under great restraint: in particular they have a salutary effect in restricting the practice of female infanticide. The fear that the souls of the murdered little ones may bring misfortune induces many a Chinese father and mother to spare the infant daughters whom otherwise they would have killed at birth. Humane and wealthy people take advantage of these superstitious fears to inculcate a merciful treatment of female infants; for they print and circulate gratuitously tracts which set forth many gruesome examples of punishments inflicted on unnatural fathers and mothers by the ghosts of their murdered daughters. These highly-coloured narratives, though they bear all the marks of a florid fancy, are said to answer their benevolent purpose perfectly; for they sink deep into the credulous minds to which they are addressed: they touch the seared conscience and the callous heart which no appeal to mere natural affection could move to pity.

But while the fear of the ghost has thus operated directly to

enhance the sacredness of human life by deterring the cruel, the passionate, and the malignant from the shedding of blood, it has operated also indirectly to bring about the same salutary result. For not only does the hag-ridden murderer himself dread his victim's ghost, but the whole community dreads it also and believes itself endangered by the murderer's presence, since the wrathful spirit which pursues him may turn on other people and rend them. Hence society has a strong motive for secluding, banishing, or exterminating the culprit in order to free itself from what it believes to be an imminent danger, a perilous pollution, a contagion of death. To put it in another way, the community has an interest in punishing homicide. Not that the treatment of homicides by the tribe or state was originally conceived as a punishment inflicted on them: rather it was viewed as a measure of self-defence, a moral quarantine, a process of spiritual purification and disinfection, an exorcism. It was a mode of cleansing the people, and sometimes the homicide himself, from the ghostly infection, the atmosphere of death, which to the primitive mind appears to be something material and tangible, something that can be literally washed or scoured away by water, pig's blood, sheep's blood, or other detergents. But when the purification took the form of laying the homicide under restraint, banishing him from the country, or putting him to death in order to appease his victim's ghost, it was for all practical purposes indistinguishable from punishment, and the fear of it would act as a deterrent just as surely as if it had been designed to be a punishment and nothing else. When a man is about to be hanged it is little consolation to him to be told that hanging is not a punishment but a purification. But the one conception slides easily and almost imperceptibly into the other, so that what was at first a religious rite, a solemn consecration or sacrifice, comes in course of time to be a purely secular function, the penalty which society exacts from those who have injured it: in short, the sacrifice becomes an execution, the priest steps back and the hangman comes forward. Thus, criminal justice was probably based in large measure on a crude form of superstition long before the subtle brains of jurists and speculative philosophers deduced it logically, according to their various predilections, from a rigid theory of righteous retribution, a far-sighted policy of making the law a terror to evil-doers, or a benevolent wish to reform the criminal's character and to save his soul in another world by hanging or burning his body in this one.

If these views are correct, the fear of the ghost has operated in a two-fold way to protect human life. On the one hand, it has made every individual for his own sake more reluctant to slay his fellows, and, on the other hand, it has roused the whole community to punish the slayer. It has placed every man's life in a double ring fence of morality and law. The hot-headed and the cold-hearted have been furnished with a double motive for abstaining from the last fatal step:

they have had to fear the spirit of their victim on the one side, and the lash of the law on the other: they are in a strait between the devil and the deep sea, between the ghost and the gallows. And when with the progress of thought the shadow of the ghost passes away, the grim shadow of the gallows remains to protect society without the aid of superstitious terrors. It is thus that custom often outlives the motive which originated it. If only an institution is good in practice, it will stand firm after its old theoretical basis has been shattered: a new and more solid, because a truer, foundation will be discovered for it to rest upon. More and more, as time goes on, morality shifts its ground from the sands of superstition to the rock of reason, from the imaginary to the real, from the supernatural to the natural. In the present case, the State has not ceased to protect the lives of the peaceful citizens because the faith in ghosts is shaken. It has found a better reason than old wives' fables for guarding with the flaming sword of Justice the approach to the Tree of Life.

To sum up this brief review of the influence which superstition has exercised on the growth of institutions, I think I have proved, or at least made probable, that—

I. Among certain races and at certain times superstition has strengthened the respect for government, especially monarchical government, and has thereby contributed to the establishment and maintenance of civil order.

II. Superstition has strengthened the respect for private property, and has thereby contributed to the security of its enjoyment.

III. Superstition has strengthened the respect for marriage, and has thereby contributed to a stricter observance of the rules of sexual morality, both among the married and the unmarried.

IV. Superstition has strengthened the respect for human life, and has thereby contributed to the security of its enjoyment.

But government, private property, marriage, and respect for human life are the pillars on which rests the whole fabric of civil society. Shake them, and you shake society to its foundations. Therefore, if government, private property, marriage, and respect for human life are all good and essential to the very existence of civil society, it follows that superstition, by strengthening every one of them, has rendered a great service to humanity. It has supplied multitudes with a motive—a wrong motive, it is true—for right action; and surely it is better for the world that men should do right from wrong motives, than that they should do wrong with the best intentions. What concerns society is conduct, not opinion: if only our actions are just and good, it matters not a straw to others whether our opinions be mistaken. The danger of false opinion, and it is a most serious one, is that it commonly leads to wrong action; hence it is unquestionably a great evil, and every effort should be made to correct it. But of the two evils, wrong action is, in itself, infinitely worse than

false opinion ; and all systems of religion or philosophy which lay more stress on right opinion than on right action, which exalt orthodoxy above virtue, are so far immoral and prejudicial to the interests of mankind : they invert the true relative importance, the real ethical value, of thought and action, for it is by what we do, not by what we think, that we are useful or useless, beneficent or maleficent, to our fellows. As a body of false opinions, superstition is indeed a most dangerous guide in practice, and the evils which it has wrought are incalculable. But vast as are these evils, they ought not to blind us to the benefit which superstition has incidentally conferred on society by furnishing the ignorant, the weak, and the foolish with a motive—bad though it may be—for good conduct. It is a reed, a broken reed, which has yet supported the steps of many a poor erring brother who but for it might have stumbled and fallen. It is a light, a dim and wavering light, which, if it has lured many a mariner on the breakers, has yet guided some wanderers on life's troubled sea into a haven of rest and peace. Once the harbour lights are passed and the ship is in port, it matters little whether the pilot steered by a jack-o'-lantern or by the stars.

That, ladies and gentlemen, is my plea for Superstition. Perhaps it might be urged in mitigation of the sentence which will be passed on the hoary-headed offender when he stands at the judgment bar. Yet the sentence, do not doubt it, is death. But it will not be executed in our time. There will be a long, long reprieve. It is as his advocate, not as his executioner, that I have appeared before you to-night. At Athens cases of murder were tried before the Areopagus by night, and it is by night that I have spoken in defence of this power of darkness. But it grows late, and with my sinister client I must vanish before the cocks crow and the morning breaks grey in the east.

[J.G.F.]

WEEKLY EVENING MEETING,

Friday, February 12, 1909.

HIS THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L.
Sc.D. F.R.S., President, in the Chair.

PROFESSOR HAROLD ALBERT WILSON, M.A. D.Sc. F.R.S. *M.R.I.*

The Electrical Properties of Flame.

IF a flame is brought near to an insulated conductor charged with electricity, the charge disappears. This is explained by supposing that the gases in the flame are partially dissociated into ions. A neutral molecule splits up into two ions, one having a negative charge and the other a positive charge. The conductor, if positively charged, attracts the negative ions out of the flame, and their charges when they reach it neutralise its charge.

If a plate of an insulator, such as ebonite, is placed between the flame and the charged conductor, the ions are still attracted through the plate; but when they reach it they cannot get through, and so remain on its surface. The side of the plate turned towards the flame thus gets a charge of opposite sign to that on the conductor. This shows that the disappearance of the charge in the first case was due to an opposite charge attracted out of the flame, and not to the charge on the conductor escaping into the flame.

We have a stream of gas rising from the flame, and the ions go up in the stream. The ions of opposite sign attract one another, and when two come together their charges are neutralised and the two ions are said to have disappeared by recombination. Thus, as we go up in the stream of gas from the flame the number of ions diminishes. If the stream of gas is allowed to pass up a long tube containing along its axis a series of charged electrodes, then the bottom electrode will be discharged first, and then the next one, and so on. The ions are used up in discharging the electrodes, so that the electrodes are discharged in order, beginning with the lowest one. When the lower electrodes have been discharged, the upper ones begin to be discharged, but more slowly, because many of the ions disappear by recombination before they get far up the tube. Another effect also comes in; as the gases cool down the ions do not move so freely through them, and so are not so easily attracted by the electrodes. This makes the rate of discharge of the upper electrodes still slower.

Thus, as we go down towards the flame the number of ions and their mobility rapidly increases, and right inside the flame the number is so large that the flame behaves like a good conductor of electricity.

If the terminals of an induction coil are connected to two Bunsen

burners, sparks can be passed from the tip of one flame to the tip of the other. The temperature of the flame is about $2000^{\circ}\text{C}.$, so that the density of the gases in it is about one-seventh of their density at the ordinary temperature. The potential difference required to send a spark through the flame is about the same as that required to send a spark through an equal length of air at one seventh of ordinary atmospheric pressure. It appears therefore that the ions do not make it easier for a spark to pass. This is due to the fact that the current in the spark is greater than the ions can carry, so that the potential difference has to be enough to produce more ions, and so is the same in the flame as in unionised air at the same density.

To study the conductivity of flame, it is convenient to use a row of small Bunsen flames placed so that they touch each other. I use a row of fifty flames burning from quartz tubes 1 cm. apart. This gives a flame 50 cm. long, and about 10 cm. high. The quartz tubes insulate very well, so that a current can be passed along the flame horizontally from one end to the other.

If two parallel platinum electrodes immersed in the flame are connected to a galvanometer and battery, it is found that a measurable current is obtained. The relation between the current (i) and the difference of potential (V) between the electrodes is given by the equation $V = A\bar{i}^2 + B\bar{i}$ where A and B are constants and \bar{i} denotes the distance between the electrodes. If \bar{i} is small, say one or two millimetres, the term $B\bar{i}$ is negligible (except when i is very small), and we get $V = A\bar{i}^2$. In this case the current is almost independent of the distance between the electrodes.

The reason for this peculiar relation between the current and potential difference becomes apparent when the variation of the potential along the flame from one electrode to the other is examined. An electrometer is connected to two platinum wires, which are immersed in the flame and can be moved along horizontally between the electrodes. Each wire takes up the potential of the flame at the point where the wire is situated, so the deflection of the electrometer indicates the difference of potential between the two points where the wires are put in. Suppose one wire is allowed to touch the positive electrode and the other is gradually moved along the flame from the positive to the negative electrode. It is found that in the space between the electrodes there is a small uniform potential gradient, but near each electrode there is a comparatively sudden drop in the potential. The drop near the negative electrode is much larger than the drop near the positive electrode. Thus, nearly all the electromotive force of the battery is used up close to the negative electrode. This shows that nearly all the resistance offered by the flame to the passage of the current is close to the negative electrode. The positive ions in the flame move towards the negative electrode and the negative ions towards the positive electrode, in fact the current is carried through the flame by these two streams of ions. Hence, close to the

negative electrode the current must be carried entirely by positive ions moving towards it, and at the positive electrode the current must be entirely carried by negative ions. We find that the resistance near the negative electrode is much greater than near the positive electrode, so that we conclude that the negative ions carry the current more easily than the positive ions. With a given electric force, the negative ions move very much faster than the positive ions. It has been shown experimentally that the velocity of the negative ions is about 10,000 cm. per sec. for one volt per cm., while that of the positive ions is about 100 times smaller than this.

In the flame away from the electrodes the electric force is found to be proportional to the current, so that here the flame obeys Ohm's law like a metallic conductor. Its conductivity is about $10''$ times less than that of copper. In the equation $V = Ai^2 + Bdi$, the term Bdi is the part of the E.M.F. used up between the electrodes, so it is proportional to the current and to the distance. Professor Sir J. J. Thomson has shown theoretically that the drop of potential near the electrodes should be proportional to the square of the current, as is found experimentally to be the case.

The conductivity of a Bunsen flame may be compared with the conductivity of liquids, such as water. In pure water some of the molecules are dissociated into ions and the water is a conductor, although only a poor one. But if a salt like sodium chloride is dissolved in the water the salt dissociates into ions almost completely, and the conductivity is greatly increased. Suppose we hold a bead of salt on a platinum wire in a flame, then the salt volatilises and the flame is filled with its vapour, and, just as with the water, the conductivity is enormously increased.

With the long flame and an electrode at each end, we can try the effect on the current of putting salt in different parts of the flame between the electrodes. In this way it is easy to show that the current is practically unchanged, unless the salt vapour is put in close to the negative electrode, but in that case it produces a very great increase in the current. This confirms the conclusion that nearly all the resistance to the passage of the current is situated close to the negative electrode. When the salt is put in anywhere it diminishes the resistance there to a small fraction of its value, but it is only close to the negative electrode that the diminution in the total resistance is appreciable. If we measure the potential difference between two points in the flame away from the electrodes, and then put salt vapour in the flame between them, we find that the P.D. drops to a small fraction of its value although the current is the same as before. This shows clearly that the salt vapour greatly increases the conductivity wherever it is put in.

If some salt is put on the negative electrode, the sudden drop in potential there almost disappears, and we get a nearly uniform potential gradient from one electrode to the other, so that now the resist-

ance is nearly uniformly distributed along the flame. If now salt vapour is put in anywhere between the electrodes the current is increased. If, for example, we fill half the length of the flame with salt vapour, we nearly double the current.

When salt is put on one electrode, the flame can be used as a rectifier for an alternating current, for when the salted electrode is negative the resistance of the flame is much smaller than when it is positive.

I have measured the conductivities of a number of alkali salt vapours in a current of air flowing along a platinum tube heated in a gas furnace. An electrode was fixed along the axis of the tube, and the current from it through the salt vapour to the surrounding tube was measured with a galvanometer. It was found that at temperatures above 1400°C. , and with electro-motive forces of about 1000 volts, the current was proportional to the amount of salt passing through the tube, and for different salts in equal quantities inversely proportional to the electrochemical equivalent of the salt. This shows that the quantity of electricity per molecule of salt is the same for all salts. It was also found that the quantity of electricity carried per molecule was equal to that carried per molecule when a solution of salt in water is electrolysed. It appears therefore that the laws of electrolysis discovered by Faraday for liquids apply also to salts in the state of vapour.

When a molecule of salt like sodium chloride dissociates into two ions in water, the sodium atom forms the positive ion and the chlorine atom the negative ion, and when a current is passed through the solution the sodium is attracted to the negative electrode and the chlorine goes to the positive electrode. We might expect the same thing to happen when a current is passed through the salt vapour in a flame. If we put two wires in the flame, and put some sodium salt on one and then connect them to an induction coil, and pass a discharge from the salted one to the other, we find that the yellow sodium vapour appears at it when it is the negative pole but not when it is positive. This shows that in the flame the positive ions of the salt vapour contain the metal just as they do in solutions. The negative ions, however, do not appear to be the same in flames as in solutions. In flames the very high velocity of the negative ions indicates that they are the electrons whose properties have been investigated in vacuum tubes by Sir William Crookes and Sir Joseph Thomson. The positive ion, then, is an atom or molecule, while the negative ion is an electron, the mass of which is several thousand times smaller. This is the explanation of the fact that the negative ions move 100 times more quickly than the positive ions.

[H.A.W.]

WEEKLY EVENING MEETING,

Friday, February 19, 1909.

HIS ROYAL HIGHNESS THE PRINCE OF WALES, K.G. G.C.M.G.
G.C.I.E. G.C.V.O. P.C. LL.D. F.R.S., Vice Patron,
in the Chair.

SIR HENRY CUNYNGHAME, K.C.B. M.A. M.I.E.E. M.R.I.

Recent Advances in Means of Saving Life in Coal Mines.

My connection at the Home Office during the last sixteen years with the administration of the statutes relating to mining has afforded me a unique opportunity of becoming acquainted with various improvements whereby that industry is rendered more safe. I propose briefly to describe some of the difficulties that have been encountered, and the means by which they have been met. I think it must be admitted that no like period in the past has afforded an example of similar progress.

I shall have the pleasure of mentioning many names of engineers and others now living. It has been one of the principal pleasures of my life to have been enabled constantly to consult them, and to gather from the lips of our leading experts what I could never have learned from books and papers only. I propose to select from the mass of material at my disposition four subjects of interest, namely, improvements in safety lamps, the use of explosives in mines, explosions of gas and coal dust, and apparatus for enabling men to work in poisonous atmospheres.

Before I enter into details, I shall venture to give a brief description of work in a mine, dealing only with such points as are necessary to make the lecture understood by those who have no specially technical knowledge of the subject. I have only a few minutes to spare on this head, and therefore I hope it will not be tedious.

In Great Britain, coal lies in the earth in layers or seams, between a floor and roof. These seams are fortunately generally horizontal. When coal is to be extracted, the first thing is to make two shafts from the surface down to the lowest part of the seam, provided with huge passenger and coal lifts worked by an engine at the surface by means of ropes of hemp or of steel.

The lifts or "cages" go with great speed. At the centre of their journey they sometimes travel at the rate of half a mile, or even a mile a minute; yet they alight like a feather at the bottom. The winding is done by the manipulation of a lever by an engine-winder.

on whose skill and attention depends the safety of all who descend the mine. There are difficulties in the way of automatic cage-winding which I believe will be overcome in the future.

[Photograph.]

When the visitor arrives at the bottom of the mine, his eyes, unaccustomed to the darkness, can see but little. He seems to be in a huge coal-cellar with long galleries running out of it, in which appear lights twinkling in the distance carried by men whose dress is as black as their faces.

Every mine has two shafts. The reason of this is to secure ventilation. For the seams of coal give off gas, and this, added to the contamination of the air by the breath of the workmen, needs a constant renewal of fresh air, that goes down one shaft called the down cast, and comes back up the other called the up cast. The draught used once to be obtained by fires and tall chimneys. Now it is done by means of ventilating fans. From the down cast shaft leads the main haulage road. From this the stall roads branch away.

[Diagram.]

The diagram shows a simple district of a mine. A main haulage road leads up close to the place where the coal is being got. Branches into it serve to bring the coal back to the down cast shaft. The ventilating air runs down the shaft, then along the main haulage road, then along the coal face, and then turns back through the return air-way. Of course it follows that the purest air is along the main haulage road, the air in the return air-way being corrupted with the foul gases of the mine. Therefore, people entering the mine go along the main haulage road, and return the same way, the return air-way not being commonly used for traffic.

There are many ways of getting the coal. I will describe one called the long-wall system. Along the coal face a number of men are distributed, six feet or more apart. The coal is got out by each man working away straight in front of him; so that the men advance against the coal face like a line of skirmishers, and the coal face retires before them.

The roof is supported by timber props, and a line of rails runs along the front of the face, parallel to the line of men.

The first operation is holing, that is the undercutting of the coal close to the floor: this is done by men with picks. They have to get right in under the coal, and to prevent its falling on them they put sprags. They usually work nearly naked to the waist, on account of the heat. The work is exhausting; they are bathed in perspiration, from which there are two results, one that they are clean physically

(for nothing is so cleansing as perspiration), and the other is, that miners very rarely die of gout.

The working model here illustrates the method.

[Working model.]

From this we see that in long-wall working, the operation is as though an army of ants marched forward in line, eating the meat out of a sandwich and letting the upper layer of bread fall down upon the lower layer behind them.

All this time the ventilation is steadily carrying away the foul air from the face, for of course, where the new surfaces of coal are exposed, there most of the gas exudes. Firemen constantly examine the roof and face for gas, in a manner which I will presently describe.

Here, too, most of the deaths occur. Urged by a desire to get their work done, some of the men are not always careful to set sprags and props, and from time to time masses of loosened coal fall on an unfortunate miner. Great experience is necessary to watch the splitting coal, and the bending roof, whose weight often crushes the timbers and cracks them into match-wood. I here show some photographs of the workings and of men at work.

[Twelve photographs.]

In former days women and children assisted under ground in the mines. The work was hard and rough. I do not know that their lives can be described as wretched. They were strong and healthy, but the labour was unsexing and quite unsuitable for them, and we may now feel glad that no women or children are allowed to be below ground. The six slides are taken from the Report of the Royal Commission on the labour of women and children. They show the former laborious methods of getting the coal.

[Six photographs of women, etc.]

Now machinery is taking the place of human labour in mines as elsewhere, and coal-cutters are beginning to be employed, driven either by compressed air or by electricity. They consist of large wheels armed with teeth, that rotate, and, like a huge circular saw, held horizontally, hole out the coal. Sometimes they consist of bars of metal armed with teeth. There are, in fact, various forms by which the bottom of the seam is scratched out, so as to allow the

superincumbent mass of coal to fall. Here are two slides representing coal cutters at work.

[Five photographs of coal cutters and other machinery.]

The future of coal mining, of course, lies in the use of machinery, which both saves labour and also avoids many dangers.

Having thus endeavoured to give an elementary idea of work in a mine, I come to the main subject of my lecture, namely, the dangers attendant upon it.

If from 5 to 16 per cent. of fire-damp, which is a sort of coal gas, is mixed with air and ignited, an explosion of more or less violence takes place, but when the mixture is either too poor or too rich in gas, it will not explode.

[Experiment.]

It is desirable that gas should never be allowed in a mine in a proportion of more than about 2 per cent., without immediate means being taken for its removal.

The earliest mode of lighting in a mine was the simple expedient of a tallow candle stuck into a lump of clay and dabbed on anywhere, most often on to the front of the workman's hat.

The dangers of gas explosions led to attempts to supersede the use of naked lights. The first of these consisted of a steel wheel turned by hand, against which a flint was pressed so as to emit a shower of sparks. This gave a feeble light, but it was soon discovered that it would ignite gas, and was therefore abandoned.

The need for a safe lamp became imperative. In 1816 Sir Humphrey Davy, Professor of Chemistry to this Institution, attacked the problem, namely, to convey air to a light in such a manner that, though the air was contaminated with gas, yet the flame of the lamp would not be communicated to the gas outside.

It first occurred to Davy to try a long tube as an inlet. This he did with success, for it was found that a flame from an explosive mixture would not, naturally, traverse a copper tube $\frac{1}{4}$ of an inch in diameter.

The reason of this apparently curious fact is, that the copper sucks the heat away so fast, that the temperature of the flame which impinges against it, is reduced to a point too low to burn. The smaller the tube, the greater the effect. So that, if we took a sort of network of very small bits of tube piled upon one another, you

[Diagram.]

would have, as it were, a flame-sieve. From this, it was but a step to substitute a gauze, for it was found that flame would not pass

though a gauze made of sound iron or copper wires 28 to the inch. This experiment is easy to show. Here I have a hand screen made of iron wire gauze 30 meshes to the inch, and you see that it is practically impossible to get the flame through it.

[Experiment.]

The lamp as designed by Davy was of the form shown here, and

[Davy lamp.]

I am enabled to exhibit the actual lamps used by Sir Humphrey Davy in his experiments, and which have been preserved here as mementos of that distinguished discoverer.

The Davy lamp is now provided with a glass, and the gauze screened by an iron bonnet, to prevent accidental injury.

A lamp easily goes out in a mine if you carry it carelessly. The question then arises, how is it to be relit without using a naked flame in the mine. Some are relit by an electric spark got from an electric machine in the mine, so arranged that while the lamp is being lit it is enclosed in an airtight iron chamber. Some again are self-lighting.

Here is a Wolf self-lighting lamp, such as are used in Germany.

[Experiment.]

When air containing gas surrounds a flame, a peculiar appearance is given to it. When the flame of a common lamp is turned very low and gas is present, a faint sort of halo is seen over the flame. The diagram shows the appearances of the flames. Here I have a

[Diagram.]

lamp placed in a glass box to which gas can be admitted. You can see when the gas percentage rises, the gradual rise of the flame.

[Experiment.]

The duty of repeatedly testing for gas is of course incumbent on the officials in a mine, and when gas is seen the men should at once be withdrawn and the ventilation increased so as to clear it away.

Outbursts of gas called blowers occasionally defy even the greatest precaution.

In the model of working coal the position was shown for the hole in which the explosive is put to bring down the coal. This operation of blasting is called "firing a shot." The best explosive

to use is gunpowder, so far as the getting of the coal is concerned, for its action is more of a gentle push than an explosion, whereas the explosives made with gun-cotton and similar compounds are very sharp and shattering in their action, and thus make the coal into dust. This is the reason why it is so difficult to adapt high explosives for use in ordinary fowling-pieces. The old black powder is dirty, but is the safest.

But in a mine quite the contrary is the truth. For gunpowder has the greatest capacity of all explosives for igniting gas. When a shot is to be fired in a mine, the requisite quantity of the explosive with the fuse or detonator is put in the hole, which is then filled up and rammed or stemmed with clay. The fuse is then ignited either by means of a train of powder contained in a long paper tube like a firework, or else according to the more modern and better methods, fired by electricity. Here is an ordinary electric fuse.

[Experiment.]

The efficacy of the action of the shot in bringing down the coal is dependent on the hole being well rammed or "stemmed" with good clay. If the stemming is insufficient, the explosion blows it out like a charge from a gun, and little work is done on the coal. This is called a "blown-out" shot. And, moreover, it is a curious thing that the more you keep an explosive to its work the less opportunity is given it of setting light to gas. I was almost tempted to say that Satan finds some mischief still for blown-out shots to do; for it is almost always the blown-out shots by which gas is fired. This fact has led in France, England and Germany to the use of a machine for testing explosives. It consists of a tunnel of iron 2 ft. 6 in. in diameter and 28 feet long. This is supposed to represent a gallery in a mine. At the end of the gallery is a gun made with small bore and tremendous strength, being wound with steel wire. The explosive to be tested is put into the gun, which is then rammed full of fine clean clay. The tunnel is filled with an explosive mixture of 15 per cent. of air; a paper diaphragm being put over the end to keep the gas in.

The operators then retire behind a screen and fire the shot. If the quality of the explosive is unsuitable, the gas explodes as well as the charge, and with a loud report, and flame can be seen through small glass windows fixed in the side of the tube.

An explosive must pass this test successfully 20 times. Inasmuch as blown-out shots only occur rarely, perhaps once in five or ten thousand firings, with a safety explosive one should only get a blown-out shot capable of firing gas on an average once in 120,000 times. If you add to this, that it is strictly incumbent on men to test carefully so that no trace of gas is present when the shot is fired, you will see that explosions of gas ought to be exceedingly rare, and

so they are. But, in spite of care, accidents will occasionally happen, though we may be thankful to say that of late years an immense improvement is taking place in mining, and accidents are becoming more few. In this connection you may be interested to see a diagram showing as years go on the decreasing proportion of accidents to the quantity of coal got, and to the number of men employed.

[Diagram.]

The machine I referred to is shown in the slide. It was designed by Captain Thomson, the late Chief Inspector of Explosives, whose lamented death deprived us of one of the most brilliant men of science among the inspectorate.

[Two photographs.]

I have now to enter upon what I think is the most interesting portion of my paper, namely, the advance that has recently been made in our knowledge of the causes of explosions in mines and of the way in which the miners meet their death.

For many years after the discovery of the safety lamp explosions in mines were always attributed to the presence of fire-damp and to its accidental ignition, and the deaths of the men whose bodies were found blackened and singed to the violence of the explosion.

One fact was noticed as curious, namely, that when the bodies were brought to the surface, instead of exhibiting the pallor of death, the cheeks of young persons appeared red as in life. I remember a poem in which was described the bringing to the surface of the body of a miner and its recognition by the girl to whom he was betrothed, and who could not believe he was dead, so lifelike was his complexion.

For years the full significance of these appearances was not appreciated.

But when Sir Humphrey Davy retired from his post he left a successor, Michael Faraday, by whom the first great step in the detection of the true causes of coal mine explosions was destined to be taken.

In the year 1844, after the Haswell colliery disaster, Faraday and Lyell were sent to report upon it, there being, in those days, no inspectors of mines.

In their report they say: "In this explosion it is not to be supposed that fire-damp was the only fuel. The coal-dust, swept by the rush of wind and flame from the floor, roof and walls of the works would instantly take fire and burn if there were oxygen enough present to support its combustion, and we found the dust adhering to the faces of the pillars, props and walls. This deposit was in some parts half-an-inch, in others almost an inch thick; it adhered together in a friable, coked state."

This new theory, that coal-dust played a part in mine explosions, did not receive much attention in England, but in France, Mr. Souich and others attributed explosions in part to dust, until in 1875 Mr. Vital made a series of experiments on a small laboratory scale, from which he concluded that dust might of itself alone give rise to disasters.

In 1872 the first Coal-mines Regulation Act was passed in Great Britain, and Mr. Galloway, one of the newly appointed inspectors, at once turned his attention to coal-dust. In 1876 he presented a paper to the Royal Society in which, while admitting that explosions were usually originated by gas, he argued that they could be continued by coal-dust alone, and that if the dust were only fine enough, an explosion begun in a confined space might be propagated through a mine. Several commissions in Great Britain and in Germany then experimented upon the subject.

Still, however, the mining world was not convinced, and even in 1885 a Royal Commission reported adversely to coal-dust as the principal cause of explosions in mines.

In 1886, Messrs. W. N. Atkinson and J. B. Atkinson, inspectors of mines, produced an excellent treatise on the dangers of coal-dust. Their work was written from practical observation of various explosions.

In this work appeared an analysis, made by Professor Bedson, of gases found in the Usworth mine after an explosion, which revealed the presence of no less than $2\frac{1}{2}$ per cent. of carbonic oxide, which, for reasons I will presently give you, was alone almost conclusive proof that coal-dust had played a part in the explosion.

In 1887 a Royal Commission on coal-dust was appointed, which employed Mr. Henry Hall—one of the present inspectors of mines and one of the earliest to adopt the coal-dust theory—to conduct some experiments. Mr. Hall put a cannon of 2-in. bore, charged with $1\frac{1}{2}$ lb. of gunpowder, pointing muzzle upwards, at the bottom of a shaft 50 feet deep. A sack of inflammable dust was then tossed down the shaft, so as to fill the air in it with a cloud of dust, and the cannon fired. Repeated explosions were the result with great tongues of flame, though no gas whatever was present. One of these explosions is shown on the screen.

[Photograph.]

From this date the dangers of coal-dust began to be fully recognised, and now it may be considered fully proved, that though gas may cause small explosions in parts of the mine, general explosions are due to coal-dust ignited either by a small local gas explosion, or else by a blown-out shot.

The arguments for this view are as follows.

1. The travel of the explosions in roads over long distances, which

it is impossible to suppose were all filled with gas in an explosive condition. Thus at Seaham, the explosion ran for four miles.

2. The fact that big explosions do not occur except in dry and dusty mines.

3. The fact that explosions always run along intake airways and main haulage roads, where fine dust abounds, raised by the wagons carrying the coal, which is rendered very dry by the great quantities of dry air sent into the mine.

4. And lastly, the presence of after-damp, of which I will say more by-and-by.

It may now be of interest to show on a small scale the inflammability of certain kinds of fine dust.

Lycopodium, the seed of a fern, is very inflammable, and was once used to produce lightning on the stage.

[Experiment.]

Magnesium powder is well known. Most of the company have probably undergone the process of being photographed after some public dinner by the flash-lamp. I will put only a little here as the light is very blinding.

[Experiment.]

I can even show here by the simple expedient of blowing a little exceedingly fine coal-dust into a flame, that the dust is inflammable.

[Experiment.]

But a much better way is that arranged by Professor Bedson, which he is kind enough to manipulate for us. A small charge of fine dust is puffed from a compressed air reservoir into this globe and is ignited by the coil of platinum.

[Experiment.]

The interest of this apparatus is that by its use Professor Bedson has been enabled to show that different sorts of coal-dust have very different degrees of inflammability.

Dust when ignited can be made to traverse a gallery. A wave of air disturbance precedes the ignition stirring up the dust. Then follows a long lick of flame, which as it proceeds acquires greater and greater velocity until at last after a long run it acquires explosive violence. This little model gallery contains a layer of *lycopodium* powder, which is ignited by a puff of flame sent into it, and you see the flame gently rush along and come out at the end.

[Experiment.]

Captain Desborough has experimented with a small gun in a larger gallery, and from his experiments he and I have designed a gallery 10 feet long strewn with dust which we shall endeavour to ignite for you at the conclusion of the lecture by means of a miniature blown-out gunpowder shot.

The dangers of coal-dust having been thus described, it has next to be considered how they are to be avoided. Three different methods have been proposed.

The first is dusting and sweeping. The difficulty of this is that it is impossible effectively to remove the dust by means of brooms and such like implements, for a very little dust is enough to cause an explosion.

A vacuum cleaner has been used in some places with a certain measure of success.

The most effective method is by means of hose and sprays to wet any portions of the dusty main roads.

The disadvantages of this plan are that in many places water causes the coal to disintegrate and the roof and sides to fall, and is therefore dangerous.

A very damp mine is, moreover, not so healthy as a dry one.

Another plan is to have zones of wet in the roadways. For it is believed that explosions of dust can be stopped if they meet a wet place, and in very many cases it has been observed that explosions stopped at wet places.

In order to see whether these zones are effective, the Associated Coalowners of Great Britain have, with very proper public spirit, built a testing gallery. This gallery is 361 yards long and 7 feet 6 inches in diameter. It is made of old boilers bolted together. It has been designed and is under the care of Mr. Garforth, Past-President of the Coal Owners' Association, and one of our most skilful mining engineers. He has been good enough to have prepared for us a model of the gallery, which is in front of the lecture table.

[Model.]

He has also sent me some slides.

[Photographs.]

The explosions are created by means of blown-out shots of gunpowder, the tubes being fitted with props like the gallery of a mine, and liberally sprinkled with dry inflammable dust.

It is premature to say what results will be obtained. But, so far as we have gone, it appears probable that zones will be effective, and it seems that if stone dust is mixed with the coal the force of the explosion is much modified.

There is one precaution which it is always desirable to take if

possible, and that is to have the shot-firing done between the shifts, when the men are out of the mine. An explosion would thus injure very few people, and if the shot firers took some reserve apparatus with them, they would run a comparatively small risk, even if there were a formidable explosion. For they would be at the face where but little carbonic oxide forms, and if cut off they could be easily rescued, except in those cases where falls of roof prevented egress.

Thus, then, the idea of coal-dust as a principal factor in explosions in mines has been firmly established in Great Britain and in Germany.

Curiously enough, our ingenious and scientific French neighbours, who originally made so many experiments on coal-dust explosions, have of late years been less impressed with the part coal-dust plays in mine disasters.

When Mr. Atkinson, the Mines Inspector, and I went over after the great explosion at Courrières to visit the mine, we found that most of the French miners held the view, which ten years previously our engineers would have held, that gas must have done it. The idea was, of course, incredible: for to attribute an explosion so wide in its effects to gas, one would have to suppose that the mine was full of explosive fire-damp nearly from one end to the other. But how was it to be imagined that gas in such quantity could be present all over the mine without anyone seeing it on his safety lamp?

On the other hand, you had a mine full of the finest and most explosive dust, and as Mr. Atkinson and Mr. Henshaw—who also, at our request, visited the mine—saw at once, the walls were covered with charred coal-dust, exactly similar to that which had so often been seen in our own colliery explosions. The French engineers now entertain no doubt that coal-dust alone is capable both of originating and of propagating an explosion, and are also trying experiments upon a large scale.

Great as the change of opinion has been with regard to coal-dust, our views have had to undergo a transformation almost equally radical with regard to the causes of death after an explosion.

When gas is burned thoroughly, you have, roughly speaking, one volume of fire-damp, mixed with two volumes of oxygen, which yields one volume of carbonic acid gas and two volumes of steam: the seven volumes of nitrogen present remaining unchanged.

Therefore, after such an explosion the mine ought to be full of steam, carbonic acid and nitrogen, all the oxygen having disappeared.

But in practice this never happens. For in gas explosions there is always an excess of oxygen present.

But as we have seen, there are no such things as gas explosions on a large scale—even if there is any gas present to begin the explosion, the main result is always due to dust, and in that case, instead

of black damp, or carbonic acid, we have the products of imperfectly combusted coal, that is to say, after-damp, or carbonic oxide, the gas that kills people who commit suicide so often in Paris by means of a charcoal brazier. For whenever coal is imperfectly burnt, as in an ill-arranged stove, there not only is carbonic acid formed, but likewise carbonic oxide.

In ordinary gas as supplied in most large towns there is a considerable quantity of carbonic oxide present, so that to sleep in a small room with the unlit gas turned on is usually fatal.

The effect of carbonic oxide on the human frame is caused by the fact that it is greedily absorbed by the hæmoglobin or the red colouring matter of the blood corpuscles.

This renders them incapable of absorbing oxygen from the lungs and carrying it to the various parts of the body in order to burn up the carbon derived from the food. In consequence carbonic oxide is an active poison.

The poisonous nature of carbonic oxide has, of course, been known for many years, but it was reserved for Dr. Haldane, of Oxford, to demonstrate to the mining world what a part this poisoning plays in coal-mine disasters. I think that I may, to some extent, claim the credit of having first recognised the ability and devotion of Dr. Haldane in this work, and of having secured his services to help in the investigation of mine explosions.

The period at which the importance of carbonic oxide was most impressed on us was at the Tylerstown explosion, in 1896, at which 57 men were killed, 33 being brought out alive.

Dr. Haldane, in company with Dr. Morris, undertook and carried out the task of examining 45 of the bodies after they were recovered. The object was to discover the cause of death. When death has been caused by carbonic oxide, the blood of the dead man exhibits characteristic symptoms which Dr. Haldane has devised a simple and practical manner of testing. I have his apparatus here, and he has been good enough to say that he will show it in detail after the lecture to anyone who is interested.

Provided with the apparatus and with the assistance of the inspectors the examination proceeded, and resulted in the classical report of 1896 on the causes of death in colliery explosions and underground fires.

The bodies were covered with an adhering layer of charred coal-dust. But there came the surprising discovery that in only five cases was the death due to the violence of the explosion. In all the other cases death had been due to carbonic oxide, showing that the men must have lived and breathed perhaps for hours after the explosion. Of the rescued men, a number had been rendered unconscious, also by the after-damp. The death is quite a painless one; the only symptoms are a slight smarting of the eyes and throat, and then, though the lamps are burning well, and there is plenty of air to

breathe, the person affected feels weak and drops down unconscious, never to recover consciousness again. The mode of resuscitation is that given to drowning men. Fresh air must be artificially forced into the lungs, and oxygen administered. An apparatus has been invented for forcing oxygen into the lungs by pressure, so as to enable the hæmoglobin that has not been poisoned, and the blood serum to absorb oxygen, when the poison is gradually expelled and the patient recovers.

In the case of the Tylerstown bodies there was often an extraordinary appearance of life: the lips were pink, and in only a few cases was there the paleness of death.

Exactly similar results were found with the horses which had been killed.

I remember asking Dr. Haldane whether it would be possible to invent a machine capable of detecting carbon monoxide, so that rescue parties going down into a mine would be warned when there was danger.

Shortly afterwards he pointed out that nature has provided us with a machine of the greatest delicacy, namely a mouse. So rapid is the circulation of these little creatures, that an atmosphere which would take 30 minutes to affect a man, will cause a mouse to become helpless in about 3 minutes. So that now mice are always kept at hand at collieries for use in case of need. The men make pets of them, and in one case a miner refused to let his mouse go down saying that he did not mind going himself, but he was not going to have his mice poisoned!

It is a curious circumstance that after colliery explosions mice are found uninjured. After the Hemstead disaster a mouse was brought up by one of the rescue party. We intended to give him to Dr. Haldane to see if long life in a very fiery mine had made him and his kin immune to the poison; but he slipped out of his cage, and disappeared behind the wainscot, and there was no time to attend to him further. It is said that some mice resist the poison much more than others: it is therefore desirable to take several down. The orders are that when one is affected the party should return, and the mouse usually recovers.

A very small proportion of after-damp is poisonous, for the affinity of hæmoglobin for carbon monoxide is 250 times as great as for oxygen, so that the action is cumulative. About 0·2 per cent. of carbon monoxide in air will cause 67 per cent. of the hæmoglobin to become saturated, and helplessness would ensue and probably death. It requires a volume of a pint of carbon monoxide to be absorbed into a man to cause death, and hence with $\frac{1}{3}$ per cent. of carbon monoxide in the air, if he were at rest, he might live an hour. If he were in motion, as the breathing is much deeper, 25 minutes or half an hour would suffice. Though death from this poison is painless recovery is unpleasant, being accompanied by sickness and headache.

These facts point out clearly that, inasmuch as most dry coal-dust is to be found in the roadways of a mine, there will be found the carbonic oxide. The best way therefore is not to be in a hurry to get out, but to retire into the recesses of the mine away from the large roads, and remain quiet.

It is believed that after the Park Slip explosion, in which 56 men were lost, all might have been saved, if they had remained in their working places.

The case of Roderick Williams deserves notice. He was a fireman at the Tylerstown explosion. Finding his road blocked by after-damp he retired to some old workings, where he remained an hour till he was rescued.

On a previous occasion he saved the lives of a whole company of men by forcibly preventing them getting past him to the shaft. They were saved; but one man, who was too strong for him, got past, and was afterwards found dead.

The importance of getting a supply of fresh air rapidly into a mine after an explosion cannot be over-rated.

But intelligence must be exercised so as not to drive poisonous air into places where men may be in refuge.

It now remains for me to describe an apparatus which is coming into use whereby men may go into poisonous atmospheres with safety, just as divers go down into the sea.

The idea of a contrivance which would enable a man to breathe in a poisonous atmosphere is of old date.

But the first practical form of apparatus is the design of Mr. Fleuss, who is still living, and has more than once risked his life in trying experiments with it.

The principle upon which such machines depend is as follows.

An ordinary man at rest breathing 14 times a minute, and with a lung capacity of about $\frac{1}{2}$ a litre, that is a pint, absorbs $\frac{1}{3}$ of a litre of oxygen each minute. But if he is doing violent work he will absorb about 2 litres a minute, and thus require 240 litres in two hours.

The carbonic acid exhaled by a man in this time, is capable of being absorbed by 2 lb. of caustic soda or potash.

Whence, then, it follows that if receptacles containing 240 litres of oxygen and 3 or 4 lb. of caustic soda can be carried by a man, they will enable him to do hard work in a mine for 2 hours, or if he lay down quietly, to live even for 6 or 7 hours.

The apparatus consists of a cylinder of steel, filled with oxygen under great pressure. This oxygen is emitted through a valve, either fixed as in the Fleuss, Shamrock, or Draeger apparatus, or else an automatic valve as in W. Garforth's apparatus, which regulates the oxygen supply according to the breathing.

The advantage of this plan is that if a man is not doing hard work

his oxygen is automatically economised, and hence he could last out longer if need arose.

The breath expelled from the mouth goes into a receptacle con-

[Photograph.]

taining the caustic soda or potash, which absorbs the carbonic acid. The breath is then refreshed by a fresh addition of oxygen from the cylinder, then it goes to a radiator to cool it, and, thus renovated, it is again sucked into the lungs. The nitrogen of the air goes round and round, being breathed and exhaled over and over again, the oxygen is turned by the body into carbonic acid and in this condition is absorbed by the potash.

Stations are being established all over the country at which men are to be trained to the use of this apparatus. It can hardly be said to be perfect even yet, and a good many men have perished through accidents with its use. There is, however, no doubt that these difficulties will be overcome.

The photographs show the Fleuss apparatus, and the Shamrock apparatus designed by W. Mayer, of Westphalia, and the Draeger apparatus, another form of the Shamrock. The "Weg," or Mr. W. E. Garforth's, apparatus is also shown.

[Four photographs.]

In addition to these, two other apparatus must be mentioned. The Aerolith consists of a sack containing liquid air absorbed in loosely packed asbestos. There is no need for any potash, for the air expelled from the lungs is simply breathed away. The full charge of the bag is 5 litres of liquid air. As it escapes it becomes expanded and cooled. It is pleasant to use and the apparatus is very light, as there are no heavy cylinders to carry. This is one of the latest applications in a practical way of the work done by Sir James Dewar. The next apparatus I have great hopes of. It consists of a bag containing sodium-potassium-peroxide. This extraordinary chemical seems as though expressly designed for breathing apparatus, for when damped it exhales oxygen, leaving caustic soda and potash behind, which in their turn absorb carbonic acid.

It would be perfect, were it not that the chemical is very inflammable, and two men, one in Germany and one in London, have been injured by its use.

Dr. Hill's interesting experiments in giving oxygen to exhausted athletes have recently attracted considerable attention.

I have here an apparatus, designed by Dr. Leonard Hill, for supplying oxygen to invalids by means of sodium-potassium-peroxide. In glass bottles it is quite safe.

[Siebe Gorman oxygen apparatus and powder.]

I will ask a man wearing the Fleuss apparatus to step in so that you may see it.

[Fleuss's man.]

Next the Aerolith.

[Simoni's man.]

I will next show you the Weg apparatus. This is interesting because the wearer, Hopwood, has been decorated by his Majesty with the King Edward Medal for saving life in mines, and this is the apparatus with which he earned it.

I will now shut him up in a cabinet, such as is used in testing, and fill it with poisonous smoke, and you will see that he is quite at ease. In fact, his breathing is cut off from the atmosphere around him, so that it is only his eyes that would be affected by pungent vapour.

My task is now ended all but one experiment, and I hope that the audience feel that to the saving of life in mines earnest and ingenious men are giving the best of their ability, and that some progress is being made.

I ought not to conclude without one observation. When I spoke of safety lamps, I had to mention the name of Sir Humphrey Davy, the illustrious Professor of Chemistry at this Institution. When I had to deal with dust, I had to point out the work of his successor Faraday, and when dealing with the last development of rescue apparatus, I had to refer to the work of our present Professor of Chemistry, Sir James Dewar, who, I trust, may long be spared to continue his useful labours.

If any proof were wanted of the utility of Institutions like this, and of the degree to which they merit public support, no better could be given than the fact that in a lecture like I have given the work of the Royal Institution had to be so often mentioned.

I will now conclude by exploding a gunpowder cartridge in a small model mine, the floor of which is strewn with coal-dust.

This experiment does not always succeed, for (fortunately) coal-dust does not always explode, but I hope you will see the flame issue from the mouth, driving before it a black cloud like that seen at real colliery explosions, and which smoke, in order to spare the dress of the audience, I shall endeavour to catch and shut up in this chamber.

[H. C.]

WEEKLY EVENING MEETING,

Friday, February 26, 1909.

SIR WILLIAM CROOKES, D.Sc. F.R.S., Honorary Secretary and
Vice-President, in the Chair.

PROFESSOR H. L. CALLENDAR, M.A. LL.D. F.R.S.

Osmotic Phenomena and their Modern Physical Interpretation.

OSMOTIC pressure is a phenomenon of such importance in the theory of solutions, and in the interpretation of all vital processes, and so much valuable work has recently been directed to its elucidation, that, although it is a somewhat thorny and difficult subject, no apology is needed for any serious attempt, however inadequate, at its explanation.

One of the earliest recorded experiments on osmotic pressure is that of the Abbé Nollet, who found that a bladder containing alcohol, when immersed in water, absorbed water so greedily as in many cases to burst the bladder. The experiment illustrates in an imperfect manner the fundamental property of all animal and vegetable membranes of allowing some substances to pass through them by osmosis more easily than others. In many cases such membranes, while freely permeable to water, are practically impermeable to certain substances in solution, and play the part of sieves in directing and controlling diffusion. It will readily be understood that results of the greatest importance to biology have been obtained by studying this property of *semipermeability*, as it is called, but the application of natural membranes to the physical study of the subject is necessarily limited on account of the difficulty of obtaining sufficiently large and perfect membranes capable of withstanding any considerable pressure.

Artificial membranes of sufficient fineness to be impermeable to such substances as sugar in solution, were first prepared by Traube by means of precipitated pellicles of substances like copper-ferrocyanide. The first quantitative measurements of osmotic pressures of considerable magnitude were made by Pfeffer with membranes of this kind deposited in the pores of earthenware pots fitted with suitable manometers for indicating the pressure developed. Pfeffer found that when a semipermeable vessel, filled with sugar solution, was immersed in water, the membrane being freely permeable to water, but not to the dissolved sugar, the solution absorbed water through the membrane by osmosis until the internal pressure reached a certain magnitude sufficient to balance the tendency to absorption. The osmotic

pressure developed in the state of equilibrium was found to be proportional to the strength of the solution, and to increase with rise of temperature at the same rate as the pressure of a gas at constant volume. A few years later van 't Hoff, reviewing these experiments in the light of thermodynamics, showed that the osmotic pressure of a dilute solution should be the same as the pressure exerted by a number of molecules of gas equal to those of the dissolved substance in a space equal to the volume of the solution, that it should be the same for all solutions of equal molecular strength, and that osmotic pressure followed the well-known laws of gas-pressure in all respects. The most important generalisation was hailed as the first step to a complete kinetic theory of solution, and the osmotic pressure itself has generally been regarded as due to the bombardment of the sides of the semipermeable membrane by the particles of solute, as though they were able to move freely through the solution with velocities comparable to those of the molecules of a gas. Such a view would not now be seriously maintained, but the fascinating simplicity of the gas-pressure analogy has frequently led to the attempt to express everything in terms of the osmotic pressure, regarded simply, but inaccurately, as obeying the gaseous laws, and has done much to divert attention from other aspects of the phenomena, which, in reality, are more important and have the advantage of being more easily studied. It was very soon discovered that the gaseous laws for osmotic pressure must be restricted to very dilute solutions, and that the form of the laws was merely a consequence of the state of extreme dilution, and did not necessarily involve any physical identity between osmotic pressure and gas-pressure. Many different lines of argument might be cited to illustrate this point, but it will be sufficient to take some of the more recent experimental measurements of osmotic pressure by the direct method of the semipermeable membrane.

Morse and Frazer in 1905 succeeded in preparing ferrocyanide membranes impermeable to sugar, and capable of withstanding pressures of more than 20 atmospheres. They operated by Pfeffer's original method, allowing water to diffuse into the solution in a porous pot until the maximum pressure was developed. There are many serious experimental and manipulative difficulties which the authors carefully considered and discussed in applying this method, but they succeeded in obtaining very consistent results. As a first deduction from their investigations they considered that they had established the relation that the osmotic pressure of cane-sugar was the same as that exerted by the same number of molecules of gas at the same temperature in the volume occupied by the solvent, and not in the volume occupied by the solution. In other words, the osmotic pressure of a strong solution was greater than that given by van 't Hoff's formula for a dilute solution in proportion as the volume of the whole solution exceeded the volume of the solvent contained in it. It was a very natural extension of the gas-pressure analogy to

deduct the volume occupied by the sugar molecules themselves in order to arrive at the space in which they were free to move. Unfortunately the later and more accurate series of measurements by the same experimentalists at 0°C. and 5°C. , gave nearly the same osmotic pressures as at 24°C. , and would appear to show, either that there is little or no increase of osmotic pressure with temperature, and that the pressures at 0°C. are much greater than those given by their extension of the gas-pressure analogy, or that one or other of the series of experiments are in error.

About the same time Lord Berkeley and E. J. Hartley undertook a series of measurements of the osmotic pressures of solutions of various kinds of sugar at 0°C. by a greatly improved experimental method, which permitted the range of pressure to be extended to upwards of 100 atmospheres. Instead of allowing the solvent to diffuse into the solution until the equilibrium pressure was reached, they applied pressure to the solution until balance was attained. The method of Lord Berkeley and Hartley possesses several obvious advantages, and it is impossible to study the original memoir without being convinced that they have really measured the actual equilibrium pressures with an order of certainty not previously attained or even approached. The pressures found were in all cases greatly in excess of those calculated from the gas-pressure of the sugar molecules in the volume occupied by the solution (according to van 't Hoff's formula for dilute solutions), or even in the restricted volume occupied by the solvent (according to Morse and Frazer's assumption).

Lord Berkeley endeavoured to represent these deviations on the gas-pressure analogy by employing a formula of the van der Waals type, with three disposable constants. Out of some fifty formulæ tested, the two most successful were those given in Table I. The constants A , a , and b were calculated to fit the three highest observations for each solution. Values calculated by the formulæ for the lower points were then compared with the observations at these points, with the results given in Table I. for cane-sugar. It is at once evident that, even with three constants, the gas-pressure analogy does not represent the results satisfactorily within the limits of error of experiment. Moreover, with three constants the equation cannot be interpreted, so that the gas-pressure analogy becomes useless as a working hypothesis, or as a guide to further research. On the vapour-pressure theory, to be next explained, the results are much better represented, as shown in column C, with but a single constant, and that a positive integer with a simple physical meaning.

VAPOUR-PRESSURE THEORY.

On the vapour-pressure theory, osmotic equilibrium depends on equality of vapour-pressure and not on an imaginary pressure which the particles of the dissolved substance would exert if they were in

TABLE I.—OSMOTIC PRESSURES OF CANE-SUGAR SOLUTIONS.

Osmotic Pressures Calculated by various formulæ.

Van 't Hoff.	Morse and Frazer.	Lord B. (1).	Lord B. (2).	C.	Do. Observed. Lord B.
35·6	53·2	68·4	67·7	67·6	67·5
27·6	37·4	45·0	43·4	43·7	44·0
19·7	24·4	27·7	25·4	26·8	26·8
11·2	13·8	14·6	12·2	14·1	14·0

Lord Berkeley's equations—

$$(A/v - P + a/v^2)(v - b) = RT \quad . \quad . \quad . \quad (1)$$

$$(A/v + P - a/v^2)(v - b) = RT \quad . \quad . \quad . \quad (2)$$

the state of gas at the same volume and temperature. The vapour-pressure of any substance is a definite physical property of the substance which is always the same under the same conditions of pressure and temperature and state, and is easily measured in most cases for liquids and solutions. Equality of vapour-pressure is one of the most general, as well as the simplest, of all conditions of physical equilibrium. Ice and water can only exist together without change under atmospheric pressure at the freezing-point $0^{\circ}\text{C}.$, at which their vapour-pressures are the same. Below the freezing-point the vapour-pressure of water is greater than that of ice. Either is capable of stable existence separately within certain limits, but if the two are put in communication, the vapour, being mobile, passes over from the water at higher pressure to the ice at lower pressure until equality of vapour-pressure is restored by change of temperature, or until the whole of the water is converted into ice.

In the case of ice and water, equality of vapour-pressure can also be restored by a suitable increase of pressure. This is the well-known phenomenon of the lowering of the freezing-point by pressure. By considering the equilibrium of water and vapour in a capillary tube, Lord Kelvin showed that the vapour-pressure of water, or any other liquid, was increased by pressure according to a very simple law, the ratio of the increase of vapour-pressure, dp , to the increase of pressure, dP , on the liquid being simply equal to the ratio of the densities of the vapour and liquid, or inversely as the specific volumes, v and V . This relation, which may be written $V dP = v dp$, is merely a special case of Carnot's principle, and was deduced by assuming the impossibility of perpetual motion. Assuming a similar relation to apply to ice, Poynting showed that when a mixture of ice and water was subjected to pressure, the vapour-pressure of the ice must be increased more than that of the water (since the specific

volume of ice is greater than that of water). Consequently, some of the ice must pass over into water, and the temperature must fall until the vapour-pressures are again equal. The lowering of the freezing-point by pressure, as observed by Lord Kelvin, and calculated by James Thomson, agrees precisely with that deduced as above from the condition of equality of vapour-pressure.

[3] Similar considerations apply to the equilibrium between a solution and the pure solvent, or between solutions of different strengths. To take a simple case, the vapour-pressure p'' of a sugar solution is always less than the vapour-pressure p' of water at the same temperature, and the ratio p''/p' of the vapour-pressures depends simply on the concentration of the solution, diminishing regularly with increase of concentration and being independent of the temperature. If separate vessels containing solution and water are placed in communication at the same temperature by a tube through which the vapour has free passage, vapour will immediately pass over from the water to the solution in consequence of the pressure difference, and will condense in the solution. The immediate effect is to produce equality of vapour-pressure by change of temperature. This takes only a few seconds. The vapour-pressure then remains practically uniform throughout. As diffusion proceeds and the temperature is slowly equalised, the water will gradually distil over into the solution, but the process of diffusion is so infinitely slow compared with the equalising of vapour-pressure that the final attainment of equilibrium would take years unless the solution were continually stirred.

The reason why equality of vapour-pressure is so important as a condition of physical equilibrium is that the vapour is so mobile and so energetic as a carrier of energy in the form of latent heat. The first effect is generally a change of temperature, but if the temperature is kept constant there must then be a change of concentration. Thus if two parts of the same solution are maintained at different constant temperatures, the concentrations will change so as to restore equality of vapour-pressure, if possible. Thus in a tube of solution, the two ends of which are maintained at different temperatures, the dissolved substance will appear to move towards the hotter end. What really happens is that the vapour, which is the mobile constituent, moves towards the colder end. If the tube is horizontal, with a free space above the liquid for the vapour, this transference will be effected with extreme rapidity. In fact, it will be practically impossible to establish an appreciable difference of temperature until the transfer is effected. If the vapour has to diffuse through the solution in a vertical column heated at the top, the process is greatly retarded, but the final effect is the same, and can be readily calculated from the relation between the vapour-pressure and the concentration.

In explaining the production of osmotic pressure as a necessary

consequence of the laws of vapour-pressure, there is one difficulty which, though seldom expressed, has undoubtedly served very greatly to retard progress. How can an insignificant difference of vapour-pressure, which may not amount to so much as one-thousandth part of an atmosphere in the case of a strong sugar solution at $0^{\circ}\text{C}.$, be regarded as the cause of an osmotic pressure exceeding 100 atmospheres, or 100,000 times as great as itself? The answer is that the equilibrium does not depend at all on the absolute magnitude of the vapour-pressure, but only on the work done for a given ratio of expansion, which is the same in the limit for a gramme-molecule of any vapour at the same temperature, however small the vapour-pressure. Indirectly the smallness of the vapour-pressure may have a great effect in retarding the attainment of equilibrium, especially if obstructive influences, such as other vapours or liquids, are present. Thus mercury at ordinary temperatures in the open air is regarded as practically non-volatile. Its vapour-pressure is less than a millionth of an atmosphere, and cannot be directly measured, though it may easily be calculated. When, however, we take mercury in a perfect vacuum, such as that of a Dewar vessel, the presence of the vapour is readily manifested by its rapid condensation on the application of liquid air in the form of a fine metallic mirror of frozen mercury. The least trace of air or other gas in the vacuum will retard the condensation excessively.

Under the conditions of an osmotic-pressure experiment we have solvent and solution in practical contact, separated only by a thin porous membrane. It will facilitate our conception of the conditions of equilibrium if we imagine the membrane to be a continuous partition pierced by a large number of very fine holes, of the order of a millionth of an inch in diameter. If the holes are not wetted by the solution or the water, the liquid cannot get through unless the pressure on it exceeds 100 atmospheres, but the vapour has free passage. If the solvent and solution are under the same hydrostatic pressure, the vapour-pressure of the solvent will be the greater, and the vapour will pass over into the solution. Since the surfaces are practically in contact, no appreciable difference of temperature can be maintained. If the solution is confined in a rigid envelope, so that its volume cannot increase, the capillary surfaces of the solution will rapidly bulge out as the vapour condenses on them, and the pressure on the solution will increase until condensation finally ceases, when the vapour-pressure of the solution is raised to equality with that of the pure solvent. The osmotic pressure is simply the mechanical pressure-difference which must be applied to the solution in order to increase its vapour-pressure to equality with that of the pure solvent. If any pressure in excess of this value is applied to the solution, the vapour will pass in the opposite direction, and solvent will be forced out of solution. The osmotic work required to force a gramme-molecule of the solvent out of the solu-

tion is the product of the osmotic pressure P by the change of volume U of the solution per gramme-molecule of solvent abstracted. In the state of equilibrium of vapour-pressure, this osmotic work $P U$ must be equal to the work which the vapour could do by expanding from the vapour-pressure p' of the pure solvent to the vapour-pressure p'' of the solution. Neglecting minor corrections, we thus obtain the approximate relation—

$$P U = R \theta \log (p'/p'').*$$

From this point of view the osmotic pressure of a solution is not a specific property of the solution in the same sense as the vapour-pressure, or the density, or the concentration, but is merely the mechanical pressure required under certain special conditions to produce equilibrium of vapour-pressure when neither the temperature nor the concentration are allowed to vary. One might with almost equal propriety speak of the "osmotic temperature" of a solution, meaning by that phrase the difference of temperature required to make the vapour-pressure of the solution equal to that of the pure solvent. The observation of the elevation of the boiling-point of a solution above that of the pure solvent is a familiar instance of a special case of such a temperature difference. It is just as much a specific property of the solution as the osmotic pressure, and would only require a perfectly non-conducting membrane for its production. No one would regard the rise of boiling-point as being the fundamental property of a solution in terms of which its other properties should be expressed. By similar reasoning osmotic pressure should not be regarded as existing *per se* in the solution, and as being the cause of the relative lowering of vapour-pressure and other phenomena. This point of view does not detract in any way from the reality and physical importance of the effects of osmotic pressure when it comes into play, but it puts the phenomena in their true light as consequences of the law of vapour-pressure.

Regarded as a verification of the laws of vapour-pressure, direct measurements of the osmotic pressure are of the highest value, but there are comparatively few cases known at present in which such direct measurements are possible. In other cases, the osmotic pressure, if it exists, can always be calculated from a knowledge of the vapour-pressure. For the elucidation of osmotic phenomena and many other problems in the theory of solutions, we are compelled to make a systematic study of the relations of vapour-pressure. Much has been done in this direction in the past, but, owing to the difficulty of the measurements, much remains yet to do. I may, therefore, be pardoned if I allude briefly to some of the methods

* Obtained by integrating $U dP = v dp$. Planck, *Thermodynamik*, also *Zeit. Phys. Chem.*, xli. 212, 1902, and xlii. 584, 1903.

which I have employed for this purpose, and some of the conclusions at which I have so far arrived.

It is often a difficult matter, when the difference of vapour-pressure between a solution and the solvent is small, to measure the pressure difference directly to a sufficient degree of accuracy. A method very commonly employed, which has been brought to a high degree of accuracy by Lord Berkeley and his assistants, depends on the observation of the losses of weight of two vessels, containing solution and solvent respectively, when the same volume of air is aspirated slowly through them in succession. To secure accurate results, the air must pass very slowly. One complete observation takes about a week to perform successfully, and involves many difficult manipulations. I have endeavoured to avoid this difficulty by measuring the temperature difference in place of the pressure difference, since the temperature difference remains nearly constant, while the pressure difference tends to diminish in geometrical progression with fall of temperature. The method adopted for this purpose is that indicated in the diagram of the Vapour-Temperature Balance. The temperatures of solution and solvent, contained in separate vessels communicating through a tap, are adjusted, until, on opening communication between them, there is no flow of vapour from one to the other, as indicated by a change in the reading of a pair of thermo-junctions immersed in the solvent respectively. The corresponding difference of temperature is observed, and since the vapour-pressures of the solvent are known, it is easy to calculate the required ratio or difference of the vapour-pressures of solvent and solution at the same temperature. When the vapour-pressures are very small, it may be difficult to observe the change of temperature on opening the tap, unless the apparatus is very carefully exhausted. A more delicate method in this case is to observe the direction and magnitude of the current of vapour from solution to solvent, or *vice versa*, by means of the "Vapour-Current Indicator," illustrated in the companion diagram. This consists of a delicately suspended vane, the deflections of which are read by a mirror, and will readily indicate a difference of pressure less than the thousandth part of a millionth of an atmosphere.

The vapour-current indicator is so constructed that its deflections are very accurately proportional to the pressure difference, much more so in fact than any form of electric galvanometer. It can also be employed for direct measurements of small differences of vapour-pressure. The chief difficulty in this case is to ensure the absence of air or other disturbing factors. A method of avoiding this difficulty is to work at atmospheric pressure, and to measure the pressure difference between two vertical columns of air saturated with the vapours of the solvent and solution respectively.* The temperature

* I first showed this experiment ten years ago, in illustration of the delicacy of the apparatus, at a Friday Evening Lecture at the Royal Institution.

difference may be adjusted to balance, and is preferably measured by means of a pair of differential platinum thermometers, which permits a higher order of accuracy to be attained than the thermo-electric method.

VAPOUR-PRESSURE IN RELATION TO MOLECULAR CONSTITUTION.

The well-known law of Raoult, according to which the relative lowering of vapour-pressure of a solution is equal to the ratio of the number of molecules n of the solute to the number of molecules of solvent N in the solution, has thrown a great deal of light on the molecular state of the dissolved substance in dilute solutions, but fails notably in many cases when applied to strong solutions. In the case of homogeneous mixtures of two indifferent volatile substances, such as benzol (C_6H_6) and ethylene chloride ($C_2H_4Cl_2$), which mix in all proportions without mutual action, a slightly different but equally simple law is known to hold very accurately throughout the whole range of concentration from 0 to 100 per cent. The vapour-pressure of each ingredient is simply proportional to its molecular concentration. In other words, the ratio of the partial vapour-pressure p' of either constituent at any concentration to its vapour-pressure p'_0 in the pure state at the same temperature is equal to the ratio of the number of its molecules n' in the solution to the whole number of molecules $n' + n''$ of both substances in the solution. Such is evidently the form of the simple mixture law. For substances which form compounds in the solution, or whose molecules are associated or dissociated, this simple law is widely departed from. In a recent paper, "*On Vapour-Pressure and Osmotic Pressure of Strong Solutions*" (Proc. R.S.A., vol. lxxx. p. 466, 1908), I have endeavoured to extend this simple relation to more complicated cases by making the obvious assumption that, if compound molecules are formed, they should be counted as single molecules of a separate substance in considering their effect on the vapour-pressure. With this proviso the vapour-pressures of strong solutions are well represented by a natural extension of the simple mixture law, and it becomes possible to investigate the nature of the compounds formed in any case. To take a simple instance, suppose that each of the n molecules of the dissolved substance combines with a molecules of the solvent, the total number of molecules of the solvent being N . The ratio of the vapour pressure p'' of the solvent in the solution to the vapour-pressure p' of the pure solvent at the same temperature will then be the same as the ratio of the number $N - an$ of molecules of free solvent in the solution to the whole number of molecules $N - an + n$ in the solution, each compound molecule being counted as a single molecule.

With the simple formula—

$$p'/p'' = (N - an + n) / (N - an)$$

the values of the vapour-pressure are very easily calculated from the molecular concentration n for simple integral values of the hydration factor a . The osmotic pressures are also readily deduced from the ratio of the vapour-pressures (p'/p'') by the formula

$$PU = RT \log (p'/p'').$$

The value $a = 5$ fits the osmotic pressures for cane-sugar very well, as shown in the column headed C in Table I. The value $a = 2$ fits Lord Berkeley's observations on dextrose equally well up to pressures of 130 atmospheres. The same value $a = 5$ for cane-sugar also fits the observations on the depression of the freezing-point and the rise of the boiling-point, as it necessarily must, since these phenomena also depend on the vapour-pressure. The freezing-point method is the easiest for getting the ratio of the vapour-pressures to compare with the formula. At the freezing-point of an aqueous solution, the vapour-pressure of the solution must be the same as that of ice, provided that ice separates on freezing in the pure state. The ratio of the vapour-pressure of ice to that of water at any temperature below 0°C . is easily calculated. All the best recorded results, except those of a few associating substances, give simple positive integral values of a . Even in the case of associating substances, like Formic Acid and Acetone, the curves are of the same type, but the value of a is negative. Dissociating substances, like strong electrolytes, present greater difficulties, on account of the ionisation factor. But allowing for the uncertainty of the ionisation data, they seem to follow satisfactorily the same law of vapour-pressure.

It appears from the form of the proposed law that the hydration factor a makes very little difference to the vapour-pressure in weak solutions, which follow Raoult's law as a limiting case, but it makes a very great difference in strong solutions, when nearly all the free water is used up, and the denominator $N - an$ is small. Thus the depression of the freezing-point of a strong solution of calcium chloride is more than five times as great as that calculated from the number of ions present in the solution. Each ion appears to appropriate no less than 9 molecules of water. The factor $a = 9$ gives a very good approximation to the freezing-point curve, as far as the uncertainty of the data permit. When $N = an$, the vapour-pressure would be reduced to zero, according to the formula, but the formula ceases to apply when the vapour-pressure of the compound molecules themselves becomes equal to that of the solution. At or before this point the molecules will dissociate with the formation of lower hydrates. Many analogous phenomena are already known, and a

more complete study of the vapour-pressures of strong solutions may be expected to throw additional light on the subject.

The essential point of the theory here sketched is that the equilibrium existing in a solution is one between definite chemical compounds and the solvent, giving rise to a simple vapour-pressure relation by means of which the phenomena may be studied and elucidated. There is a great deal of work to be done before such a theory can be regarded as established, but in the mean time it may serve very well as a working hypothesis for correlating experimental results, and suggesting new lines of investigation. Regarded in this light, the vapour-pressure theory may serve a useful purpose, and judging by the experimental data at present available, I think I may fairly claim to have made out a good *prima-facie* case for the theory.

[H. L. C.]

NOTE.—The vapour-current-indicator is a development of the old smoke-jack. A light spiral vane with a mirror attached is suspended in a tube which nearly fits it by means of a quartz fibre. Joule (Proc. Phil. Soc., Manchester, vii. 35) employed a wire spiral suspended by a silk fibre for indicating air currents, but does not seem to have adapted it for purposes of exact measurement. The instrument shown in the lecture gave a deflection of 30° (500 mm. at 1 metre) for a velocity of air current $\cdot 01$ cm./sec. The sensitiveness might easily have been increased, but the above amply suffices for most purposes.

GENERAL MONTHLY MEETING,

Monday, March 1, 1909.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Alfred Edward Garrett, Esq., B.Sc. F.R.G.S. F.R.A.S.

Daniel Jones, Esq.

Ernest Lunge, Esq.

Thomas Horrocks Openshaw, Esq., C.M.G. F.R.C.S.

Miss Power,

The Rt. Hon. Sir George Wyatt Truscott,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mrs. Wigan for her Donation of £10 10s. to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Honorary Secretary announced the decease of Professor Julius Thomsen, on February 13, 1909, and the following Resolution of Condolence, passed by the Managers at their Meeting held this day, was read and unanimously adopted :—

Resolved, That the Managers of the Royal Institution of Great Britain desire to record their deep sense of the loss the scientific world has sustained in the decease, in the 83rd year of his age, of Professor Julius Thomsen, Ph.D. M.D. Hon.F.R.S. Hon.F.C.S., the eminent Danish Chemist, President of the Royal Danish Society of Science, formerly Professor at the University of Copenhagen, identified for his epoch-making work on Thermochemistry, published in 1882-6, in four volumes, and one of the most distinguished Honorary Members of the Royal Institution.

On the occasion of the Commemoration of the Faraday Centenary in 1891, Professor Julius Thomsen was elected an Honorary Member of the Royal Institution.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with the family in their bereavement.

The following After-Easter Lecture Arrangements were announced :—

PROFESSOR FREDERICK W. MOTT, M.D. F.R.S. F.R.C.P., Fullerian Professor of Physiology, R.I. Two Lectures on THE BRAIN IN RELATION TO RIGHT-HANDEDNESS AND SPEECH. On *Tuesdays*, April 20, 27.

PROFESSOR SVANTE ARRHENIUS, D.Sc. Hon. F.C.S. *Hon. Mem. R.I.* Two Lectures on COSMOGONICAL QUESTIONS. (The Tyndall Lectures.) On *Tuesdays*, May 4, 11.

PROFESSOR JOHN GARSTANG, B.Litt. M.A. F.S.A. Two Lectures on THE HITTITES: (1) MONUMENTS OF EGYPT AND ASIA MINOR; (2) RECENT DISCOVERIES IN ASIA MINOR AND NORTHERN SYRIA. On *Tuesdays*, May 18, 25.

F. GOWLAND HOPKINS, Esq., M.A. M.B. D.Sc. F.R.S. Two Lectures on BIOLOGICAL CHEMISTRY. On *Tuesdays*, June 1, 8.

JAMES PATERSON, ESQ., A.R.S.A. R.W.S. R.S.W. Three Lectures on ASPECTS OF APPLIED AESTHETICS: (1) HOW A TRUE ART INSTINCT MAY BE BEST DEVELOPED; (2) LANDSCAPE, OLD AND NEW; (3) ART AND ETHICS. On *Thursdays*, April 22, 29, May 6.

JOHN G. MILLAIS, ESQ., F.Z.S. Three Lectures on NEWFOUNDLAND. On *Thursdays*, May 13, 20, 27.

PROFESSOR W. E. DALBY, M.A. B.Sc. M.INST.C.E. Two Lectures on A MODERN RAILWAY PROBLEM: STEAM *v.* ELECTRICITY. On *Thursdays*, June 3, 10.

ROBERT T. GÜNTHER, ESQ., M.A. F.R.G.S. F.L.S. Two Lectures on THE EARTH MOVEMENTS OF THE ITALIAN COAST, AND THEIR EFFECTS. On *Saturdays*, April 24, May 1.

PROFESSOR WALTER RALEIGH, M.A. Two Lectures on 1. EDMUND BURKE; 2. BURKE'S PROSE. On *Saturdays*, May 8, 15.

W. H. R. RIVERS, ESQ., M.D. F.R.S. Two Lectures on THE SECRET SOCIETIES OF THE BANKS' ISLANDS. On *Saturdays*, May 22, 29.

FREDERICK FROST BLACKMAN, ESQ., M.A. D.Sc. F.R.S. Two Lectures on THE VITALITY OF SEEDS AND PLANTS: (1) A VINDICATION OF THE VITALITY OF PLANTS. (2) THE LIFE AND DEATH OF SEEDS. On *Saturdays*, June 5, 12.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XVIII. 1^o Semestre, Fasc. 1-2. 8vo. 1909.

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British Architects, Royal Institute of—Journal, Third Series, Vol. XVI. Nos. 7-8. 4to. 1909.

British Astronomical Association—Journal, Vol. XIX. No. 4. 8vo. 1909.

Memoirs, Vol. XVI. Part 2. 8vo. 1909.

Cambridge Philosophical Society—Proceedings, Vol. XV. Part 1. 8vo. 1909.

Carnegie Institution, Washington—Contributions from the Mt. Wilson Solar Observatory, No. 30. 8vo. 1908.

Chemical Industry, Society of—Journal, Vol. XXVIII. No. 3. 8vo. 1909.

Chemical Society—Proceedings, Vol. XXV. Nos. 351-2. 8vo. 1909.

Journal for Feb. 1909. 8vo.

Chemistry, Institute of—Register of Fellows, 1908. 8vo.

Cracovie, Academy of Sciences—Bulletin, 1908, Classe des Sciences Mathématiques, Nos. 9-10; Classe de Philologie, Nos. 6-9. 8vo.

Davies, John S., Esq. (the Author)—The Mechanism of the Solar System. 8vo. 1908.

De Kantzow, Admiral H. P., R.N. M.R.I.—Christopher Columbus. By Filson Young. 2 vols. 8vo. 1906.

Editors—Agricultural Economist for Feb. 1909. 4to.

Airship for Jan.-Feb. 1909. 4to.

American Journal of Science for Feb. 1909. 8vo.

Analyst for Feb. 1909. 8vo.

Astrophysical Journal for Jan. 1909. 8vo.

Athenæum for Feb. 1909. 4to.

Author for Feb.-March, 1909. 8vo.

British Homœopathic Review for Feb. 1909. 8vo.

Chemical News for Feb. 1909. 4to.

Editors—continued.

- Chemist and Druggist for Feb. 1909. 8vo.
 Concrete for Feb. 1909. 8vo.
 Dyer and Calico Printer for Feb. 1909. 4to.
 Electrical Contractor for Feb. 1909. 8vo.
 Electrical Engineer for Feb. 1909. 4to.
 Electrical Engineering for Feb. 1909. 4to.
 Electrical Review for Feb. 1909. 4to.
 Electrical Times for Feb. 1909. 4to.
 Electricity for Feb. 1909. 8vo.
 Engineer for Feb. 1909. fol.
 Engineer-in-Charge for Feb. 1909. 8vo.
 Engineering for Feb. 1909. fol.
 Horological Journal for Jan.-Feb. 1909. 8vo.
 Illuminating Engineer for Feb. 1909. 8vo.
 Journal of the British Dental Association for Feb. 1909. 8vo.
 Journal of Physical Chemistry for Jan. 1909. 8vo.
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WEEKLY EVENING MEETING,

Friday, March 5, 1909.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer
and Vice-President, in the Chair.

THE RIGHT HON. VISCOUNT ESHER, G.C.B.
G.C.V.O. D.L. M.A.

*The Letters of Queen Victoria.**

(By the gracious permission of His Majesty The King, Patron of the
Royal Institution.)

It has been said that the characteristic of English Monarchy is that it retains the feelings by which the heroic kings governed their rude age, and has added the feelings by which the Constitutions of later Greece ruled in more refined ages. Possibly this idea might have been expressed in more elegant language, but the idea itself is sound, and true. Our system of government—Constitutional Monarchy—is a happy blending of the personal influence of an hereditary rule with the organized expression of popular opinion. The will of the majority is the decisive factor, but it is subject to the indirect guidance of a monarchical sentiment acting and reacting through the person of the Sovereign.

Few things are more difficult to explain than the precise value and force of the influence of the Crown in public affairs. Perhaps there is no advantage in trying to elucidate the mystery, for it is to an atmosphere of mystery, to the unrent veil between the Crown and the People, that the influence of the Sovereign upon national policy is largely due. I am not generalizing, but am speaking of England—of our country—and of the times in which we are living. It is a fact that thoughtful men did not always look with favour upon the mystery of which I have spoken. Mr. Fox declaimed against the hidden influence of George III., as the undetected agency of “an infernal spirit.” Later on, however, a great change occurred, and forty years ago wise and liberal-minded politicians were in the habit of saying with reverence, “*We shall never know, but when history is written, our children may know what we owe to the Queen and Prince Albert.*” This attitude of faith towards the beneficent influence of the Sovereign power was a new thing, unregarded by the statesmen of the House of Hanover. History, as the secrets of the past three decades slowly leak out in memoirs and correspondence, has revealed

* Published in “The Times” of Saturday, March 6.

this Royal influence working backwards and forwards, like a shuttle, through the slowly forming web of our political fabric, undetected at the time, but largely responsible for the harmonious colouring of the whole. The published correspondence of Queen Victoria has carried onward the curious story a further stage. No one can read the volumes printed last year, by leave of the King, and fail to perceive that men forty years ago were right, and that the nation owes a heavy debt of gratitude to the Queen and Prince Albert. I approach the consideration of these volumes with much diffidence.

THE ATMOSPHERE OF HISTORICAL WORKS.

It has always appeared to me that the true significance of any historical work is to be found in what—for want of a better designation—must be called atmosphere. Few have been able to create it. The most famous prose writer of ancient Greece, with light touches and in half-a-dozen lines, carries the reader straight into the palaestra at Athens, and you seem to feel the hot summer sun beating down upon the playgrounds, and can see the teachers seated on the low benches, and the white-robed scholars grouped round them. The greatest of English poets has made the Forest of Arden as real to us as the Forest of Windsor—and to many men, as to the first Duke of Marlborough, the only history that is really alive is Shakespeare's. Dumas the Elder and Sir Walter Scott possessed this magical gift, while among living Englishmen, if a master-writer of history has been lost in George Meredith, perhaps lovers of literature have been the gainers because he chose another field. These supreme artists, as I have said, could create atmosphere, and the mere mention of their names shows the hopelessness of the task before me.

I shall, however, make no serious attempt, for, by the gracious leave of His Majesty the King, I am enabled to quote certain passages from the unpublished journals of the Queen which will create for us that atmosphere which, as I have said, is so essential to the true understanding of character and of events. Before proceeding further I should like to state concisely the questions which readers of the correspondence of Queen Victoria should set before themselves, and seek to answer:—What do we owe to Queen Victoria? What was the secret of her influence? What will be her place in history? I cannot pretend to answer them, but we can perhaps proceed some little way together along the path which leads to their ultimate solution.

THE BEGINNING OF THE JOURNALS.

On the day, the 24th of May, 1832, that the little Princess Victoria was thirteen years old, her life, as described by herself, began. As described by herself, because on that day her mother

gave the child a small octavo volume, half-bound in red morocco, with the words "Princess Victoria" stamped on the side. The first entry is as follows:—

"This book Mama gave me, that I might write the journal of my journey to Wales in it. VICTORIA."

From this time forward, in volumes which, as years rolled on, varied much in shape, but were uniform in so far as the pages were invariably plain and unruled, the Princess and Queen wrote the account of every day until within a few weeks of her death. Of the Queen's journals there are altogether over a hundred volumes, all closely written in her small running hand. The last entry is dictated and dated the 12th of January, and the Queen died on the 22nd of January, 1901. When Louis XIII. of France was born the medical attendant of Queen Marie of Medicis began to keep a journal, in which he recorded day by day for years—until, indeed, the hour of the King's death—his master's life. That journal, the most minute I know of, is a poor and meagre record compared with the journals of Queen Victoria.

Perhaps it is well here to mention that these journals will never be seen hereafter in their entirety. By the Queen's express wish, they have been carefully examined by her youngest daughter, who with infinite labour has copied in her own hand many volumes of them, excising the passages which the Queen desired should not be seen by any eye but hers. Still, when this pious work is complete, the story of a Royal and noble life will be without any parallel. All the earlier journals, certainly up to the date of the Queen's marriage, and during that year she began her 24th volume, are untouched and remain in her own handwriting.

Imagine the small fair child, fatherless and companionless, except for her devoted mother and her "Faithful Lehzen," as she always called the lady who watched over her youth, sitting at the window of a rather plain room in Kensington Palace on those June days in 1832. The echoes of the great reform controversy raging out of doors failed to penetrate those quiet precincts as she wrote her first entries in these journals.

A DAY OF HER LIFE.

Here is the account she gives of a day of her life. As I have said, I am allowed to quote by permission of his Gracious Majesty the King:—

"Thursday, 21st February, 1833. I awoke at 7 and got up at 8. At 9 we breakfasted. At $\frac{1}{2}$ past 9 came the Dean till $\frac{1}{2}$ past 11. At 10 minutes to 12 we went to pay a visit to Aunt Gloucester. At $\frac{1}{2}$ past 1 we lunched. At 2 came the Duchess of Northumberland. At

3 came Mr. Steward till 4. At 4 came Mad. Bourdin till $\frac{1}{2}$ past 4. At 7 we dined. At 9 we went to the play to Drury Lane, with Jane, Victoire and Lehzen, as usual. It was *The Sleeping Beauty* or *La Belle au Bois Dormant*, for we came at the end of *Don Juan*. The Sleeping Beauty is a very pretty ballet, in three acts, but it would take me too much time to enumerate. The principal characters were, Princess Iseult, Mdle. Duvernay, who is a very nice person : she has a very fine figure and dances beautifully, so quietly and so gracefully, somewhat in the style of Taglioni. She appeared in three different dresses, but in my opinion she looked best when she danced in the Dance of the NAIADES as the Spirit of the Princess. We came home at 1. I was soon in bed and asleep."

The writer was thirteen and a half years old. For five years this daily record continues, and we have a simple and extraordinarily graphic picture of a young girl, whose high destiny was but half revealed to her, enjoying the theatre and fine music with passion, galloping about on her pony, reading history with the Dean of Chester, washing her pet dog, and making short abstracts of the sermon on Sunday.

ANOTHER PASSAGE.

Here is another typical passage :—

"Tuesday 14th July, 1835.—I awoke at 7 and got up at 8. At 9 we breakfasted. At $\frac{1}{2}$ past 9 we walked out till a $\frac{1}{4}$ past 10. At 11 came the Dean till 12. At 12 came Mr. Westall till 1. At 1 we lunched. The Duchess of Northumberland was present at the first lesson. At $\frac{1}{2}$ past 2, I sat to Mr. Collen till $\frac{1}{2}$ past 3. At a $\frac{1}{4}$ to 4 came the Dean till a $\frac{1}{4}$ past 4. At 5 we went out with Lehzen and came home at 6. At $\frac{1}{4}$ to seven we dined. Lady Theresa dined here. At 8 we went to the opera with Lady Theresa and Lehzen. It was the *dear Puritani*. Grisi was in perfect voice and sang and acted *beautifully* ; but I must say, that she shows her many fatigues in her face, and she is certainly much thinner than when she arrived. It is a great pity, too, that she now wears her front hair so much lower than she did. It is no improvement to her appearance, though (do what she may) *spoil* her face she *never* can, it is too lovely for that. And besides, she forgot to change her dress when she came on to sing the Polacca. In general she comes on to sing that as a bride, attired in a white satin dress with a wreath of white roses round her head ; instead of which, she remained in her first dress (likewise very pretty) of blue satin with a little sort of handkerchief at the back of her head. Lablache, Tamburini and Rubini were also all 3 in high good voice. The exquisite quartet 'A te o cara,' and the *lovely* Polacca 'son vergin vezzosa,' were both enchored as was also the *splendid* duet 'Il rival.' After the opera was over, Grisi, Rubini, Lablache and

Tamburini, came out and were loudly applauded. The two last always make a separate bow to our box, which is very amusing to see. We came away immediately after the Opera was over, for the ballet is not worth seeing since *La Déesse de la Danse* has flown back to Paris again. She appeared for the last time on Saturday, the 4th of this month. We came home at 10 minutes to 12. I was *highly amused* and *pleased* ! We came in while Tamburini was singing his song, which is just before the lovely duet between Grisi and Lablache."

THE GIRL QUEEN.

Then suddenly, this young girl was awakened out of sleep, and found an Archbishop kneeling at her slippered feet, acclaiming her Queen. The passage is well known, and is published in the correspondence.

"I was awoke at 6 o'clock by Mama, who told me that the Archbishop of Canterbury and Lord Conyngham were here and wished to see me. I got out of bed, and went into my sitting room (only in my dressing gown) and alone, and saw them."

A few lines further on she writes :—

"Since it has pleased Providence to place me in this station, I shall do my utmost to fulfil my duty towards my country ; I am very young, and perhaps in many, though not in all things, inexperienced, but I am sure that very few have more real good will and more real desire to do what is fit and right than I have."

Then again, writing the account of this, to her, most wonderful day, she says :—

"At 9 came Lord Melbourne, whom I saw in my room, and of course quite alone, as I shall always do all my Ministers. He kissed my hand, and I then acquainted him that it had long been my intention to retain him and the rest of the present Ministry at the head of affairs, and that it could not be in better hands than his. He again then kissed my hand."

HER 18TH BIRTHDAY.

The Queen was 18 years and three weeks old. There had been potent forces at work moulding her character, and preparing her for this supreme moment. Three weeks before she writes in her unpublished journal :—

"Wednesday, 24th May.—To-day is my 18th birthday ! How old ! and yet how far am I from being what I should be. I shall from this day take the *firm* resolution to study with renewed assiduity, to keep my attention always well fixed on whatever I am about, and

to strive to become every day less trifling and more fit for what, if Heaven wills it, I'm some day to be ! ”

Here is another extract :—

“ Thursday, 15th June.—Got up at 8. After 9 we breakfasted. The children played in the room. At 10 Mary, dear Lehzen and I drove out and came home at 10 minutes to 11. *Wrote ! !* The news of the King are so very bad, that all my lessons save the Dean's are put off, including Lablache's, Mrs. Anderson's, Guazzaroni's, etc., etc., and we see *nobody*. I regret rather my singing-lesson, though it is only for a short period, but duty and *proper feeling* go before all *pleasures*. 10 minutes to 1.

“ I just hear that the Doctors think my poor Uncle the King cannot last more than 48 hours ! Poor man ! he was always kind to me, and he *meant* it well I know ; I am grateful for it, and shall ever remember his kindness with gratitude. He was odd, very odd and singular, but his intentions were often ill interpreted.

“ Wrote my journal. At about $\frac{1}{4}$ past 2 came Lord Liverpool and I had a highly important conversation with him—alone.”

And yet another.

“ Friday, 16th June.—Wrote to Uncle Leopold. At a $\frac{1}{4}$ to 2 came Stockmar and stayed till 3. Had a long and important conversation with him.”

Her uncle Leopold of Belgium and his trusted emissary Stockmar had spoken very privately, but very gravely to her, and with due subservience to the Powers of Heaven, but to none other, she was ready to take up the burden of Kingship, and with *her* Ministers to govern *her* Kingdom.

AFTER HER ACCESSION.

Two days after her Accession the journals strike a more girlish note :—

“ Saturday, 24th June.—Saw Lord John Russell. *Wrote*. I really have immensely to do ; I receive so many communications from my Ministers but I like it very much.”

Three days later the young Queen writes with evident and confirmed delight :—

“ Tuesday, 27th June.—Got up at $\frac{1}{2}$ past 8. At $\frac{1}{4}$ to 10 we breakfasted. The children played in the room. Wrote my journal. At about 20 minutes past 11 came Lord Melbourne and stayed till $\frac{1}{2}$ past 12.

“ A little after $\frac{1}{2}$ past 12 came Lord Palmerston and stayed till a little past 1. He is a clever and agreeable man. Saw Lord John

Russell and Lord Melbourne for a minute. At a few minutes past 2 I went down into the saloon with Lady Lansdowne ; Col. Cavendish, the Vice-Chamberlain (Lord Charles Fitzroy), and the Comptroller of the Household (Mr. Byng) were in waiting. Lord Melbourne then came in and announced that the addresses from the House of Commons were ready to come in. They were read by Lord John Russell and I read an answer to them both. Lord Melbourne stood on my left hand and Lady Lansdowne behind me. Most of the Privy Counsellors of the House of Commons were present. After this Lord Palmerston brought in the Earl of Durham, who is just returned from St. Petersburg. I conferred on him the Grand Cross of the Bath. I knighted him with the Sword of State which is so enormously heavy that Lord Melbourne was obliged to hold it for me, and I only inclined it. I then put the Ribbon over his shoulder. After this the foreign Ambassadors and Ministers were severally introduced to me by Lord Palmerston. I then went upstairs and gave audiences to the Earl of Mulgrave and to the Earl of Durham. The latter gave a long account of Russia.

"Did various things. Saw Stockmar. As I did not feel well I did not come down to dinner, but dined upstairs. I went down after dinner. Stayed up till 10. I wore the blue Ribbon and Star of the Garter in the afternoon."

"A WONDERFUL AND MYSTERIOUS DUTY."

In this land, and keeping the doctrines of the Revolution of 1688, and the Act of Settlement in remembrance, we may be sure that the young Queen had no illusions about "Divine right" to rule, but it is clear at this time that she was conscious of a wonderful and mysterious duty which had been imposed upon her by Divine Providence, and this conscientious obligation remained in her mind all her days. Dogma had but little place in her inner life, but her character and conduct as Sovereign and woman were influenced by deep religious conviction of the sacredness of her calling. She believed and acted upon the belief that her country was governed under the form of a Monarchy, of which she was not only the spiritual and temporal head, but the appointed guardian, and through all her actions this predominant note can be traced. Mr. Canning was in the habit of saying that the British Constitution was a Monarchy checked by two Assemblies—one hereditary, independent alike of Crown and people ; the other elective, springing from the people ; "but," he said, "there are some who argue as if it were originally a democracy, merely inlaid with a peerage and a Crown."

Queen Victoria had no doubts and no misgivings about the matter. Through the close-written volumes of journals to which I have alluded there can be traced this firm conviction, unchallenged, as it seemed to her, that it was *her* duty and function to choose the

best men to govern her country and her people, and to watch carefully lest in foreign affairs or domestic politics, or in administration or in legislature, or in the choice of instruments, her Ministers—as she deemed them—should betray her confidence or swerve from the paths of their predecessors. She laid strong stress on precedent, and although she rarely expressed views on domestic affairs, she believed herself to be responsible for continuity in the forms of government and for stability in foreign policy.

“SOVEREIGN OF THESE REALMS.”

There are in the Archives at Windsor, of which I have charge, 1050 volumes of papers, the correspondence of Queen Victoria, bound in large folio volumes, and there will be another 200 volumes to be added when the arrangement of these papers is complete. Through them all, from the earliest letters to and from Lord Melbourne, some of which have been included in the book published last year, to the last letters to and from Lord Salisbury, there appears the sentiment and convictions I have described. The Queen, with unconscious heroism, not only was always herself, but thoroughly believed in herself, as Sovereign of these Realms. From the passages I have quoted it can be seen how thoroughly, as a young girl—almost a child—she “took herself seriously,” to use a homely phrase, and her point of view never changed as time rolled on. On the very day of her Accession, and ever afterwards, she never seemed to doubt that the country was *hers*, that the Ministers were her Ministers, and that the people were *her* people. Ministers and Parliaments existed to assist *her* to govern. She was the Ruler of her Kingdom, and the Crown was, in her eyes, not the coping-stone of the fabric, but the foundation upon which the fabric rested.

This outlook, with its pathetic earnestness, and at times almost tragic persistence, was the source of the Queen’s influence, and sometimes the cause of her few mistakes. It helped her to safeguard the regal tradition, and it enhanced in her eyes the virtue of precedent. She became cautious in the selection of confidants, and wary in granting assent. She wished to know everything that her Ministers proposed to do in good time, so that she might consider before approving. She became insatiable for detail. In foreign affairs, and whenever interests affecting the Navy or the Army were under discussion, she expected to be consulted, and indeed insisted upon it. The Prince Consort, with that intense earnestness that breathed through every fibre of his nature, became her willing partner and helpmate. Undoubtedly to the influence of Baron Stockmar, who had been the travelling companion of Prince Albert, and who showed himself to be a profound student of English social and political life, can be traced these convictions, so strongly held by the Queen and by the Prince.

King Leopold and Stockmar, and the Prince Consort later, and the Queen apparently always, believed that control and independent criticism by the Crown was the most effective check upon the danger which besets Constitutional Monarchies of leaving the administration of State affairs in the hands of specialists. From the critical zeal of the Queen and of the Prince, Ministers occasionally suffered inconvenience ; but, as these volumes, I think, show, the country derived nothing but benefit. And if this is true, it is a lesson for all time, both for Sovereigns and for public servants.

THE BEDCHAMBER PLOT.

The correspondence of Queen Victoria illustrates in a striking manner the working of our curious system of constitutional checks and balances. After the death of Mr. Pitt in 1806 it is well known that the power and the influence of the Crown began to decline, and when the Queen came to the Throne in 1837 no one could have realised that within two years Sir Robert Peel, of all men, a spirit so proud and cold, would find himself saying to a young girl not yet twenty years old, "that he had consulted with those who were to have been his colleagues, and that they agreed . . . that unless there was *some demonstration*" of confidence, they could not undertake to govern the country. This Queen was a mere child, and these were grave men. Imagine the irony of the situation ; and yet it is the material factor in the history of our country. The Queen in later years used to speak of the episode of 1839, the Bedchamber Plot, as was called her well known refusal to part with her Whig ladies, when Sir Robert Peel tried to form a Tory Government, and she used to say that although she knew she had acted wrongly, she had never been able to determine what, under the circumstances, would have been the right course to take. The Queen's action, the action of this young girl, resulted in the return of Lord Melbourne to office, so curiously was the power of the Crown directly and effectively exercised by a youthful and female Sovereign. Possibly her youth and sex accounted somewhat for the result.

Here is the Queen's description of the matter :—

"When to my utter astonishment he asked me to change my Ladies—my principal Ladies!—this I of course refused ; and he upon *this resigned*, saying, as he felt he should be beat the very first night upon the Speaker, and having to begin with a minority, that unless he had this demonstration of my confidence he could not go on ! You will easily imagine that I firmly resisted this attack upon my power, from these people who pride themselves upon upholding the prerogative ! I acted quite alone, but I have been, and shall be, supported by my country. . . ."

THE QUEEN'S COURAGE AND CONFIDENCE.

The point which I wish particularly to bring out is that the Crown exercised in this case real power by direct action, although in later years the Queen realised, with profounder wisdom and after a long experience, that the real power of the Crown lies along the path of influence and not of direct action. But perhaps the most striking and abiding interest is the light which is thrown upon the Queen herself. Already she had learnt the use of the words "power" and "prerogative." She shows courage and confidence, courage to "act quite alone" and "confidence in my country." These two qualities of courage and confidence never deserted her through the long years that followed.

In the dismal Crimean winter of 1854, in the terrible summer of 1857, amid the horrors of the Mutiny, in the dark days of 1900, amid the losses of brave troops, her high spirit was unshaken and her confidence undimmed. Others quailed, but the Queen never. She scouted the idea of failure. "All will come right!" was her constant cry. There was nothing fatalistic about her optimism. It was based on profound faith in the reasonableness and endurance of the English people—characteristics which she shared with those Puritan classes whom she so thoroughly understood, and who never once misunderstood her.

THE INFLUENCE OF THE CROWN.

Let us return for a moment to the influence of the Crown upon English politics. The character of the Queen is a factor of the greatest importance if the contention is sound that it was her influence, rather than her direct action, as Sovereign which revived the interest of the British people in monarchical institutions and in a certain degree remoulded the Constitution. "In England the Constitution changes incessantly; or, rather, it does not exist." That was the view of an eminent French writer, often quoted; and in the hundred years which elapsed between the accession of the Queen's grandfather, George III., and the death of the Prince Consort, a student of constitutional history can trace at least three different systems of government. George III. during his healthy and vigorous manhood reigned and governed. After the death of Mr. Pitt the government passed under the control of an oligarchy, and neither George IV. nor William IV. exercised much direct or indirect power.

But I think the correspondence shows that from the moment Queen Victoria ascended the Throne a change began, and the indirect power of the Crown, with the assistance of King Leopold and Stockmar, and finally of the Prince Consort, was strengthened year by

year, until publicists came to believe that what was in reality the outcome of unique circumstances, and moral conditions dependent mainly upon the sex and characteristics of the Queen, was inherent in the Constitution itself. It was Mr. Gladstone who pointed out how considerable in amount was "the aggregate of direct influence normally exercised by the Sovereign upon the counsels and proceedings of her Ministers." He was alluding to the direct influence of the Queen, and not to her indirect influence, which he well knew was greater still.

Mr. Gladstone's language, according to his habit, was guarded, but he was making no reluctant admission. Although during his long public life, especially in later years, he was often impeded, and, he may have sometimes thought, harassed, by the desire of the Sovereign to know and to question, he was to the end of his days fully alive to the valuable influence of the Crown in public affairs, and always anxious to safeguard the prerogatives of the Sovereign. "In office or in Opposition," says his biographer, "he lost no opportunity of standing forth between the Throne and even a faint shadow of popular or Parliamentary discontent." Nor, it may be added, did he hesitate to appeal for support, as in the case of Irish Disestablishment, to the influence of the Queen; nor, as in the case of the abolition of purchase in the Army, did he shrink from advising the use of her prerogative.

MR. GLADSTONE AND THE QUEEN.

If Mr. Gladstone, with his popular sympathies, his masterful disposition, and his wide experience of public affairs, considered it one of his special duties as Prime Minister, as distinguished from his Cabinet, to watch and guard the relations between the Crown and the people of this country, it can only have been because he was keenly alive to the value of the Crown to the country. If, as has been said, he stood in awe of the Crown as an institution, and if his standard for the individual who represented it was exacting, it could only have been because he shared the Queen's fervent belief in the essential good which the Throne and the occupant of it could exercise in the interests of the people. I do not choose Lord Melbourne to bear witness to the character of Queen Victoria, or to the uses of the Crown, for Lord Melbourne was too much under the charm of the young girl, whose early steps, she herself has told us, he guided, and whom he cherished, if the word is permissible, as a father might a daughter. He could never quite forget the figure of the girl-Queen, stepping, as it were, from innocent sleep, with bare feet and dazzled eyes, upon the slippery steps of her Throne.

I do not choose Lord Beaconsfield, for just as the imagination of the author of "A letter on a Regicide Peace" was inflamed by a glimpse of the French Queen at Versailles, so was the imagination of

the author of "Tancred" fired in 1877 by the Empress-Queen, of whom forty years before he had written—"We will acknowledge the Empress of India as our Suzerain, and secure for her the Levantine Coast. If she like, she shall have Alexandria as she now has Malta: it could be arranged. Your Queen is young; she has an *avenir*." Never was there a more curious example of a statesman who "wrought in brave old age what youth had planned." I choose Mr. Gladstone because he was a Minister of the Crown three years before Queen Victoria ascended the Throne, and his death preceded hers by less than three years; because his long life coincided with hers, and because, as is well known, there was no great sympathy at any time between what has been so deftly called the Queen's fixity of nature and Mr. Gladstone's eager, mobile, versatile range.

THE THRONE AS AN INSTITUTION.

On one occasion, at the most tragic moment of the Queen's life, in December 1861, it is true that for a short while these two unsympathetic temperaments came into close harmony.

Of the vast number of letters of condolence received by the Queen on the death of the Prince, all of which were carefully preserved, she must have conceived some preference for Mr. Gladstone's, as it is noteworthy that he was the only writer who received a reply begging him to write again. But this was a mere flash. If Mr. Gladstone idealized the Throne as an institution, and if he recognized the Queen's sincerity, frankness, and love of truth, his judgment may be accepted as unswayed by intimate association with the Sovereign. If he spared neither time nor toil in endeavours to explain his policy and actions to the Queen, it was not from motives of personal devotion, so much as because he felt deeply, to use his own words, that those responsible for decisions of State "should make it their business to inform and persuade the Sovereign, not to overrule him."

If Mr. Gladstone's masterful nature, charged with popular sympathies, thought it worth while to give time and toil to the task of informing and persuading the Sovereign, it could only have been from a strong sense of the value to the people of this country of the Throne as an institution. His biographer suggests that Mr. Gladstone was deeply moved by his sense of chivalry and his sense of an august tradition, and I would not venture to disagree, but I feel confident that Mr. Gladstone was also largely influenced by his long Ministerial experience and his intimate knowledge of the inner working of the Constitution. If that is true, and if Mr. Gladstone's formed judgment was based on fact and experience, it is justified by much of what has been revealed in the published correspondence of Queen Victoria.

THE QUEEN AND POLITICAL INITIATIVE.

But there is even stronger proof at present unrevealed, for it was after 1861, when the published correspondence closes, that, owing to the responsibilities of high office and personal intercourse, he obtained a deeper knowledge of the inner workings of the Monarchical system under our institutions, and a firmer basis for his reasoned opinions. The story, however, as it is unfolded to the reader of the Queen's Letters, illustrated clearly Mr. Gladstone's so-called "idealism," and explains his point of view. Only a very few instances can be quoted. These do not show—and this is a cardinal point—initiation by the Sovereign of foreign policy, or attempts to divert into some special political channel the course of public events. There are no signs of doctrinaire statecraft, or claims to authority or privilege. They do, however, illustrate, in clear and unmistakable fashion, the most important attributes, the retarding and arresting action of the Crown.

As I have said, the Queen very rarely took what is called political initiative. That function, so clearly Ministerial, was as a rule left scrupulously alone, although in the free domain of science and art the Prince Consort showed a stimulating zeal and a marked capacity for originating new departures. The first of the great series of International Exhibitions was promoted by him, and the success achieved was mainly due to his persistence and unwearied activity of mind and body. But if the Queen rarely initiated a policy, she could be pertinacious and consistent. Many times during her long reign she encouraged the flagging energies of her Ministers, and urged them to be consistent in their aims and to show firmness in carrying out a policy to which they had committed the nation.

THE EXERCISE OF PATRONAGE.

In another important sphere of government she showed unremitting care. She scrutinized the exercise of patronage by public servants, and no appointment of serious importance, whether ecclesiastical, naval, military, or civil, could be made unchallenged by and unexplained to the Sovereign. The comparative inaccessibility of the Crown to ordinary influence was realized by the Queen, and her letters, full of heart-searching upon these matters of patronage, show how keenly alive she was to the nature of the trust she believed herself to hold for her people. Every appointment had to be explained and justified, sometimes at considerable length and in minute detail, by the Minister recommending it.

The Queen rarely approved a "submission" unless the reasons were fully stated, and often they had to be re-stated, and oftener supplemented, in consequence of queries from the Palace. In September 1841, when the Queen was only two-and-twenty years

old, she is found suggesting to Sir Robert Peel that, "for the future it would be best in all appointments of importance that before a direct communication was entered into with the individual intended to be proposed, The Queen should be informed of it, so that she might talk to her Ministers fully about it"; and she tells Sir Robert that "she feels it her duty to state freely and at all times her opinion," and begs him to do the same. It is clear that, young as she was, the Queen's expression of opinion was welcomed by Peel and Melbourne, as a support against pretensions which they found difficult to resist, but it is also clearer that they both welcomed the clarifying process of having to explain and argue the claims of candidates for high appointments before so unbiassed a tribunal. In the process the patience of a Minister may often have been tried, but the value to the service of the people, of a system which rendered jobbery difficult and imposture unlikely, cannot well be over-estimated.

KING LEOPOLD'S ADVICE.

The advice of King Leopold to the Queen on her accession had been never to decide a question of importance on the day when it was submitted to her. He, wise ruler as he was, had made it a practice not to let any question be forced upon him for immediate decision. Even, he writes to the Queen, when he was disposed to accede he always kept the papers with him some while before he returned them. He urged her to get every proposal laid before her in writing, though it had been made in the first instance verbally by a Minister.

This golden rule, reiterated by Stockmar and enforced by the Prince Consort, was invariably adhered to by Queen Victoria to the end of her life, and we may safely attribute to the habit thus formed the avoidance of many mistakes, not only by her, but by her Ministers. In the year 1841, a year of momentous change for the Queen, when she lost Lord Melbourne, her first Prime Minister, we find him, after his resignation, urging Sir Robert Peel to write fully to her Majesty, and elementarily; and he again lays stress on the necessity for caution in giving verbal decisions. It was similar advice to that offered by King Leopold, but from a wholly different quarter, not from the point of view of a Monarch, but of a Minister. The underlying reason was the same. It was to ensure the clarifying process, and the avoidance of avoidable error by Minister and Sovereign.

CORRESPONDENCE WITH PRIME MINISTERS.

If the remarkable correspondence between Lord Beaconsfield and the Queen is ever published, nothing will be found to be more striking than the minute care with which he, notwithstanding his perspicacity

and infinite resource, reasoned and debated in daily letters and memoranda the successive stages of his foreign policy. No one can read those documents without conviction that they were written quite as much for his own enlightenment as for that of the Sovereign. Mr. Gladstone paid even a higher tribute to the value of this invaluable function of the Crown by the care he bestowed upon the letters written to the Queen, when he was almost overwhelmed by the pressure of a controversy such as that which raged over the Disestablishment of the Irish Church.

Every reader of the correspondence will, I think, be even more forcibly struck by the effect of this process in the higher sphere of foreign politics, where the interests of the country were vitally concerned, as exemplified in the long and grave disputes between the Crown, Lord Palmerston, and Lord John Russell. It would be wearisome to unravel once more these old controversies, even if anything was to be gained by attempting to decide upon their merits. It is sometimes asserted that the Queen never carried her point, and that she invariably in the end had to give way. Even if this were true it would be a misleading statement—if the deduction is that the remonstrance was in vain and the time wasted. The discussion was the important thing—discussion in an atmosphere free from political dust-clouds, whichever way the issue was decided.

THE QUEEN AND LORD PALMERSTON.

Readers of the correspondence cannot fail to notice a certain aristocratic showiness—I was about to say vulgarity—about Lord Palmerston's methods, which in those days captivated his fellow-countrymen. But they lowered him, and the cause of freedom which he finely represented, in the eyes of even his well-wishers abroad. Often the tone of his despatches was softened by the suggestions of the Queen and of the Prince. It was not the advice he gave, in his haughty way, to foreign Governments that moderate men objected to, but his mode of giving it. If the Queen disliked his diplomatic style, she objected more to his studied withdrawal from her on certain occasions of a privilege highly prized—her right to see despatches on foreign policy before they were sent abroad.

To read despatches was no perfunctory duty for the Sovereign. It has seemed absurd to superficial observers that venerable statesmen of the highest ability—Lord Palmerston, Lord John Russell, Lord Aberdeen—should have been constrained to submit grave State papers upon highly technical matters to a young Sovereign and her husband for criticism and approval. These harassed statesmen, perhaps momentarily irritated, may not have realized so fully as we realize now the importance which attached to a system which, by an indirect and circuitous method, enforced reconsideration rather than control, and obtained an appeal from the Foreign Secretary to the Prime

Minister. Every one knows that in theory this check is ever present in a Cabinet. In practice, however, it very frequently lapses or is evaded.

To consult the Prime Minister before sending a despatch which might have determined the policy of the country was, in practice, at the option of the Foreign Secretary. Lord Palmerston sometimes consulted Lord John Russell, oftener he did not. But, oftener still, the Prime Minister, worried by duties of Cabinet management, did not apply his mind except perfunctorily to what was primarily the business of a colleague. The useful clash of different minds upon affairs of capital importance was tacitly avoided, and consequently lost. It was in these cases, and they are many, when either from the Prime Minister's absorption in other work, or from his failure to grasp at hasty sight the full meaning of Lord Palmerston's phraseology, that the criticism or remonstrance of the Sovereign led always to reconsideration and almost invariably to amendment.

THE PRINCIPLE OF CABINET RESPONSIBILITY.

The value, the inestimable value, of the delay imposed by the Crown was not to obtain sanction for the view the Sovereign happened to express—that was not the vital issue—but to get the intellect of another statesman of first rank, and often of the whole Cabinet, applied to a problem which could not safely be left to be solved by a single mind.

There are many illustrations of this thesis scattered through the volumes of the Queen's Letters, not only in relation to foreign, but to domestic affairs. In every case the Sovereign was triumphant, if triumph is measured, not by the ultimate issue, but by the vindication of this sound principle—that the act of a single Minister should not be allowed to commit the country to a vital policy without the conscious and reasoned adherence of his colleagues in the Cabinet. The ultimate decision was a minor consideration, compared with the principle of Cabinet responsibility as against individual Ministerial action.

For this the Queen fought steadily all her life. So watchful was she, that often we find her calling the attention of the Prime Minister of the day, not to the action, but to the speech of some colleague who, in her view, appeared to compromise the responsibility of the Government as a whole by some unwary or unauthorised declaration. It was her opinion, expressed on many occasions, that if a Minister made speeches in the country, he should not outstep the limits of Cabinet agreement, and that he should not be permitted to pledge himself to a policy without at the same time pledging his colleagues. That the Queen was right in her interpretation of constitutional doctrine need not be argued, as every one of her Prime Ministers supported her view.

It may be thought that any Prime Minister of strong character and vigorous intellect would enforce these rules. Experience shows, however, that all Prime Ministers—Lord John Russell often, Lord Beaconsfield once or twice, Mr. Gladstone frequently—are tempted to turn a blind eye towards a too impetuous colleague.

On the other hand, there is no example, in this correspondence, of a Minister not appreciating with some relish the support against an unruly colleague which was offered him by the Sovereign.

THE QUEEN'S HATRED OF WAR.

Another point well worth noting is that the most careful scrutiny of the published and unpublished letters shows beyond dispute that the influence of the Crown was uniformly asserted in the interests of peace and against action which might lead to war. Although no one could show rarer determination when once the die was cast, and more firmness to reap the fruit of national sacrifices than the Queen, there is no instance in the whole of her reign where she can be shown to have favoured war, or encouraged those who were anxious for it. There are many to the contrary. Two will suffice.

It was largely due to the pertinacious support given by the Queen to those members of the Cabinet who in 1850 favoured peace that England was not dragged by Lord Palmerston and Lord John Russell into the contest between Prussia and Denmark. It was an occasion when the sentiment of the country and the policy of a powerful Minister came into conflict, and no one, reading the inner history of that conflict of opinion, can doubt that the peaceful issue was largely determined by the action of the Queen. The non-intervention of Great Britain in 1850 was largely due to the joint endeavour of the Queen and the Prince Consort; and, later on, it was to the infinite credit of the Prince that in 1861, at a moment of national heat and excitement, this country was saved from the crime of a war with the United States. The proofs of these statements are to be found in the volumes of the Queen's Correspondence.

We are not concerned, however, with the merits of these bygone controversies. The question as to which policy was right may still be argued. But we are concerned with the illustration afforded by the Queen of the effect of throwing the whole weight of the indirect influence of the Crown into the scale of peace. Had she acted otherwise the result would have been to cast doubt upon the institution of Monarchy, and possibly, at some period, to have jeopardised the Crown.

FLEXIBILITY OF THE UNWRITTEN CONSTITUTION.

At this point may I pause for a moment to note once more the singular flexibility of our unwritten Constitution, and the ease and smoothness with which the relations between the Crown and a representative Government adjusted themselves to varying conditions?

A young Queen exercises her doubtful prerogative with the support of a Liberal majority in a reformed House of Commons against a powerful Tory combination headed by Sir Robert Peel and the Duke of Wellington. What a paradox is here! A Liberal Prime Minister of great determination endeavouring to force a measure of Irish Disestablishment through a Tory House of Lords appeals to the Sovereign for assistance, and achieves success through her mediation. What a confusion of democratic ideals is there! Institutions more hide-bound, less malleable, would not have stood the strain—and these volumes of the Queen's Correspondence contain lessons for all who, for the sake of symmetry, or abstract polity, or momentary convenience, may desire to substitute dogmatic restriction and a statutory formula for so flexible a medium of Government.

If I have made my meaning clear, the value of a Monarchical system like ours should be enhanced by a study of the Queen's Correspondence. Unqualified eulogy would be unworthy of our subject, and the last thing Queen Victoria would have desired. In her public capacity as Sovereign of these realms she occasionally committed errors of judgment; but not often. It would be vain to select examples either for praise or blame. We have been engaged upon an examination of causes and results, rather than upon a critical estimate of specific acts.

HER MAJESTY'S "ONLY SERIOUS ERROR."

This, however, I should like to say. I have had exceptional opportunities of examining at first hand the inner history of a reign, extending over sixty years, during which every document was preserved—even the least important of telegrams. It has been my duty to arrange this vast mass of political papers with as much care as I could devote to the task, and I can assert with the fullest conviction, that I have found no trace of any grave mistake committed by the Queen in her capacity as Sovereign.

Perhaps the only serious error made by the Queen was her seclusion during the long period from 1861 to 1874—when she allowed her deep feelings as a woman to prevail against the claims made upon her as Head of the State. But these claims were of the lesser kind. The greater claims she met during those years in a degree which will only be fully realized, if it should become possible to publish a further selection of her correspondence during that period. She displayed none of the graver faults of the greatest of her predecessors on the Throne. Although often treated with ingratitude she never showed the resentment of Elizabeth. The cold indifference characteristic of William III. was foreign to her nature. Although she resembled in many ways her grandfather, George III., she could have been relied upon not to misunderstand the American Colonies.

It is necessary to speak of her private life—it was so bound up with her public life—and upon the connection between these her

influence over her people mainly rested. At this point, owing to his Majesty the King's gracious permission to quote from her Journals, the Queen has spoken and can speak for herself. I have known of no better way to bring home to you the deep underlying truth about Queen Victoria than to quote her own words, at different and characteristic periods of her life.

The passages I have quoted were not intended when they were written for any eye but hers. It was only many many years later, when confronted with fabulous statements about herself and her family, which had obtained credence, that she began to contemplate using material accumulated over a long period of time, for the purpose of giving a picture, that was truthful, of persons and events so absurdly travestied. This change of sentiment about publicity influenced her to print extracts from her Journals, and subsequently determined his Majesty the King to allow the publication of her correspondence.

FROM DOLLS TO POLITICS.

When I spoke of the importance of atmosphere in history, and the difficulty of creating it, I had, as I have said, already determined not to make the attempt. My intention was to give you pictures of the Queen in her own words. We have had a glimpse of the Child Princess in that "Palace in a Garden" which appealed so strongly to the author of "Sybil," the hours passed in the schoolroom with the Dean of Chester, or at a music-lesson, or washing her terrier "Dash," with an occasional ride on her pony, accompanied by her mother, and on Sundays making extracts of the sermon.

There was the weekly letter from her uncle, King Leopold, to be read, and perhaps a lecture to be heard in the presence of her Mother, from Baron Stockmar. She played with her dolls. There were hundreds of them, small dolls, most of which she dressed herself, and ticketed with well-known names of illustrious persons whom she had seen dining at Kensington Palace, or whom she had watched from the Duchess of Kent's box at the Opera. All these dolls were carefully preserved and are alive to this day numbered and catalogued in the young Princess's child hand.

Then suddenly she was Queen. After her accession her life completely changed. To comparative isolation and greyness succeeded a period of high tension and keen enjoyment. Rose, not grey, became the prevailing colour. Her mind expanded at the touch of this wonderful spring-time. A secluded maiden, whose only draught at the fountain of life had been an evening at the theatre, was suddenly translated from the schoolroom to the most exciting spheres of politics and of regal state. Her companions were thenceforth Ministers of State, *her* Ministers.

She no longer dressed dolls, but presided at Councils. She, who had never walked down the staircase at Kensington Palace unless held by the hand, like a little child, rode twenty miles of a morning,

at the head of a cavalcade of courtiers. She, who had spent her mornings with the excellent Dean of Chester, reading geography, now spent her afternoons with her Prime Minister, discussing the affairs of Europe.

PICTURES FROM THE JOURNALS.

But the aroma of the schoolroom was about her still. Here is her Journal for Monday, April 2, 1838 :—

“I said to Lord Melbourne I was so stupid that I must beg him to explain to me about Sir William Follett again ; he answered very kindly ; ‘It is not stupid, but I daresay you can’t understand it’ ; and he explained it to me like a *kind* father would do to his child ; he has something so fatherly, and so affectionate and kind in him, that one must love him.”

A week later the Queen writes describing one of many evenings spent with her Prime Minister at Buckingham Palace :—

“Sunday, April 8. Lord Melbourne looked over one of the Volumes (the 6th) of a work called ‘Gallery of Portraits’ : there are portraits of all sorts of famous people in it, with short Memoirs of them attached to them. Lord Melbourne looked carefully over each, reading the accounts of the people and admiring the prints. I wish I had time to write down all the clever observations he made about *all*. It is quite a *delight* for me to hear him speak about all these things ; he has such *stores* of knowledge ; such a wonderful memory ; he knows about everybody and everything ; *who* they were, and *what* they did ; and he imparts all his knowledge in such a *kind* and agreeable manner ; it does me a *world* of good ; and his conversations always *improve* one greatly.

“I shall just name a few of the people he observed upon :—*Rayleigh*. *Hobbes* ; who was ‘an infidel philosopher’ ; he had been tutor to one of the Earls of Devonshire, he said. *Knox* ; Lord Melbourne observed that those Scotch Reformers were very violent people ; but that Knox denied having been so harsh to Mary Queen of Scots as she said he had been. *Lord Mansfield*. *Melancthon* ; whose name means Black Earth in Greek, and whose head he admired. *Pitt* ; whose print Lord Melbourne said was very like ;—‘He died in 1806 when I came into Parliament ;’ he (Ld M) came in for Leinster. *Wesley* ; Lord Melbourne said that the greatest number of Dissenters were Wesleyans ; he read from the book that there were (at his death) 135,000 of his followers. *Porson* ; Lord Melbourne said : ‘I knew him ; he was a great Greek scholar’ ; and looking at the print—‘it’s very like him.’ *Leibnitz* ; a great German philosopher, and a correspondent of Queen Caroline, wife to George II. ; spoke of her being so learned and her whole Court too ; ‘The Tories laughed at it very much,’ and Swift ridiculing the Maids of Honour, wrote : ‘Since they talk to Dr. Clark, They now venture

in the Dark.' *Addison*; Lord Melbourne admires his 'Spectator,' his 'Cato' he also admires, but says it's not like a Roman tragedy; 'there is so much love in it.' Addison died at Holland House; he disagreed very much with his wife, Lady Warwick. Holland House was built, he said, by Rich, Lord Holland, in the reign of Charles 1st. *Mamlame de Stael*; whose print he thought very like; 'She had good eyes, she was very vain of her arms.' She was over here in '15 and died in '17, aged 51; she disliked dying very much; Lord Melbourne also knew her daughter the Duchesse de Broglie; he said 'Louis Philippe dislikes her as much as Napoleon did *her Mother*.' Lord Melbourne saw Madame de Broglie for a moment when he was at Paris for the last time in 1825. He read from the book, and with great emphasis, the following passage, what Napoleon said of Madame de Stael: 'They pretend that she neither talks politics nor mentions me; but I know not how it happens that people seem to like me less after visiting her.' *Queen Elizabeth*; spoke of her, and that her Mother must have been very handsome; etc.

"Spoke of pictures; Lord Melbourne does not admire Murillo much, nor Rubens; he so greatly prefers the Italian Masters to any others; spoke of subjects for painting; of the Holy Family being constantly painted. 'After all,' he said, 'a woman and a child is the most beautiful subject one can have.'"

Then she adds:—

"It was a most delightful evening."

A RIDE OF TWENTY-TWO MILES.

There is nothing very remarkable in these utterances of Lord Melbourne. The interesting aspect of them is the circumstances in which they were delivered. The normal evenings were spent in this fashion, following after mornings consumed in reading despatches and in signing her name, to be succeeded by afternoons occupied in riding through the streets and through the crowds that waited daily at Hyde Park Corner to see the Queen:—

"At $\frac{1}{2}$ past 12, I rode out with Lord Conyngham, Lord Uxbridge, Lord Byron, Lady Mary, dearest Lehzen, Miss Cavendish, Miss Quentain, Sir F. Stovin and Col. Cavendish, and came home at $\frac{1}{2}$ p. 3, having ridden *twenty-two miles*. . . . We rode very hard and Tartar went most *delightfully*, never was there such a dear horse. We rode to Richmond, through part of the Richmond Park, out at Robin Hood Gate, and home over Wimbledon Common and Vauxhall Bridge. It was as hot as summer, and *going* I thought I should have melted; coming over Wimbledon Common there was some delicious air. It was a heavenly day. At 6 m. p. 4 came Lord Melbourne and stayed with me till 20 m. to 5. He seemed well. Spoke a good deal of my ride."

Two more extracts from these early journals and I have done :—

"1838. Monday, July 9. At $\frac{3}{4}$ p. 11 I went to a Review in Hyde Park. I could have cried almost not to have *ridden* and been in *my right* place as I ought; but Lord Melbourne and Lord Hill thought it more prudent on account of the great crowd that I should not *this* time do so, which however now they all see I might have done. Lord Anglesey (who had the command of the day, looked so handsome, and did it beautifully and gracefully) regretted much I did not ride. I drove down the lines. All the Foreign Princes and Ambassadors were there, and the various uniforms looked very pretty. The troops never looked handsomer or did better; and I heard their praises from all the Foreigners and particularly from Soult. There was an immense crowd and all so friendly and kind to me."

A ROYAL DINNER PARTY.

"1838. Wednesday, July 25. Wrote my journal. At a $\frac{1}{4}$ to 8, I went into the Throne Room with my Ladies and gentlemen and Feo and Mama, where I found the Duchess of Gloucester, the Duke of Sussex, the Duke and Duchess of Cambridge, and Augusta and George. After waiting a little while, we went into the green drawing-room, which looked very handsome lit up, and was full of people *all* in uniform. I subjoin an account of all the arrangements, and all the people. After remaining for about five minutes in that room, talking to several people, amongst others to good Lord Melbourne, we went into dinner which was served in the Gallery and looked, I must say, most brilliant and beautiful. We sat down *one hundred and three* and *might* have been more. The display of plate at one end of the room was really very handsome. I sat between uncle Sussex and Prince Esterhazy. The music was in a small Orchestra in the Salloon and sounded extremely well. Uncle Sussex seemed in very good spirits, and Esterhazy in high force, and full of fun, and talking so loud. I drank a glass of *stein-wine* with Lord Melbourne, who sat a good way down on my left between the Duke of Devonshire and Lord Holland. After dinner we went into the Yellow Drawing-room. Princesse Schwartzenberg looked very pretty but tired; and Mme. Zavadowsky, beautiful and so sweet and placid. About 20 m. after we ladies came in, the gentlemen joined us. I spoke to almost everybody; Lord Grey looked well; the Duke of Wellington, ill, but cheerful and in good spirits. I spoke for some time also to Lord Melbourne who thought the Gallery looked very handsome; and that the whole 'did very well'; 'I don't see how it could be better,' he said. He admired the large diadem I had on.

"At about 11 came some people who (as the Gallery was full of dinner, &c.) were obliged to come through the Closet, and of whom I annex a list. Lady Clanricarde I did not think looked very well; Lady Ashley, Lady Fanny, Lady Wilhelmine, and Lady Mary Grimston looked extremely pretty. Strauss played delightfully the whole

evening in the Saloon. After staying a little while in the Saloon, we went and sat down in the further Drawing Room, next to the Dining Room. I sat on a sofa between Princesse Schwartzberg and Mme. Stroganoff; Lord Melbourne sitting next Mme. Stroganoff; and in a little while Esterhazy near him, and Furstenberg (who talked amazingly to Lord Melbourne, and made us laugh a good deal); behind him. The Duchess of Sutherland and the Duchess of Northumberland sat near Princesse Schwartzberg, and a good many of the other Ambassadors and Ambassadors were seated near them. The Duchess of Cambridge and Mama, &c., &c., were opposite to us; and all the others in different parts of the room. Several gentlemen, foreigners, came up behind the sofa to speak to me. We talked and laughed a good deal together. I stayed up till a $\frac{1}{4}$ to 1. It was a successful evening."

The language is very simple, but Macaulay's famous description of a scene in Whitehall is not more vivid.

AN ATMOSPHERE OF DEEP MEMORIES.

I have some faint hope that through the medium of these quotations from the Queen's Journals I may have been able to create that atmosphere of which I spoke. Remembering, as I myself do, what in later days that atmosphere was, I am more than diffident. During these later years, from which the published correspondence is far removed, there was a hushed reverence surrounding the Queen, hard to describe, and difficult even to suggest. It is no exaggeration to say that eminent statesmen and humbler folk alike moved through the corridors of Windsor as through a shrine. It was not the atmosphere of sycophancy or adulation. It was the atmosphere of deep memories, of noble names, of Imperial growth, of national struggles, and of glorious triumphs. It was an atmosphere of queenly pity, of intrepid courage, of personal sorrows, and of duties simply performed through long years, stretching far back beyond the remembrance of any save the Queen herself. In spite of its grandeur, there was a solitude, an aloofness, about the life of the Queen, which made men half afraid to speak above a whisper.

I have dwelt, I hope not unduly, upon the earlier years of the Queen's reign, for it is these years that the published correspondence covers. In preparing that correspondence for publication it was felt that it should tell its own story, and that no attempt should be made to analyse or discuss the character and actions of the Queen. If it should be found possible to bring the story down to a later period the same course will be followed. And I may say that the interest deepened as the years rolled on. This is not only because events are more recent, and the personalities of those who surrounded the Queen are more vividly known to us; but because after the loss of her guide and counsellor in 1861 the character of the Queen changed and strengthened. For the first time she stood absolutely alone. Al-

though, as she herself said, in her desolate and isolated condition she turned to Lord Palmerston and to Lord John Russell as old and tried friends, they did not and could not occupy the place that had been filled by Lord Melbourne in her girlhood, and by the Prince Consort through her happy married life. It is only within the last few months that by an accident the Queen's letters to Lord John Russell have come to light—and it is curious to observe that for four years she wrote to him in her own hand at least once a day. During most of that period Lord John Russell was Foreign Secretary. The Queen was learning to walk alone.

WHAT WE OWE TO QUEEN VICTORIA.

This is not the time or place in which to attempt any deeper analysis of the character of Queen Victoria. If, as Cardinal Newman once said, men are guided by type rather than by argument, and if the majority are swayed more by example than by the logic of facts, the Queen has rendered a mighty service not only to her people but to her successors on the Throne of this Kingdom. No Sovereign ever exercised over the minds of men and women of many races a more powerful influence.

We started to inquire—What we owe to Queen Victoria; what was the secret of her influence; and what will be her place in history? I venture to hope that to these questions I may have suggested, under the necessary limitations of such an occasion as this, a partial reply. We owe to Queen Victoria the reinstatement of the Monarchical principle in the eyes of all grave and earnest men. We owe to her the deep respect with which the British Crown is regarded by the subjects of this vast Empire. The secret of her influence was her unfaltering devotion to duty, her simple regard and—if the word is not misplaced—her narrow adhesion to the plain unvarnished truth in every action and relation of her long life. To attempt to expose her weaknesses would be an unbecoming and singularly fruitless task. We do not claim—those who were her loyal and devoted subjects—that she was other than extremely human. But we do claim that, in the glare of her great virtues, her faults may be allowed to lie in shadow.

The Queen's place in history cannot yet be defined. There are few more treacherous quicksands than those which surround the domain of historical forecast. This much, however, may be safely ventured: that as the reign of Elizabeth rounded off and set a seal on that period of splendid intellectual growth, during which England became one of the first of European Powers, so the reign of Queen Victoria rounded off and set a seal upon that no less heroic period of commercial and racial expansion in which Great Britain became a world-wide Empire.

WEEKLY EVENING MEETING,

Friday, March 12, 1909.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L.
F.R.S., President, in the Chair.

SIDNEY GEORGE BROWN, Esq., M.I.E.E. *M.R.I.*

Modern Submarine Telegraphy.

THIS lecture relates to modern submarine telegraphy, and, therefore, I shall omit the historical part of the subject and start with the cable itself, as we deal with it now.

The signals to form the messages are sent over the submarine cable as electric currents. The cable consists of a central copper wire : this is the conductor for the current, and to prevent the electricity escaping from the wire it is insulated along its entire length by gutta-percha.

Gutta-percha is chosen for submarine work, because of its very high insulating properties and its not being acted on, or suffering chemical change under water.

The gutta-percha covered wire is called the core : this core, before it can be laid at the bottom of the sea, must be surrounded by jute serving and steel wires for protection when being laid and during its existence after.

When dealing with the electrical properties of a cable, the core only is considered, and for all practical purposes it may be taken that the return conductor to the current is the water immediately outside the gutta-percha.

A core of any given length has a certain time rate of signalling ; that is to say, when a voltage is applied at one end, the effective current, that as a consequence flows in the wire, does not arrive at the distant end instantaneously, but takes time to grow.

The time rate of signalling is inversely proportional to the product of the resistance of the wire and the electrostatic capacity of the core.

This is termed the "K.R." or capacity resistance law, a law first pointed out by Lord Kelvin.

It follows from this law that if you double the length of any given kind of cable you reduce its speed for signalling to one quarter.

The time rate is inversely proportional to the resistance multiplied by the capacity. If you make a certain sized core (size of gutta-percha) with a large copper, up to a certain point you decrease the resistance and increase the capacity ; but there is a critical value giving

the minimum K.R. This critical limit, or the point when the size of the copper is reached to give the lowest K.R., is when the diameter of the copper is to the diameter of the core as 1 : 1·65.

There is another advantage in keeping the resistance low for any K.R : the time constant only determines the time when the current at the far end reaches a certain percentage of the possible maximum after the application of the voltage at the sending end. Of course, the quantity of current after any given time is determined again by the voltage of the sending battery and is inversely as the resistance of the cable.

For instance, if two cables were constructed of equal K.R. but one had a larger copper of half the resistance of the other, with equal sending batteries the one with the lower resistance would deliver twice the current at the receiving end, at the ends of equal times, and could therefore be made to work at a faster rate. It should also be a cheaper cable, because copper is less expensive than gutta-percha.

Against these electrical advantages should be placed several mechanical disadvantages : the reduction of the thickness of the insulation might result in a greater liability to faults developing after the cable was laid.

With such a heavy wire, which would naturally have to be well stranded, to reduce the stiffness, the liability of the decentralisation during manufacture would be greater than with existing cores.

These mechanical difficulties could, I feel sure, be overcome, say by greater care being taken in the manufacture, or by substitution for the present yielding gutta-percha of dry cotton or similar material well impregnated with gutta-percha compound.

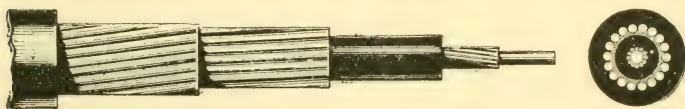


FIG. 1.—ATLANTIC 1894 CABLE.

I take an Atlantic cable laid in 1894 (Fig. 1) as having the greatest size of copper for size of core ; I take this core to illustrate the improvement that might result by increasing the copper up to the largest size electrically permissible :—

1894 CABLE.				
Diameter of core	.	.	.	0·466 inch
„ of copper	.	.	.	0·202 inch
Resistance per nautical mile	.	.	.	1·684 ohm
Capacity „ „	.	.	.	0·420 microfarad

The cable is 1852 nautical miles long, and its K.R. is 2·41, and its speed of working under the capacity block system of duplex about 205 letters per minute.

THE IDEAL CORE. (FIG. 2.)

Diameter of core	0.466 inch
„ of copper	0.282 inch
Resistance per nautical mile	0.864 ohm
Capacity	0.700 microfarad
K.R. for 1852 nautical miles	2.06

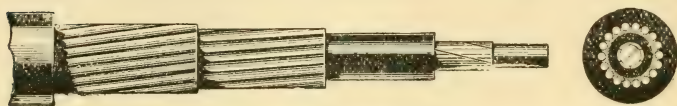


FIG. 2.—IDEAL CABLE.

Speed of working with same duplex system about 240 letters per minute, and the current received with this speed would be twice as strong as in the actual cable, so that a still greater speed than that given would result, perhaps a speed of 260 letters per minute, a sending battery of 40 volts to be used on both cables.

The copper conductor offers resistance to the electric currents that flow along it; this resistance by itself would, with sufficiently sensitive receiving instruments, not affect the speed of signalling: it produces what is termed “attenuation” or a weakening of the signalling current.

There is also a lateral storage of electricity along the outside of the copper due to the capacity of the insulating material to absorb a charge of electricity; this property is termed the electrostatic capacity of the core.

To allow this to be more fully understood, I shall take mechanical analogies:—

Resistance in electricity is equivalent to *friction* in mechanics, *capacity* to *elasticity* of a spring, and *self-induction* to *inertia*.

If I force water through an iron pipe, the friction in the pipe offers resistance to the flow of water; the same quantity that is forced in, flows out at the receiving end, but the energy accompanying the flow of water suffers attenuation, as part is wasted in overcoming the frictional resistance.

Suppose that, instead of taking an iron pipe, I take a soft india-rubber pipe, a new kind of phenomenon will be noticed. As I force the water in, the resistance that the water encounters in flowing along the pipe causes the rubber to swell, and the rubber will continue to swell until it has acquired sufficient strain to press with sufficient force on the water to overcome the friction of the pipe.

At the sending end, that is, the end where we are forcing in the water, the pipe will swell the most, because the pressure on the water is there the greatest, and the frictional resistance offered by the pipe to its flow also the greatest. As we move along, the swelling will be less, being least at the far end, that is, at the receiving end, where the water escapes.

At the instant that we start forcing the water in, practically none escapes at the receiving end, the pipe commences to stretch and the water begins to flow out, continuously increasing in quantity, until it obtains a steady value : this steady value is reached when the pipe has ceased to expand.

The time taken for the pipe to expand and for the water to reach a steady value is termed the variable period. The less the elasticity of the pipe and the less the resistance to water flowing through it, the less the time taken to reach the steady value.

This is equivalent to our submarine cable, where the less the capacity and the less the resistance, the less the time constant, or the quicker the rate of signalling.

Now the swelling of the pipe or the capacity effect of the cable does not destroy the energy in the water or of the electricity respectively ; this is very different from the waste of energy through resistance, and if by some method we could compensate for the capacity we could signal through the conductor at any rate we liked, being limited only by the strength of our battery and the sensitiveness of our receiver.

I may say that the current usually received would be 1000 times greater if we had no capacity but only the resistance to deal with.*

As before stated, the cable has resistance : the current therefore suffers attenuation. It also possesses capacity : the signalling currents through it therefore suffer distortion.

Before dealing with this distortion, I must refer you to the diagram of the signals as they are sent into the cable (Fig. 5) and received from it on the siphon recorder.

You will notice that the signals, arranged to form the alphabet in the cable code, are of varying lengths, being 1, 2, 3, 4 and 5 times the length of the individual or shortest signal.

Sending and receiving on this principle is electrically equivalent to working the cable with varying electrical frequencies of 6, 3, 2, etc., complete periods per second.

The lower the frequency the less the capacity affects the current, so that the higher frequencies of 6 and 3 a second are more attenuated than those of 2 and less. The signals that form the letters in the alphabet are differentially attenuated, the quicker signals, such as those forming a C, are much weaker when they arrive to operate the receiving instrument than the slower signals that form the letters M, O, and so on for the other and longer signals.

Submarine cable signalling of the present day affords us with an

* I must here refer to the fact that Mr. Heaviside twenty years ago showed that by giving series inductance to a cable we could greatly increase our rapidity of signalling.

This will be understood from Table I. and Figs. 3 and 4 showing curves. Unfortunately we see no practical method of carrying out Mr. Heaviside's suggestion, so that I must go on speaking of the submarine cable as it really is.

electrical illustration of that fable of "the tortoise and the hare" or the principle of "more haste, less speed."

As the slower signals get through the cable with more vigour than is necessary, the ingenuity of experimenters is to retard them and to assist as much as possible the quicker ones, so that all the signals, whatever their period, shall arrive with exactly the same strength.

Cromwell Varley in 1862 patented a system for the reduction of distortion on cables by inserting condensers of suitable capacity in series with the conductor at each end of the cable.

The reason for the abolition of distortion is obvious: the condenser absorbs the signals of slow frequency, while the cable transmits them.

The condenser allows the signals of high frequency to pass through it, although the cable has attenuated them. It is therefore possible to so arrange the condensers at each end of the line that the condensers and the cable together will more or less correct one another, and the distortion be reduced.

Unfortunately, the absorption of a series condenser is relative, and is inversely proportional to the frequency; it absorbs more of

TABLE I.

Nauts (x).	I.		II.		III.		IV.	
	Volts.	Amps.	Volts.	Amps.	Volts.	Amps.	Volts.	Amps.
0	40.0	0.1264	40.0	0.1264	40.0	0.0408	40.0	0.041
300	12.25	0.039	12.7	0.042	31.0	0.0316	23.7	0.0244
600	3.8	0.0125	4.4	0.0137	23.9	0.0244	14.2	0.0147
900	1.1	0.005	1.5	0.0055	18.5	0.0189	8.3	0.0088
1200	0.35	0.0012	0.48	0.00155	14.2	0.0146	5.1	0.0051
1500	0.15	0.00065	0.2	0.00083	11.0	0.0112	3.04	0.0031
1825	0.0453	0.000143	0.0418	0.000132	8.32	0.0085	1.71	0.00175
Total lag behind V_0	371°	371°	392°	347°	1717°	1714°	1714°	1714°

Except in Case I. (near its end) the lag in every case is proportional to x .

Frequency, 6.36 per second.

Submarine telegraph cable — $r = 1.684$ ohms per naut, $k = 0.42$ mfd. per naut. The current received by recorder would be 82 times this if we had no capacity.

At x nauts from sending end these are the volts and amperes:—

I. There is a recorder with 317 ohms resistance at the end of 1825 naut cable.

II. Infinite cable.

III. Infinite cable, 0.4 henrys per naut; no leakance. Not much distortion.

IV. Infinite cable, 0.4 henrys per naut; leakance, 1.768×10^{-6} ohms per naut to give no distortion.

(See Figs. 3 and 4.)

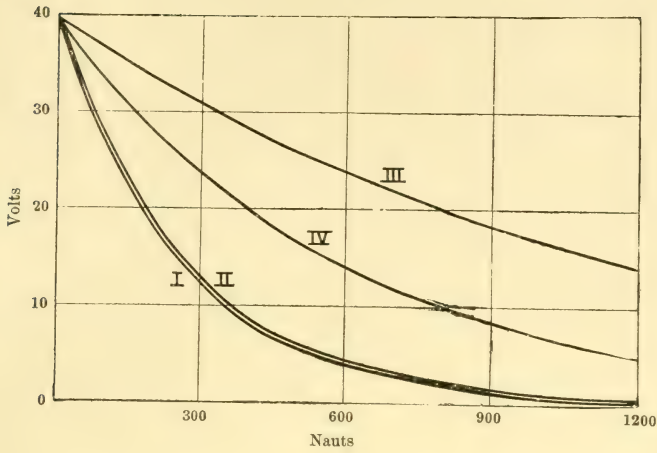


FIG. 3.

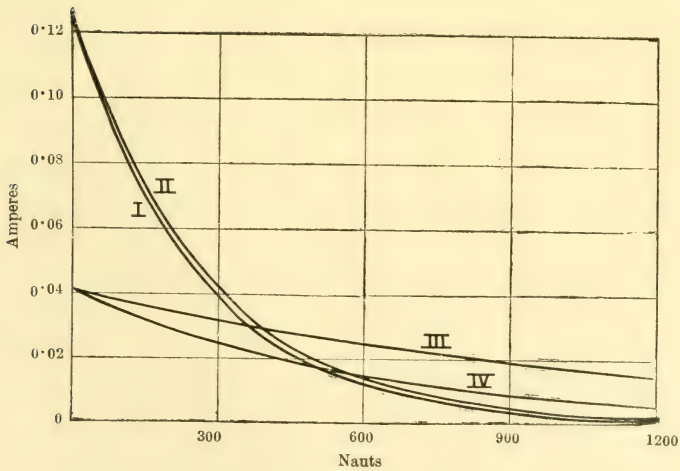


FIG. 4.

The above curves are plotted from the results given in Table I.

the slow than the quick signals; at the same time it does absorb some of the quick, and so far as that is concerned it is harmful; it diminishes distortion, but at the same time it adds to the attenuation.

Now "distortion" means something more than the differential transmission of various electrical frequencies: it also means the "phase relation" of the current to the voltage, and this "phase relation" varies with the various frequencies, so you see that "distortion" looked at from all sides is rather a complicated phenomenon.

By "phase relation" we mean the position of the current with regard to the voltage producing it.

To understand what "phase relation" means let us take the analogy of a pendulum in motion:—

The force keeping the pendulum swinging is a maximum at the end of each swing, while the greatest velocity resulting from this force is at the middle of the swing; obviously the times of greatest

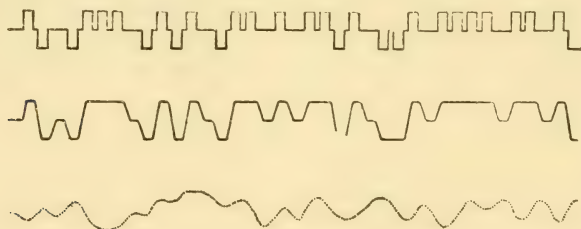


FIG. 5.

speed and greatest force are not co-incident: the one is out of phase with the other by what mathematicians would determine, in the case of the pendulum, as 90° or a quarter period.

Now the current leads the voltage at the sending end of the cable by 45° . If a series condenser is introduced to diminish distortion, it still further increases the lead, and reduces the effective power into the cable. The effective power can only be a maximum when the current and voltage are exactly in step, or in other words, when there is no "phase relation."

A receiving condenser is also harmful for the same reason as a sending condenser.

By abolishing the sending condenser and replacing the receiving one by a magnetic shunt placed across the suspended coil of the siphon recorder or relay in 1898, the speed and accuracy of signalling were materially increased.

A magnetic shunt, as employed on the cables, consists of an insulated copper wire wound round a closed circuited iron core.

The resistance of the shunt is about 30 ohms; its inductance

varies up to a maximum of from 20 to 40 henries, and its weight from 1 to 3 cwt.

In the case of a siphon recorder used as the receiver, the shunt short-circuits the suspended coil and the series condenser is abolished.

In the case of a cable relay, the series condenser is usually retained to insure that earth currents are effectually stopped, but the condenser is made large.

A shunt inductance has a similar time action on the in-coming current to that of a series condenser, but with this improvement—that it helps to reduce the phase distortion of current with voltage rather than accentuate it, as is the case with the condenser.

Having obtained the best value of the shunt alone, the following curious effect was discovered: that adding a condenser as an additional shunt, the size of the signals on the recorder got larger and more distinct. The mathematical reason for this is as follows: that for any particular frequency, say the highest frequency of the cable signalling, the shunts of inductance and capacity when properly proportioned act as a shunt of infinite resistance. For frequencies much below this it is as if we had no condenser at all. For frequencies much above this it is as if we had no inductance, but only a condenser.

To still further reduce the harmful effect of phase displacement series inductances have lately been introduced at the ends of cables, particularly at the sending end.

By placing an inductive coil of low resistance in series with the battery, at the apex of the duplex bridge, not only has the speed of signalling been increased, but the effect of what is known as “jar” on the duplex balance has also been greatly reduced.

Before proceeding to describe the instruments that work the cables, I will say a few words about “duplexing.”

All cables are now duplexed, that is to say, are arranged so that messages can be sent and received, at the same time, at each end simultaneously.

The first cables were duplexed by Stearns and later ones by Muirhead and Taylor.

Duplex reduces the speed of simplex, or of working one way only, by 20 per cent. but the total carrying power of the cable, irrespective of direction, is raised by some 70 per cent. and is for this reason valuable, and repays the trouble in maintaining the balance.

Cables are duplexed by arranging an artificial or imitation cable, which is an exact electrical copy of the real, in parallel with the real cable.

The current from the sending battery flows through two equal arms of capacity or inductance of a Wheatstone bridge arrangement, and into the real and artificial cables.

The inductive or magnetic bridge which I have applied lately

is, I think, the best to employ, because it gives, in practice, higher speeds than any other form of bridge.

The receiving instrument is joined to the commencement of the cables, and is thus not interfered with by the sending currents, because there is no tendency for the current to flow, one way or the other, the real and artificial cables having exactly the same electrical properties and acting on the sending current in the same way. But the current that is received flows only from the real cable, and is not balanced by any from the artificial, so that the receiving instrument is worked by it.

When duplex is properly adjusted it is said to be in balance, from its similarity to the adjustment of an ordinary balance used for weighing goods.

Take the ordinary balance as an illustration of the electrical one : Let one scale-pan represent the cable, the other the artificial ; if equal weights are placed in each pan the beam will not turn, but the beam will turn if, while equal weights are or are not in the pan, a small weight is added or placed on one pan.

In the cable "duplex," the receiving instrument will not be affected by the sending current because the voltage is always the same on each side of the instrument, but will turn to indicate a signal when a voltage is received or is added to or subtracted from the voltage already on the cable side, due to a voltage being applied to the cable at the far end.

In Fig. 6 is shown the simplest diagram of a cable "duplex," and Fig. 7 illustrates its mechanical equivalent ; the lettering is similarly related.

If the battery B sends equal currents into cable and artificial line, as it should do if there is a perfect balance, no current will flow through S, and thus the receiver S is unaffected by the sending voltage. Or if the pans of the balance have equal weights B placed on them, the indicator S will not move. On the contrary, if a voltage is received from the cable C, this voltage is added to or subtracted from whatever voltage may be in C at the time, due to the sending battery, and thus there will be a difference of potential across S, and the receiving instrument will be worked from currents sent from the far end of the cable and from these currents only.

In the mechanical analogy a small weight W is added to or taken from one of two equal weights in the pans C and A L, and the beam will be tilted and will be moved by this weight only, however the weights B B are varied.

The voltage of the battery as applied to the sending end of a cable is very much greater than that received from the cable to work the instrument, say in the relation of 40 volts to $\frac{1}{20}$ volts in the case of a moderately long cable, or as 800 is to 1, and the sending and received currents resulting from the same follow a similar proportion.

In the mechanical illustration I have therefore indicated the weights B and W as squares having this proportion, to give a visual indication of what this means in the balance.

The proportion I have given is only the relation of the sending voltage to that received.

If the balance were out to this proportion, the sending voltage would affect the receiver with disturbances equal in size to those due to the receiving voltage: the duplex would then be very badly indeed out of balance.

To receive properly, the sending voltage must produce no movement of the receiver whatever; that is to say, that any disturbance

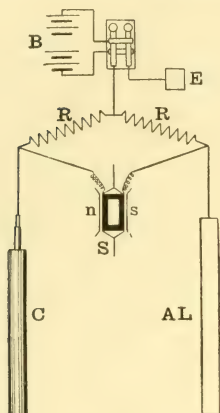


FIG. 6.

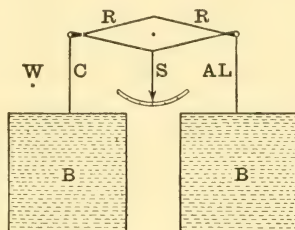


FIG. 7.

RR are the two resistances or the arms of the balance.

S is the receiver or indicator, which shows a difference of voltage or weight.

B is the battery voltage or weights in the pan.

C and AL are cable and artificial line respectively, or the two pans of the balance.

due to this cause must certainly be under $\frac{1}{10}$ of that due to the arrival current.

Taking the figures I have given, we see that the balance must be obtained and maintained so that applying 40 volts to the cable and artificial line, the two currents dividing must not vary more than what will produce $\frac{1}{200}$ volt; that is, must be balanced to an accuracy of 8000 to 1.

If, after the duplex has been established, the artificial line varies in its electrical properties as much as $\frac{1}{8000}$ of its value, the balance would require adjustment so as to keep it useful for receiving. The sensitiveness under these conditions may be considered as equivalent to the

sensitiveness of an ordinary metal balance that with 8 grammes in each pan must turn accurately with one milligramme.

I now illustrate the duplex with a mechanical model.

It is now found necessary to maintain still more perfect balances for our new method of "high-speed working of cables."—in fact, a balance that must be maintained to within the proportion of 72,000 to 1.

To do this the very greatest care has to be directed to questions of insulation and temperature correction, and special appliances are supplied to obtain this high degree of accuracy. In fact, the future of "high-speed working of cables" is locked up very much with this question of more delicate and accurate balances; and if still more perfect balances could be obtained, still higher working speeds of cables would immediately be possible.

I now come to the instruments employed to work the cables, starting with the sending end.

As before pointed out, the various letters of the cable alphabet are composed of combinations of + and - electrical impulses, or of the records that these impulses produce. The letter *e* is a + impulse, *t* a - one, *a* is composed of two impulses, a + and - and so on for all the other letters. The operator has, therefore, first to translate the message to be sent into the cable code, and then to tap on the sending-key the order of the impulses that make up the code message.

A sending-key consists of two levers: the depression by the finger of either one or the other determines which end of the battery, the + or - end, is joined to the cable.

Sending messages by hand is open to two objections: one the want of speed, the other the want of accurate spacing of the letters.

A good trained clerk can send at the rate of about 140 letters per minute; but as most cables are capable of being worked at greater speeds, automatic or machine transmission has now become universal.

An automatic transmitter is an instrument that does the work of the clerk in sending; the two levers of the hand key are now operated upon by mechanism driven by a motor, through the agency of a perforated ribbon.

Everyone who is acquainted with the Pianola or Automatic Piano Player knows that the music to be played is punched as holes in a broad paper strip; this strip is run through the machine and determines which levers are to press upon the keys of the piano.

The operation of the automatic transmitter is precisely like this, only instead of the extended keyboard there are two keys, a + and -, and the paper strip is a narrow ribbon with only two rows of holes to work the levers.

To send a message, the clerk first of all by means of a hand perforator, punches the message as combinations of holes in the paper ribbon; this ribbon, after being perforated, is fed through the automatic transmitter,

The automatic transmitter is a motor-driven instrument, adapted to feed the perforated ribbon over the ends of a pair of blunt needles. These needles are kept perpetually moving up against and away from the moving ribbon, but if there is a hole in the paper, that particular needle over which it is fed will find it, and the needle will move a little way through the hole. Attached to the two needles are contact levers which connect the cable with one or the other pole of the sending battery.

When there are no holes in the paper ribbon, the needles move up against the paper, and the further movement is arrested, and the contact with the battery is not closed, but the battery circuit is closed when there is a hole in the paper, because there is nothing now to block the needle, and the further movement through the hole enables the contact lever to close the battery circuit and thus send the signal.

The sending levers do one or other of two things: they join the cable to earth (in other words, they short-circuit the cable end), or they disconnect the cable from earth and connect it to the battery, so that the battery may send a signal.

At the end of each signal the cable is automatically put to "earth."

Every signalling impulse due to each hole in the paper is, therefore, divided into two parts, the battery or signalling and the earthing portion. These two portions are adjustable relatively to one another; when the best relationship has been found, it is maintained at that adjustment.

The object of earthing the cable after the battery contact is to allow the cable to discharge itself, and thus clear itself for the next signal.

Automatic transmitters constructed on this principle are called "plain" automatics, and are in universal use.

The "curb" was a device applied to an automatic transmitter to sharpen the signalling impulse, and thus gain greater definition and increased speed by reversing the battery at the termination of every battery period. The reverse battery voltage helped to neutralise the charge already in the cable, and thus discharge the cable in quicker time than by simply earthing the cable as in the "plain" automatic.

Unfortunately, the use of the "curb" results in a greater voltage stress on the sending end of the cable, for the reason that the reverse voltage of the "curb" is added to the voltage already in the cable ready to discharge, and the rapid reversal of current resulting upon the application of the "curb" is liable to cause "jar" disturbances on the duplex balance. For these reasons "curb" automatics are not now employed.

Instruments adapted to receive messages at the end of long submarine cables must of necessity work at the highest possible speed that the cable will allow, and are of extreme sensitiveness, and as a consequence are of great delicacy.

There are two kinds of receivers now commonly employed, viz., the siphon recorder and the "drum" cable relay.

The siphon recorder, invented by Lord Kelvin in 1867, is an instrument that inks the message as received on a moving band of paper.

The "drum" cable relay, by means of an electric contact-making device, brings in a fresh source of energy from a local battery, so that the electric signalling impulses are multiplied many times over in power, and are thus enabled to do many useful things besides inking the message, such as working signalling keys to re-transmit the message on to another line, or to guide the levers of an automatic punching machine to perforate the message.

The siphon recorder requires the constant attention of a clerk, the "drum" cable relay does not.

The siphon recorder consists of a bent glass siphon tube nearly as fine as a human hair.

The siphon is suspended by a fine bronze wire; one end of the tube dips in a reservoir of blue aniline ink, the other end can move across the surface of a travelling band of paper, upon which it inks its movement.

If the end of the siphon touched the paper, the friction thus introduced would be fatal to the proper working of the instrument, because of the loss of sensitiveness; it is, therefore, kept in a state of constant vibration by attaching the tube near its end by means of a silk fibre to an electro-magnetic vibrator. The message is thus recorded as a close row of ink dots on the moving paper, and the glass tube is quite free to swing sideways under the action of the received signals.

The siphon tube is joined by two silk fibres to a rectangular suspended coil, of fine insulated copper wire, which coil hangs in a strong magnetic field.

The currents from the cable flow through the wire of the suspended coil, and the re-action of these currents with the magnetic field cause the coil to oscillate, to one side or the other, dependent upon the direction of the current.

The motion of the coil is transmitted by means of the two fibres to the siphon, and thus the signals are recorded as received.

Ever since the invention of the siphon recorder efforts have been made to turn it into a relay, but two difficulties had to be faced. The extreme feebleness of the received signalling currents was such that they were incapable of opening and closing a battery circuit so as to do useful work in that circuit.

The reason for this is that a certain force is required to press the relay contacts together to complete the circuit, and a certain force to break the circuit when formed: these forces of "make" and "break" are too great for the cable relay to supply under normal working conditions.

The second difficulty was the want of definition in the signals

received to operate a relay: they were too ill-defined, and the zero line wandered too greatly to insure that a relay with a fixed mechanical zero would work satisfactorily.

These two difficulties were overcome by the invention of the "drum" cable relay and the magnetic shunt.

The drum cable relay (Fig. 8) is very similar to the siphon recorder. It is the same so far as the suspended coil and connecting fibres are concerned, but, in place of the siphon tube, a relay contact arm is provided.

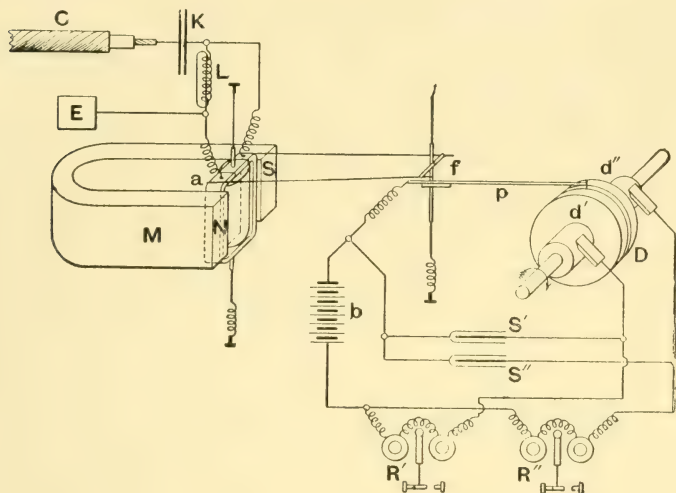


FIG. 8.—DRUM CABLE RELAY.

The end of this arm is arranged to press upon the surface of a revolving drum. The outer drum surface of gold or silver is divided into three parts: a central insulated portion, upon which the end of the contact arm normally rests when no signals are received, and portions one on each side of the central one. These outer divisions are included in the circuit of a local battery, and two post-office pattern relays.

When the relay arm is deflected to one side or the other, upon the receipt of the signal, it slides or skates into contact with one or other of the outer portions of the drum, and thus closes circuit of the battery through one or other of the post-office relays: this second relay is thus operated and in turn works a "sounder" key to re-transmit the signal into a second cable.

To reduce the electrical resistance that is found to exist in the contact between the relay pointer and the revolving drum, and to

allow a large current to pass, condensers are placed across to short-circuit the contact.

These short-circuiting condensers are very important to the proper working of the relay, as without their aid very little current indeed could be obtained in the local circuit to do useful work. The cable relay is a delicate instrument, and mechanical effects had to be produced by means of energy four millionths of that required to produce one candle power of an ordinary carbon lamp.

The operation of the relay throughout is quite automatic and reliable, and no clerk is required to supervise.

The drum relay has two properties that peculiarly fit it for cable work.

1. The relay contact is always made, because the contact arm never leaves the surface of the drum.

2. By the rotation of the drum the friction between the arm, to side motion, and the surface of the drum is reduced in a most wonderful way, so that the arm may be moved by the extremely feeble forces received at the end of the cables.

The relay has a fixed mechanical zero, the centre of the insulated portion, to which the end of the arm must return after every signal or group of signals, and the zero of the electrical signals has been made by electrical adjustment to coincide with the mechanical zero. If there were not this coincidence there would be mutilation of the re-transmitted signals.

The working of the relay is complicated by the requirements of the service, which demand that a condenser should be included in the suspended coil circuit. The object of this condenser is to exclude the possibility of interference from "earth" currents, which sometimes flow along the cable.

The presence of the "earth" current is due to outside electrical influences, atmospheric or celestial.

Now these "earth" currents, if allowed to flow through the suspended coil, would produce deflections that would interfere with the proper working of the relay.

The magnetic shunt which is always placed across the coil does shunt the "earth" current to a very great extent, but does not always get rid of it, and so to make matters sure the "unshunted" series or Varley condenser is included in the system.

The condenser, unfortunately, polarises or charges up under a series of signalling impulses of the same polarity or sign, and for this reason itself causes a wandering of the electrical zero of the signals. We are therefore trying to stop one kind of variable zero effect by a device that produces another one of its own.

The effect of the wandering zero due to the series condenser can be cured, because the wandering, unlike that of the "earth" currents, follows a regular law, viz. the law of the signals themselves.

The relay produces the signals and combination of signals in

its local circuit precisely the same as the signals or combination sent through the cable that work it and are at the same time causing the variable zero.

Current is therefore taken from the local circuit and passed through an electrical retarding device, which is called the "local correction circuit," consisting of a series of inductances and shunting resistances.

The local circuit is so adjusted in its value that the current at the far end rises exactly as there is a drop in the received signalling current through the series condenser.

The correction current is passed through a separate winding on the suspended coil of the relay, and produces an effect on the coil exactly opposite to that produced on the main winding by the variable zero itself, that is to say, two variable zeros of equal strength but of opposite directions are superimposed on the suspended coil, and thus neutralise one another. The variable zero of the signals themselves is thus eliminated.

Local correction is a very important part of the relay adjustment, and cannot very well be dispensed with.

During the last year the Eastern Telegraph Company have most generously lent me their lines for a trial of my "high speed" system of working.

The cable over which the tests have taken place stretches from Porthcurnow in Cornwall to Gibraltar, and is normally worked at 170 letters per minute, each way, with the siphon recorder as receiver.

With the new method, using a special relay (Fig. 9), traffic has been carried continuously, duplex, at 230 letters per minute.

On special trial runs, not carrying traffic, and not sending into the cable at the receiving station, although on duplex conditions, a speed of 280 letters per minute has been obtained.

The principle of operation is as follows: When a submarine cable is forced much beyond its normal speed of working the quick changing signals, such as make up the letter *c*, are the first to fail, or, in other words, do not arrive with sufficient strength to work the receiver.

It was found on trial that allowing more of the current from the cable to flow through the receiver, say by increasing the size of the receiving condenser, the first and last signal of a series of reversals could be obtained with sufficient strength to efficiently work the relay.

The relay once started is arranged to bring in fresh energy from its local battery, through a special retarding circuit, to add to the strength of the quick-changing currents, on its own coil, and thus the reversals are made strong enough to give a record, which without this aid they would have been unable to do.

By these means weak signals are built up at the receiving end of the cable, and the speed of working can thus be materially increased.

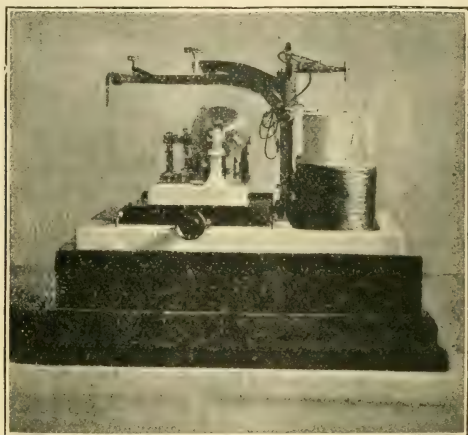


FIG. 9.—HIGH SPEED RELAY (Side view).—The pointer is constructed of quartz fibres kept in tension by a thin copper wire, the whole weight of the pointer being not more than one or two grains.

It is fortunate that the class of signal that has the greatest difficulty in getting through the cable is the easiest to be added to when received. The "high speed" relay works therefore not from the signals received from the cable only, but also from those that it transmits through its own local circuit, the record that it makes being the combined action of the two.*

For most of the calculations in this lecture I am indebted to Professor Perry. This, I am sure, is a sufficient guarantee for their accuracy.

[S.G.B.]

* A scientific man of my acquaintance tells me that I ought to put things in this way. A fluttering current arrives too weak to make a signal, but all it can do is *just to hint* that it wishes to make a signal, the hint is recognised and the local battery makes the signal required.

WEEKLY EVENING MEETING,

Friday, March 19, 1909.

GEORGE MATTHEY, Esq., F.R.S. F.C.S., Manager,
in the Chair.

RICHARD THRELFALL, Esq., M.A. F.R.S. Assoc.Inst.C.E. F.C.S.

Experiments at High Temperatures and Pressures.

WITHIN a few miles of this lecture room there is an unexplored region—to approach it we should have to move vertically downwards. It has been suggested by Mr. Parsons * that it would be worth while to make a short expedition in this direction, but the journey would be slow and the cost high—for instance, to bore a hole 12 miles deep was estimated to be a labour which would occupy eighty-five years and cost 5,000,000*l.* A well-to-do man desiring to benefit his fellow creatures could not do better than undertake this project, but till he comes forward we must perforce be content to try to imitate in our laboratories the temperature and pressure conditions which would be met with deep down in the earth.

Information, attainable from experiments under these conditions, is essential to the development of any exact concept of the structure and evolution of the earth. One of the most important questions in connection with the study of bodies under high pressures and at various temperatures is, as to whether any particular body is solid or liquid under specified conditions, and, if solid, whether it is amorphous, glassy or crystalline. That pressure would influence the melting point of solids was clearly put forward by Clapeyron in 1834, but it was not till after the establishment of the mechanical theory of heat in the “forties” of the last century that the exact numerical relations could be established, as was done by Prof. James Thomson in 1851, when he calculated for the first time the amount by which the temperature of fusion of ice would be reduced by a given increase of pressure. The ideas underlying such calculations are based on a consideration of the way in which heat is converted into mechanical work in any prime mover depending on a heat-supply, and were first formulated by Carnot in 1824, before the true nature of heat was understood. As the matter is fully dealt with in every text-book, I will merely remind you that Prof. James Thomson was able to obtain an equation between the mechanical work actually produced under stated conditions and the work which, according to Carnot’s principle, must be developed by a reversible engine operating between fixed temperature limits upon a given amount of heat.

* B. A. Reports, Cambridge, 1904, 672.

The general relation for a substance undergoing a change of state at absolute temperature T , such change involving a change of volume Δv and an absorption or emission of heat at constant pressure Q_p , is, reserving the question of sign—

$$\frac{dT}{dp} = \frac{\Delta v T}{Q_p}$$

or in words, the change of melting-point produced by unit change of pressure equals the product of the absolute temperature, and the ratio of the change of volume of unit mass on melting to the quantity of heat absorbed or emitted by unit mass in the process.

Now the greater number of substances when they pass from the liquid to the solid state evolve heat and contract in volume. An increase of volume is of course a positive quantity, and if heat is absorbed during this increase it is reckoned positive also. In the case of water, heat is evolved during freezing as in other cases, but the mixture of ice and water has a smaller volume than the solid ice. Accordingly the change of volume in this case is negative, and the melting-point falls as the pressure rises.

The first fairly exact confirmation of the theory appears to be due to De Visser,* who selected acetic acid most carefully purified as a test substance—though valuable experiments up to much higher pressures had been previously made by many others—particularly by Dewar on water,† Ferche on benzol,‡ and Damien§ on a variety of substances.

It is necessary to work with a pure substance in order to test the theory, or at all events with one whose solid phase has the same constitution as its liquid phase. If the acetic acid had not been pure the probability is that the frozen part would have contained more or less of the impurity than the unfrozen, and consequently a state of affairs not contemplated in the theory would have arisen. From the experimental point of view it is obvious that a sharp melting point is a necessary condition for its accurate observation.

A quantity of acetic acid—rather over 40 c.c.—is confined by mercury in a closed apparatus based on a previous design by Bunsen, which also contains air in a graduated tube. When the acetic acid melts it expands and compresses the air through the intermediary of the mercury—whereby the pressure can be inferred. The part of the apparatus containing the acetic acid is immersed in a bath which can be kept at any desired temperature. As the melting progresses a pressure is set up by the expansion, and finally attains such a value that no further melting can take place. We then have a mixture

* *Recueil des Travaux Chimiques des Pays-Bas*, xiii. 1893, 101.

† *Proc. R. S.*, xxx. 1880, 533.

‡ *Wied. Ann.*, xlv. 1891, 265.

§ *C. R.*, cxii. 1891, 785.

of solid and liquid acetic acid in presence of each other under a measured pressure and at a known temperature. The quantities entering into the calculation are ascertained from other experiments—notably the ratio of the change of volume to heat absorbed was ingeniously ascertained by a modification of Bunsen's ice calorimeter. The final result was that the rate of variation of temperature of melting-point with increasing pressure was calculated to be 0.02421°C. per atmosphere as against 0.02435°C. found by experiment—a difference of 0.57 per cent. I have dwelt on this work at some length in the hope that it may make the nature of the problem clear. It is to be noted that the experimental difficulties are considerable, and are enhanced by the fact that we have no *a priori* reason to suppose that the rate of change of melting-point with pressure is a constant quantity independent of the pressure. In fact it was shown by Sir Joseph Thomson about 1886* that in calculating the change of melting-point we ought to take into consideration "the difference between the energy due to strains produced by the pressure in unit mass before and after solidification." Sir Joseph Thomson's reasoning, based as it is on a generalised Lagrangian method of treating problems involving energy changes, is unsuited for discussion in a non-mathematical address, but it is easy to see that if the compressibilities of liquid and solid are different, then the change of volume accompanying the change of state of unit mass must itself depend on the pressure, and therefore the pressure change of melting-point which is proportional to the change of volume must depend on the square of the actual pressure so far as this part of the effect is concerned. This anticipation was realised by Damien in 1891, who showed that the melting-points of substances in terms of the pressure could be expressed by a formula of the kind

$$t = t_0 + a(p - 1) - b(p - 1)^2$$

t_0 being m.p. under 1 atmosphere pressure.

I think we may add that there will also be a small effect depending on changes of energy in the capillary layer separating the phases.

The first adequate investigation of the change of m.p. under pressure over a wide range of pressures was made by Barus.† Time does not permit me to do more than exhibit the results obtained, though the apparatus employed was most cleverly designed. It requires great experimental knowledge and ingenuity to infer with accuracy changes of volume of a few per cent. of the original volume at pressures of 1500 atmospheres, nearly 10 tons per square inch. If we note the pressures and temperatures of melting, and plot the result as a curve against the pressure and temperature, we obtain what is

* Applications of Dynamics to Physics and Chemistry, 259.

† Bulletin No. 96 of the U.S.A. Geological Survey, 1892.

called a melting-point curve, and this divides the field into two parts, so that on one side of the curve the temperature and pressure at each point have such values that the substance is solid, while on the other side their values are such that the substance is liquid. It is instructive, therefore, to regard the melting-point curve as the line separating the region of solid from the region of liquid. Along the line, and along it only, i.e. at the pressures and temperatures indicated by points on the line, the solid and liquid phases can exist in equilibrium together. Such a diagram is called a "Diagram of Condition."

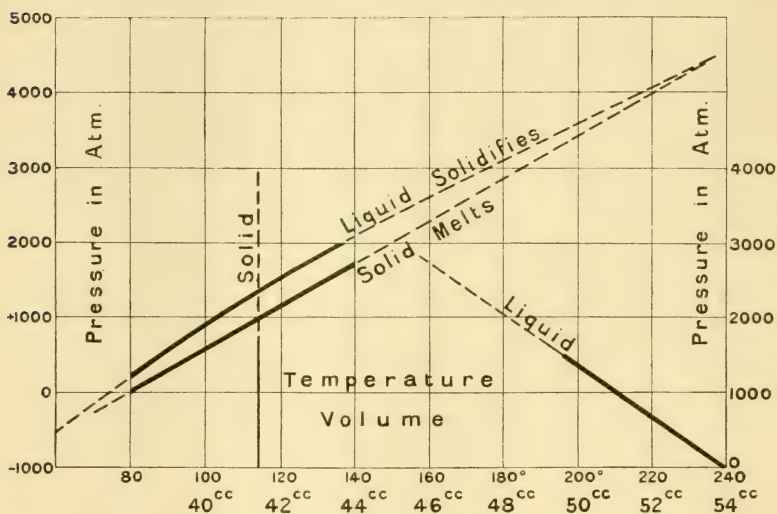


FIG. 1.

Full lines indicate party field actually explored.
Dotted lines indicate extrapolations.

By far the greater part of our information as to the quantitative relations of bodies at high pressures we owe to Prof. Gustave Tammann, who has collected his results in a book entitled "Kristallisieren und Schmelzen," whose advent (1903) must be regarded as an important event in the history of the subject.

The complete thermodynamic specification of a body involves a knowledge of its mass, volume, pressure, temperature, energy, entropy, surface tension, and nature—whether liquid, solid, glassy, crystalline or amorphous.

Prof. Tammann has simultaneously measured the pressure, temperature, volume and mass of many substances under high pressure, and at temperatures extending from 80° to 200° C.—taking cognizance

of the physical state—and has thus been able to plot out many interesting diagrams of condition. The apparatus consists of a screw press by which a piston of ebonite is driven down a steel cylinder of small known cross-section. The cylinder is filled with oil, and the ebonite piston fits practically oil-tight. The oil communicates with the oil contained in a strong steel vessel, which also incloses a glass tube open at the lower end, containing the substance and dipping below the level of mercury contained in a dish. The oil occupies the rest of the space. The steel vessel is placed in a thermostat so that its temperature can be ascertained. The oil pressure is measured by a Bourdon gauge which it was possible to standardise, thanks to the previous work of Amagat and Tait. In order to construct a diagram of condition it is necessary and sufficient to find a number of points separating the liquid from the solid area—or separating the areas corresponding to different crystalline forms in the case where the transformation of one sort of crystal into another is under investigation. To understand how this is done, it is best to take a special case. If we have a quantity of a substance under a known pressure and temperature in the piezometer and suddenly increase the pressure, so that there is not time for heat to pass in or out to any appreciable extent before the pressure gauge can be read, we have practically adiabatic compression. If the apparatus be then left to itself, the heat which we may suppose to be liberated by the pressure will slowly diffuse outwards, and the pressure will fall as time goes on. If we happen to start from a point on the m.p. curve before the pressure is raised—then the final result will be that we shall thaw or freeze more or less of the material, and the original pressure will be exactly regained, the change of state compensating the impressed change of volume. If, however, the increase of pressure has been so great that a change of state of the whole mass has been brought about, then the after variation of pressure will be so much greater that it is easy to distinguish this case from the previous one.

The accompanying diagram (p. 546), taken from Prof. Tammann's book, shows how the equilibrium curve can be located in the case of carbon dioxide and naphthalene. In the former case the temperature was 0.31°C . The pressure was 3800 kilograms per sq. cm., or 24.13 tons per sq. inch. (157.49 kilograms per sq. cm. = 1 ton per sq. inch = 152.38 atmospheres.)

The pressure was raised adiabatically to 4400 kg./cm.^2 (27.93 tons/sq. inch) and the subsequent fall of pressure plotted against a time scale for ten minutes. The pressure was then adiabatically reduced to 3550 kg./cm.^2 and the recovery curve again plotted. The equilibrium pressure must lie between the pressures approached asymptotically on the diagram, i.e. between 3825 and 3792 kg./cm.^2 . A repetition between narrower pressure limits enables the pressure to be fixed at between 3808 and 3797 kg./cm.^2 . A similar procedure fixed the pressure of the m.p. of naphthalene between 3090 and 3080

kg./cm. at the temperature considered—a difference which corresponds to 0.2°C ., the actual temperature possibly differing from the thermostat temperature by 0.1°C .

We may now pass on to the consideration of some of the results obtained, which refer not only to change of melting-points, but to changes in the temperatures of transformation of isomorphous forms.

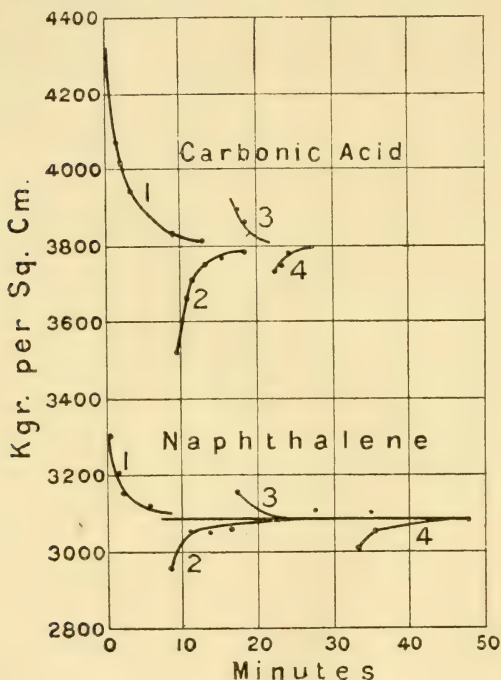


FIG. 2.

As illustrations of such changes, I show here the transformation of yellow to red mercuric iodide, which shows well in the projection microscope; also Mitscherlich's transformation of potassium bichromate, and sulphur in two forms.*

* *Experimental Demonstration of a Transformation of Sulphur.*—A microscope slide is prepared by partially melting a fragment of monoclinic sulphur, and inclosing some of the melt between the slide and cover-slip, well pressed together. The presence of unmelted monoclinic sulphur insures the crystallisation of this variety on lowering the temperature. By means of a hot stage it is possible to preserve the crystallisation long enough to exhibit it by means of a projection polarising microscope. The appearance is very charac-

The case of sulphur is one of great interest. It has long been known that sulphur can exist in at least three solid forms. It crystallises from some solvents in octahedral crystals, from others or from its liquid state in monoclinic crystals. In the latter case some amorphous sulphur is generally dissolved in the crystals, and the amorphous variety itself is formed in tough vitreous masses when molten sulphur heated till it becomes very viscous is poured into cold water. At ordinary temperatures the octahedral form alone is stable. It has been found that at atmospheric pressure octahedral sulphur is converted into monoclinic at 95.4°C. and in the process 2.7 gramme-calories per gramme of sulphur are evolved. The density of octahedral sulphur is about 2.03 and of monoclinic about 1.98 at ordinary temperatures. In accordance with the principles developed previously the transformation temperature of rhombic to monoclinic sulphur must rise with increase of pressure. So far back as 1887 Roozeboom* was able to predict that the diagram of condition for sulphur would be as shown on the next page.

Prof. Tammann has supplied the corroboration of the existence of the triple point.

Suppose that we have sulphur at a pressure of about 1500 kg./sq. cm. (9.52 tons/sq. inch) and raise its temperature to about 160°C. or more, we shall cut the melting-point curve of octahedral sulphur, and the sulphur will melt. If we then allow the sulphur to cool, keeping the pressure up, octahedral sulphur will crystallise from the melt instead of monoclinic sulphur. This very likely has some bearing on the occurrence of native crystals of octahedral sulphur.

It is not every substance which has such sharply defined properties as sulphur, though even these are not as sharp as they might be, owing to the constant presence of amorphous sulphur. An instructive case is afforded by phenol. As the diagram shows, there is a considerable region of the field in which two kinds of crystals of different density can exist together, the curves forming the boundary of this region of pseudo-equilibrium.

It may be that the two crystalline forms of carbon which ap-

teristic. Another slide is prepared, but this time all the sulphur is melted, and can generally be undercooled so far that it crystallises in what is believed to be the octahedral system. This slide is then placed in the projection microscope, when it is seen that its appearance is totally different from that of the first slide. The preparation is then heated on the hot stage, and when the transformation temperature is reached it is seen that the structure begins to change—the crystallisation breaks up and becomes granular, the granules showing in general much more colour than the original crystallisation. These granules are taken to be monoclinic sulphur. The temperature is now raised till about half the preparation has melted, and it is then allowed to cool back a little so as to crystallise. The crystals now show the characteristic monoclinic crystallisation with brilliant colours, since unmelted monoclinic sulphur is present.

* *Rec. Trav. Chim. Pays-Bas*, vi, 1887, 314.

parently can exist together indefinitely at ordinary temperatures and pressures are an illustration of the same property.

As a final illustration we may note the results for water down to -80°C. , from which it appears that it possesses three allotropic crystalline forms with at least two melting-points.

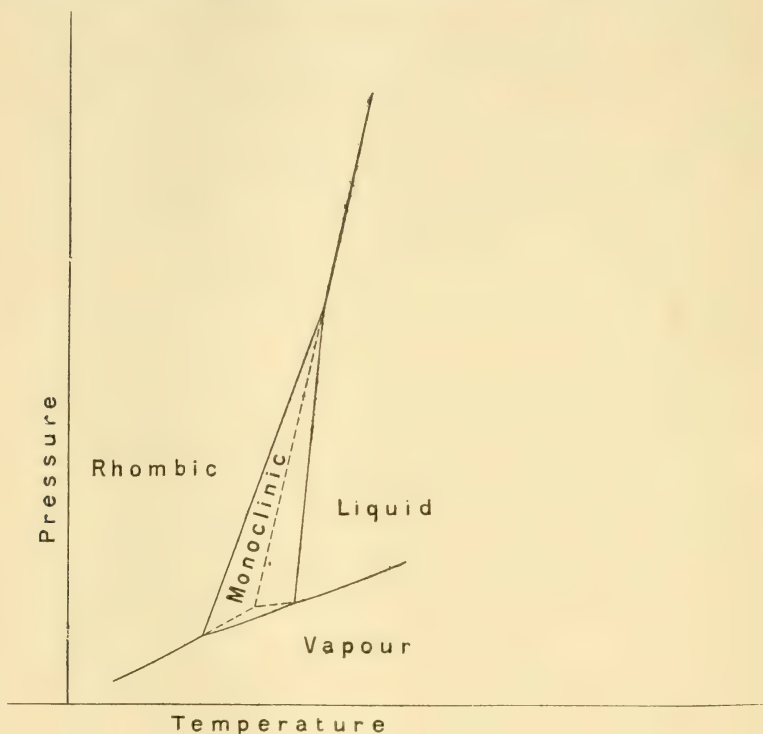


FIG. 3

The melting-curves of from thirty to forty substances have been investigated, mainly by Tammann, up to about 3000 kg/sq.cm. = 19.05 tons/sq. inch, and the general result has been to show that there is a tendency for the rate of change of melting temperature with pressure to fall off as the temperature rises, and also that many substances, which at ordinary pressures crystallise in one form only, can be caused to assume allotropic modifications under high pressure. This tendency to form allotropic modifications appears to be associated with the extent to which a substance can be under-cooled without crystallising.

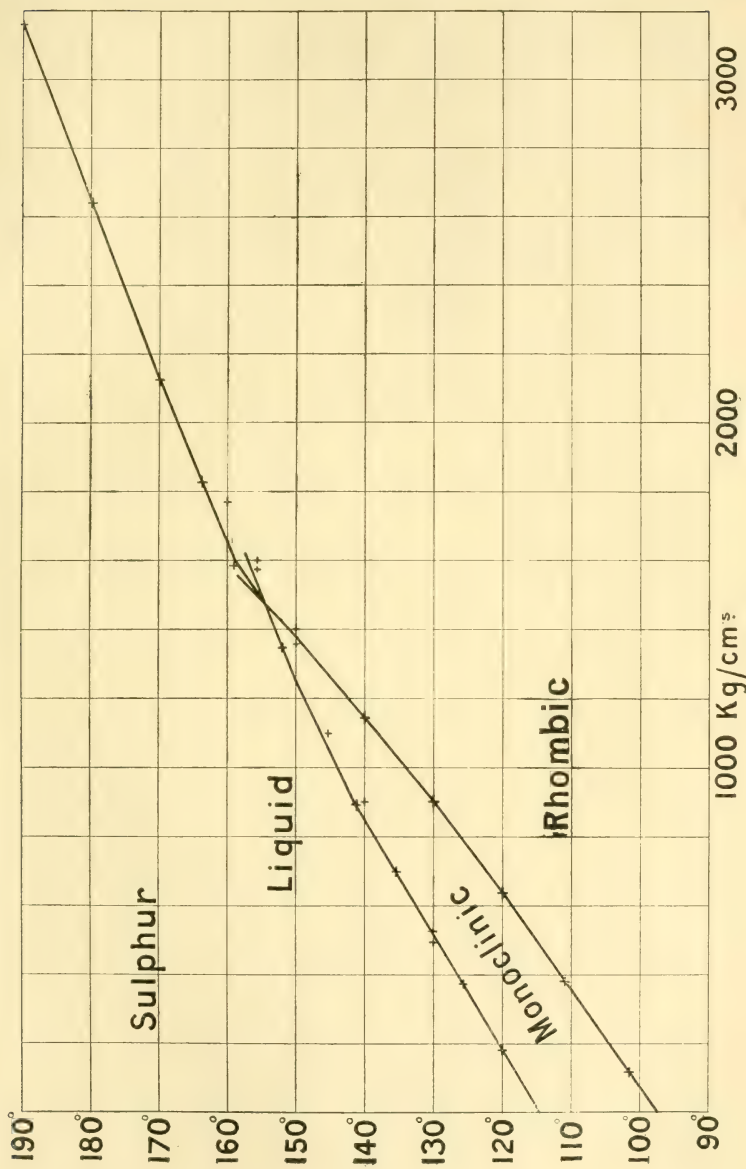


FIG. 4.

A question of the greatest interest and importance may now be formulated—what will happen if we go on increasing the pressure?

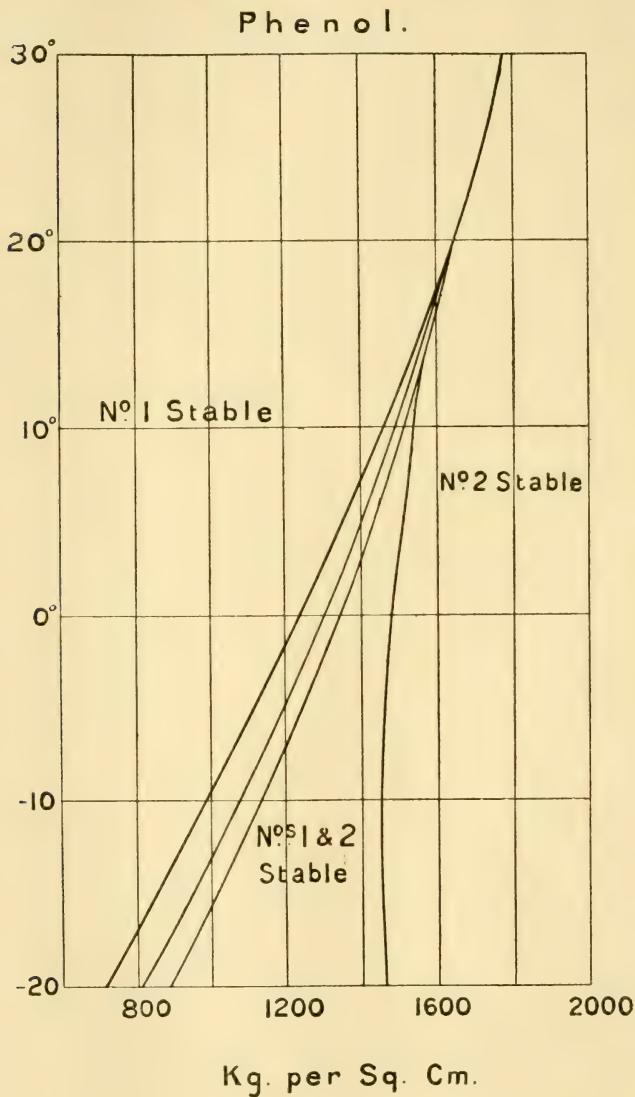


FIG. 5,

Will a state of affairs be reached in which it is no longer possible to distinguish between the liquid and its crystalline form? Will there be, in fact, a sort of critical point at which the melting-curve will end? At present we can only say that no indications of such an occurrence have been observed experimentally, and Prof. Tammann takes the point that it is highly improbable that anything in the nature of continuous transformation can take place, because a crystal has different properties in different directions related to its axes, and there is thus a much greater qualitative difference between crystals and liquids than between liquids and gases, both of which are isotropic. I must admit that this argument does not appeal to me very strongly. If it be possible to compress a substance till it reaches a state in which, at one and the same temperature, the liquid has the same density as the crystals, presumably the mean distance of the molecules will be the same in both cases. I see nothing monstrous in the view that under these circumstances crystallisation may set in gradually, and that it may not be possible to say exactly when the liquid ceases to be a fluid and becomes a crystalline solid. There are no theoretical or other grounds for supposing that the phenomena of crystal growth as observed when there is a change of volume accompanying the crystal formation, will necessarily hold when no such change of volume occurs.

If we refer to the theory of the change of m.p. by pressure it is obvious that if either the change of volume or the latent heat of melting vanish at any temperature or pressure on the melting-curve, then in the neighbourhood of this pressure the curve must degenerate to a point—or small pressure changes will not affect the m.p. It was pointed out, however, that there is a term or terms depending on the square of the pressure, and if these were relatively important the only thing we should notice would be a change of curvature at the point under consideration. It does not follow that there is no maximum or minimum to the melting temperature of any particular substance because the term in P^2 may be vanishingly small: it may be (and generally is) of opposite sign to the term in P , and in this case it is only a question of the relative importance of the terms where the maximum or minimum melting-point lies. Damien's empirical formula expresses precisely the effect to which I refer. The practical result which is of importance in questions affecting the condition of the inner layers of the earth is that we are not entitled—in fact, it is wrong—to suppose that pressure must necessarily go on raising the melting-point indefinitely; everything depends on the substance under consideration. It is therefore necessary to make such experiments as those of Tammann at vastly higher temperatures and pressures than those we have been considering, up to probably over 10,000 kilograms per sq. cm. (or 63·5 tons per sq. inch).

In 1893 some experiments were described by Parsons* in which

* Phil. Mag. xxxvi. 304.

carbon rods were heated by electricity under a pressure usually of 15 tons per sq. inch—but rising in one case to 30 tons per sq. inch. The pressure was obtained by means of a hydraulic press, but no detail is given.

I have been desirous for many years of making some experiments at high temperatures and pressures, but for a long time could think of no way of ascertaining the pressure at temperatures over a red heat except by the use of compressed gases. In 1902 Sir Andrew Noble was kind enough to have some drawings prepared for a wire-wound steel pressure vessel to carry a pressure of 50 tons per sq. inch. The pressure was to be supplied by a compressed gas, and some details of the heating arrangements were designed, when a calculation of the cost of the gas compressors, vessel and appurtenances made it clear that the undertaking would be beyond my means. I then endeavoured to find a simpler form of apparatus, and finally was led to contemplate the substitution of graphite for compressed gas, Spring having pointed out that crystalline graphite flows very easily at high pressures. A simple trial made it clear that the graphite of Ceylon does in fact possess the property of flowing like a liquid under high pressure to a sufficient degree to allow of pressure being transmitted by it. Graphite can be used with some reservations to transmit a pressure just like water or oil, though it is, of course, inferior in fluidity, and as I have now discovered occasions a loss of "head" which is not independent of the pressure itself. My former statement in the 'Chemical Society's Journal,' 1908, is erroneous, though the results of the experiments are, I believe, hardly or not at all affected by the mistake for a reason which will be clear later on. After several trials, the apparatus which I have here to-night was evolved, and some experiments were made with it. These experiments are not of any great importance, and, indeed, I feel almost ashamed of bringing them to your notice—I can only say in excuse that everything must have a beginning.

I believe, however, that the apparatus is sufficiently simple, cheap, and effective, to enable others with more leisure at their disposal to make a beginning of an investigation of the properties of matter up to 100 tons per sq. inch, and at temperatures up to about 2000° C. At present, however, it is not possible to infer with accuracy the volume of the substance under these extreme conditions, nor can its physical condition be more than approximately and indirectly inferred—we must content ourselves with the production of transformations which we can make persist down to ordinary temperatures and pressures.

If we refer again to the sulphur diagram, we shall see how this possibility may arise. If sulphur is melted and cooled slowly monoclinic crystals are found—when the temperature sinks below 98° C. these crystals undergo spontaneous transformation to the rhombic form—but all that we see is that the monoclinic crystals become

opaque: the external form of the crystals is still monoclinic, but they are merely pseudomorphs of the original crystals. To obtain large octahedral crystals we may suppose that we begin by melting sulphur and raising the temperature and pressure till the former stands at 160°C. or over, and the latter at not less than 1600 kg./cm.^2 ($10\cdot16$ tons/sq. inch).

If we then slightly reduce the temperature or raise the pressure, we shall have the crystallisation of the sulphur in the rhombic form. By maintaining the pressure as the mass cools and when it is cold releasing the pressure, we should finally extract rhombic crystals. To this we may of course add that we need not expect crystals of any size unless we cool at the proper rate. It appears that there are at least two phenomena requiring attention in relation to the production of crystals—one is the relation between the amount of undercooling necessary to induce spontaneous crystallisation, and the other is the rate at which the crystals will grow when they have once started. If we want large crystals, we must not have an excessive number of points of spontaneous crystallisation; nor must we have too high a rate of crystal growth, or the crystals will by all experience tend to be felted together. The temperature condition giving birth to the most favourable number of spontaneous centres is not necessarily the temperature at which crystals grow to the largest size, so there is really no escape from finding by direct trial the most effective way to go to work.

Another possibility is brought to light by an examination of a case of pseudo-equilibrium such as that of phenol. Here we have three regions—in one No. 1 alone is stable, in another No. 2, and in the third both Nos. 1 and 2 are stable. The case of iodide of silver is similar, but more complicated. If in the area C we change the pressure, the temperature remaining constant and the material consisting of a mixture of the two stable phases, we can alter the proportions in which these phases exist, but we cannot cause either of them to disappear.

A notable case of this kind is that of graphite and diamond, both perfectly stable in presence of each other at atmospheric pressure up to a temperature nearly that of the electric arc, say about 3000°C. If there be any similarity between the carbon and phenol diagrams, diamond would correspond to variety No. 2 of phenol, and graphite to variety No. 1, heat being evolved in both cases when the less dense modification changes into the denser. If we desire to obtain phenol 2 from phenol 1, we note that down to a temperature of -20°C. , we should require to keep the pressure always above about 600 kg./cm.^2 ; otherwise the operations would be similar to those described in the case of rhombic sulphur.

Similarly, to convert graphite to diamond on this analogy we should have to raise the temperature and pressure together to some unknown values and then let the product cool—keeping up the pressure meanwhile.

The apparatus which I have used in making the experiment is based on the transmission of pressure by crystalline graphite or the softer metals. In order to ascertain how much pressure is lost during transmission I have arranged an apparatus in which the material to be tested is exposed to a known pressure, tending to force it through a cylindrical space, identical in figure with the space in which the heating is intended to be carried out. The pressure transmitted is transferred by a simple device to a piston with a hard steel point, and this is forced by the pressure to penetrate a soft steel plate. In a subsequent experiment the same piston is forced by a known pressure into the same steel plate, so as to penetrate to the same depth as in the main experiment. It is then possible to compare the pressure transmitted with the pressure applied.

Experiments of this kind have been made with lead and with graphite as pressure transmitting substances.

So far as I know, there is no substance other than graphite combining the property of a certain amount of fluidity with the capacity to resist high temperatures; and our hope of studying chemistry at really high pressures and temperatures appears at present to depend largely upon it. It is true that some attempts have been made to use compressed gases, but the apparatus is vastly more complicated, and the experiments themselves become really dangerous in view of the immense potential energy possessed by gases at pressures of 100 tons per square inch. As illustrating this I may mention that 100 tons per square inch is about the highest instantaneous pressure noted by Sir Andrew Noble in his well-known experiments on the exploding of cordite in closed vessels. The density of nitrogen at 100 tons per square inch is, taking Boyle's law as a very rough approximation, 15,240 times its density under standard conditions. This works out to rather over 19—i.e. about the same as gold, and the energy stored is of the same order as that contained in an equal volume of cordite, though its availability is lower.

The construction of the apparatus I have used can be easily followed from the drawings. It consists essentially of a steel cylinder divided perpendicular to the longitudinal axis by a thin plate of mica: the two halves being clamped tightly together by an insulated ring and clamps at top and bottom. Pressure can be applied by an ordinary hydraulic lifting jack—the one I have used will lift fifty or sixty tons—the bore of the hydraulic cylinder being about $4\frac{1}{2}$ inches. In order to operate at a high temperature it is necessary to line the cylinder with some refractory substance, and I have generally used magnesia for this purpose, though zirconia or thoria might be better. Purified magnesia is first melted in an electric furnace and then ground in an iron mortar till it is very fine. The powder is freed from iron as well as possible by a strong magnet, and after being sifted is pressed into the cylinder little by little by hydraulic pressure so as to form a solid

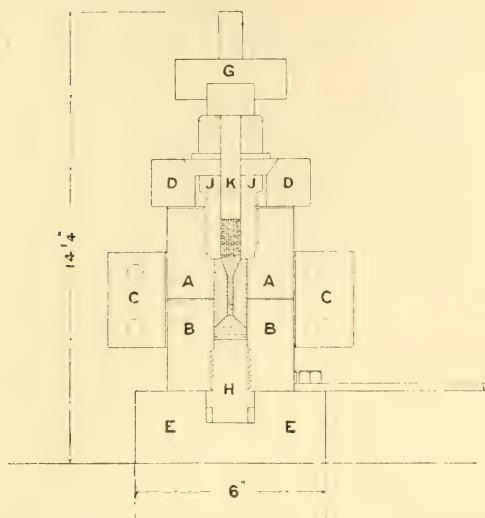


FIG. 6.

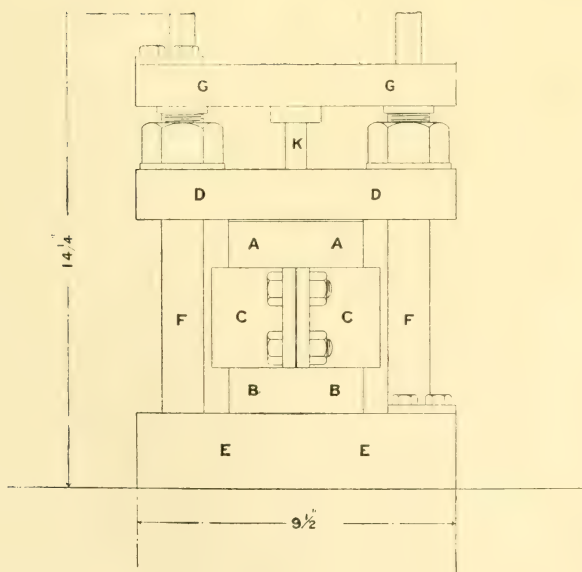


FIG. 7.

plug. This is then bored out with a hard steel drill to the required diameter. In pressing magnesia I have found that it is not possible to thoroughly consolidate the powder in greater thickness than a few millimetres, even under a pressure of 50 tons per square inch. In fact magnesia is a substance which appears to be almost devoid of the fluid properties so marked in graphite—an essential condition for its use in the apparatus. I have tried various other linings, ground flint, alumina, etc., but they have no advantage over magnesia, and are even more difficult to drill out. Alumina prepared from the crystalline hydroxide is very easily compressed into cakes, and makes a good lining, but it is too fusible for experiments on carbon, and is

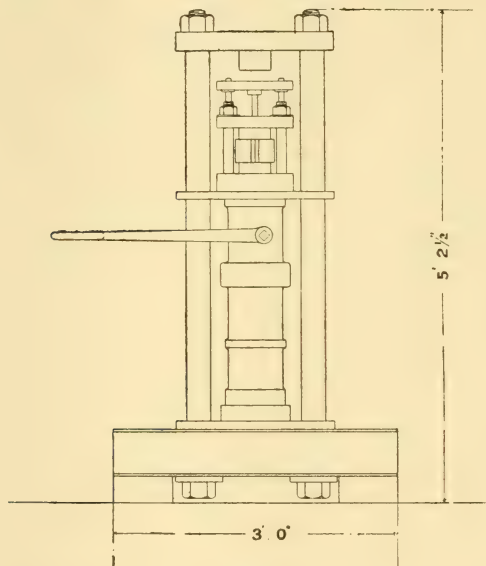


FIG. 8.

[J]

probably more easily reduced. The cylinder having been lined, the bottom is filled in with Acheson graphite in electrical communication with the base of the apparatus. The substance to be operated upon is placed in the narrow part of the bore, and packed in with graphite or lead if that is suitable. The pressure is applied by a ram of hardened high speed steel, working upon a reservoir of graphite or lead contained in the plug closing the cylinder at the top and electrically connected to the other terminal of the supply. The chief uncertainty in regard to the pressure which actually reaches the subject of the experiment lies in the possibility of the ram being

held to some extent by friction against the sides of the cylindrical hole in which it works, and in the consolidation of the graphite—with reduced fluidity, before it actually flows. One has to trust either to the hardness of the ram or to leave a space round it sufficient to allow graphite to escape, when the apparatus follows the lines of Amagat's standard pressure gauge, but the duration of the experiment is curtailed by the exhaustion of the graphite supply. A connection has to be applied for the pressure absorbed by the lead or graphite in accordance with the results of the preliminary trial. It is fair to say that no tendency of the ram to stick has ever been noticed—on the contrary, changes of volume brought about by heating have made themselves evident at once on the pressure-gauge of the hydraulic press.

When working with any form of carbon there has been no trouble in arranging to heat the body which is being compressed by electrical means. It has been found most convenient to adjust the current to about the value required by means of a resistance—large compared with that of the pressure vessel—the latter being short-circuited meanwhile. In making an experiment, the hydraulic press is worked till the desired pressure is attained, and then by opening the switch the current is thrown on to the apparatus. When the magnesia lining begins to melt, the pressure, as shown by the pump gauge, is seen to fall, graphite flows into the magnesia tube, and the pump is worked so as to compensate for this. Under these conditions the pressure is probably transmitted without appreciable loss, as the narrow part of the cylinder is now in a fluid bath. After a sufficient time has been allowed the switch is closed and the pressure kept up by pumping till the apparatus is cold. Originally an apparatus with a cylinder made in one piece was employed, and in this case there was a considerable voltage between the graphite entering the apparatus and the steel walls of the pressure vessel. After a few seconds of intense heating it frequently happened that an explosion took place, due (as could be seen by subsequent examination) to filaments of graphite being driven through the magnesia and producing short circuits against the steel vessel. With the construction above described these explosions do not occur, and there is the additional and very real advantage that when an experiment is over the apparatus can be opened in the middle and everything exposed to view.

A large number of experiments were made on different kinds of carbon and graphite. The weight of material in the highly heated part was generally from 1 to 2 grammes, and the energy supply was at a rate of 5 to 10 kilowatts for from three to six seconds. The pressure in a successful experiment lay at from 50 to 100 tons per square inch throughout. The magnesia lining was usually melted for a distance up to 1 centimetre round the graphite. Now magnesia melts at ordinary pressures at about 2000°C. , but the energy supply

is sufficient to render it possible that temperatures of from 3000° to 4000° C. may have been reached; it is possible that about 3000° C. was actually attained at the centre of the charge. The results obtained were uniform. No matter what form of carbon (excluding diamond, which was not tried) was packed originally in the apparatus the final product was soft well-crystallised graphite; which agrees with some results of similar experiments described by Mr. Parsons,* but not with the results claimed by Dr. Ludwig.†

In several experiments the crystalline mass of graphite was tested in regard to its porosity, and this was found to be considerable—a remarkable result, having in view the conditions under which it had been formed.

Another point of interest was that where the soft graphite had been driven into the Acheson graphite plug at the bottom of the apparatus it became extremely hard, so much so that a hard steel file made little or no impression upon it.

The main difference in treatment of this part of the graphite as compared with the remainder is that it was cooled much more quickly, thanks to the high heat conductivity of the Acheson graphite plug. The cause of hardening has hitherto not met with any satisfactory explanation.

No appreciable quantity of carbide of magnesia was formed in the experiments. The magnesia close to the graphite core contained traces of carbides, but as there were always traces of iron left from the drilling out process, this may be plausibly accounted for by the formation of carbide of iron.

The graphite was finally systematically searched for microscopic diamonds by Staudenmaier's modification of Brodie's method of conversion of graphite into graphitic acid,‡ or else by Moissan's modification of the same method.§ A convenient means of distinguishing diamond in fine powder from most or all of the substances which are not separated by a liquid of density 3.34 at 4° C. is to heat the powder in a silver spoon to a dull red heat in fused potassium hydroxide. Check experiments showed that diamond dust easily passing a sieve with 100 threads to the inch would withstand the action of molten caustic potash at a temperature at which the edges of the silver spoon began to melt, for five or ten minutes. Crystals of alumina or of carborundum are entirely destroyed by this fusion, but the diamond particles seemed to have undergone no change. In fact the individual fragments could be recognised under the microscope after passing through the ordeal.

I am led to consider that my experiments indicate that no wholesale transformation of amorphous carbon or graphite into diamond

* Proc. R.S., 79.

† Ber. 1898, xxxi. 1485.

‡ Zeits. für Electrochemie, 1902, 273.

§ Electric Furnace, 49, translation.

can be brought about by temperatures of the order of $2000^{\circ}\text{C}.$, and pressures of more than 50 and less than 100 tons per square inch. There is some uncertainty, as already mentioned, in regard to the actual pressures operative during the trials. Prof. Tammann has, however, obligingly drawn my attention to the fact that the equilibrium curve graphite-diamond may nevertheless have been crossed, but that no diamond was formed because time for crystallisation was not allowed under the conditions of the experiment. I confess my idea in making the trials was that the amorphous carbon or graphite might be forced to melt, and then that the conditions would require it to recrystallise as diamond—not, of course, in the form of large clear crystals, but rather in the style of bort or black diamond. It is, however, true that the pressures used may have prevented any melting at all, and that it may have been a question of recrystallisation of graphite, in which case the addition *ab initio* of diamond crystals would, as suggested by Prof. Tammann, have been of advantage in promoting crystallisation in that form.

The experiments described have only been rendered possible by the invention of high speed steel, which keeps its hardness up to nearly or quite a red heat, and any further advance—mainly in the direction of the allowance of more time—must wait for improvements in that material. It may very well be, however, that the limits of temperature within which crystallisation in diamond form can take place, are really very narrow at any pressure—and in this case it will be a matter of very great difficulty to make an apparatus in which the conditions could be kept constant for a sufficient length of time—and the difficulty would be greater the higher the temperature.

It is noteworthy from this point of view that in Moissan's artificial production of diamond very much lower pressures and temperatures were used than those just described. I have shown* that, using iron as a solvent, it is highly improbable that Moissan attained a pressure of more than 20 tons/sq. inch, and when silver was employed the pressure would be much lower. A similar criticism places the effective temperature of formation of diamond in iron or silver spheroids at something of the order of $1500^{\circ}\text{C}.$ Comparing the experiments of Moissan with those described above, it looks as if Roozeboom's opinion is at present the most probable—viz. that solvents are necessary in order to depress the crystallisation point of diamond to a temperature at which the transformation to graphite is slow enough for rapid cooling to interrupt it. In this case the next step would be to repeat the experiments I have described at the highest possible pressure in the presence of iron, though Mr. Parsons† has already made some trials in this direction with negative results. We have, however, many

* Journ. Chem. Soc., xciii. 1908, 1351.

† Loc. cit.

metals which have never been tried in this connection, and one or other of them may turn out to have the requisite properties.

I desire to acknowledge the assistance I have received in making the experiments described from my assistant Mr. C. H. Beasley, and in examining the products from Mr. T. H. Waller.

[R. T.]

WEEKLY EVENING MEETING,

Friday, March 26, 1909.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L.
D.Sc. F.R.S., President, in the Chair.

ARTHUR STANLEY EDDINGTON, Esq., M.A. M.Sc. F.R.A.S.,
Chief Assistant in the Royal Observatory, Greenwich.

Some Recent Results of Astronomical Research.

THE object of this discourse is to give some account of two or three of the principal points of interest that have come before astronomers during the past year. It has seemed preferable to concentrate attention on a few points rather than to make any survey of the general progress of astronomy, and I have naturally selected that work with which the Royal Observatory, Greenwich, was most concerned, and which can best be illustrated with slides. That must be the excuse for neglecting many important results which have been obtained along other branches of the subject.

THE EIGHTH SATELLITE OF JUPITER.

The first result of which I have to speak is the discovery of an eighth satellite of Jupiter. This body was discovered by Mr. Melotte at the Royal Observatory, Greenwich, on February 28 last year. [A slide was shown of part of the photograph on which the new satellite was found.] If there are any here who are quite unfamiliar with celestial photographs, I may explain that when a photograph is being taken of any object such as a planet or satellite which is moving relatively to the stars, it is necessary to keep the telescope pointing continually on the object making proper allowance for its motion, the stars being, as it were, left to take care of themselves. The result is that the stars make little streaks on the plate. You see these scattered about everywhere. Now, in this case the plate was made to follow the motion of the planet Jupiter during an exposure which lasted an hour and twenty minutes, and among these streaks or trails were found three round images, which evidently belonged to objects moving at nearly the same rate as Jupiter, and thus distinguished themselves from the fixed stars. It may be mentioned, however, that they do not distinguish themselves from photographic defects so readily. Two of the three images were of the objects for which the photograph was taken, namely, the sixth and seventh satellites of Jupiter discovered by Perrine at the Lick Observatory, California, in

the winter of 1904-5. Since then it has been our practice at Greenwich to photograph them at frequent intervals, for the study of their motions presents many points of astronomical interest; the seventh satellite in particular is very faint and requires powerful instruments, long exposures, and, may we add, a favourable climate for its observation. The third round speck was something new, which could not have been expected; it was an eighth satellite of Jupiter.

But it was a long while before its real nature could be determined definitely, though it was suspected from the very first. Just previous to its discovery, a number of plates of the sixth and seventh satellites had been taken, and by looking back at these, Melotte was able to trace the new object on seven of them. Thus there could be no doubt that it was a real body and not a defect of the plate. The fact that it moved along at almost the same rate with Jupiter was suspicious, but still it was by no means certain that it belonged to Jupiter; it might only be a new comet, or, still worse—a new minor planet, one of the “small vermin of the heavens.” One thing, however, was quickly established: Mr. Crommelin calculated that, whether it belonged to Jupiter or not, it was at all events near Jupiter, and not merely in the same line of sight. That in itself was sufficient to make it an interesting body. I cannot give you any idea of the calculations which ultimately showed that it belonged permanently to Jupiter: perhaps the next slide will show that the hesitancy in coming to a definite decision as to its real nature was not unjustified; it may even seem somewhat daring to have acknowledged it as probable. [The slide showed the observations of the three outer satellites made at Greenwich during the opposition of 1908.] There is no difficulty in seeing VI and VII belong to Jupiter, they are clearly circulating round it; but the eighth satellite looks, at first sight, to be going on its way regardless of Jupiter. Its path is apparently convex to the planet instead of concave to it as would be expected. It is not easy to see how it will get round it at all. That difficulty, however, disappears when the change of position of the Earth and Jupiter, altering the point of view from day to day, is taken into account. The record of the diagram breaks off in April: after that Jupiter became unfavourably placed for observation, and the satellite could only be followed by calculation through its course during the summer and autumn. At the first opportunity, however, it was searched for, and in January it was found again at Greenwich, very near to the predicted spot. [One of this year’s photographs was thrown on the screen.] It has now moved far away from Jupiter, which no longer comes on to the plate; but it is still and will be under the control of the giant planet, and we can say as confidently as in the case of the other satellites, that it is really circling round Jupiter.

The satellites of Jupiter now form a very remarkable family, and before we go on to say more about the new satellite it seems worth

while to consider this system a little in order to appreciate the importance of the latest addition. [A diagram of the system was shown.] Of the first four satellites not much need be said: their discovery by Galileo in 1610 was one of the first-fruits of the invention of the telescope. It was to the study of their eclipses that we owe one of the most epoch-making of all scientific discoveries, the discovery that light moves with a finite speed and not instantaneously: and lest it should be supposed that they are now only of historic importance, I may add that it has recently been suggested that it may be possible ultimately to determine by their aid that extraordinarily elusive quantity the motion of the aether relative to the sun.* In actual size these four satellites are quite considerable, being, in fact, larger than our moon. For 380 years these were all that were known, and the remaining four that have been found in quite recent times are very faint and minute objects. One hesitates to give an estimate of their actual sizes, but, at a rough guess, probably Satellites VII and VIII are globes about twenty-five to thirty miles in diameter. No. V was discovered by Barnard in 1892, and is remarkable for its closeness to the planet. It takes barely twelve hours to go round, and as Jupiter rotates once in about ten hours the satellite nearly keeps up with the planet. VI and VII are a curious pair. The order and regularity which seemed to characterise Jupiter's system is now quite disobeyed. There is a great gap separating them from the inner satellites. Their orbits are very nearly equal in size, in fact, No. VI takes about 250 days to revolve, and No. VII only seven days longer. The orbits are both inclined at 30° to the plane of the ecliptic, but in different directions, so that they do not intersect, and there is no fear of a collision. The orbits are actually interlocked.

The natural place where one might expect to find new satellites of Jupiter, and where, perhaps, new satellites may yet be discovered, would be in the great gap which intervenes between the inner satellites and these two; but the eighth satellite was actually far outside them all. Its orbit also is inclined 30° to the ecliptic. No. VIII is altogether a record-breaking satellite. When at the farthest point of its orbit it is 21,000,000 miles from Jupiter, a distance suggesting rather the interval between one planet and another than between an ordinary satellite and its primary. Venus sometimes approaches the Earth within a distance not much greater than this, and the minor planet Eros occasionally comes very much nearer to us. The satellite will take more than two years to go round the orbit. †

* It would seem, however, to be impossible to separate this quantity mathematically from the eccentricities of the orbits of the satellites, so that it is unlikely that a determination can be made in this way.

† A recent paper by Cowell, Crommelin, and Davidson shows that the orbit is so far from being a closed curve that it is almost meaningless to speak of a definite "period" of the satellite.

Jupiter's year, or time it takes to go round the sun, is equal to nearly twelve of ours. The satellite's "month," or time it takes to go round Jupiter, is somewhat over two of our years. It follows that the satellite goes rather more than five times round Jupiter whilst Jupiter goes once round the sun. We may say that for J VIII there are five months in the year, borrowing terminology of the Earth-Moon system. Now this is a record. All the other satellites in the solar system have a much greater number of "months" to the "year," usually several hundreds, sometimes even thousands. Our own moon previously held the record with between 12 and 13 months to the year, but the new satellite far outdoes that. Now this is a particularly interesting piece of record breaking, because the number of satellite's months in the planet's year expresses, generally speaking, the proportionate share which the planet and sun have in controlling the motion. When, as is usually the case, the number is several hundreds, that means that the share of the planet is far greater than that of the sun, and the orbital motion is easy to compute; in the case of our Moon, which, as I have said previously, held the record, the disturbance by the sun is considerable, and the prediction of the motion is a very complicated problem; but, when, as in the case of J VIII, the number gets nearly down to five, ordinary methods of calculation fail altogether, and, when Mr. Cowell attacked the problem, he had to devise an entirely new procedure for his purpose.

These four newer satellites of Jupiter thus form a remarkably varied group, each individual presenting its own particular problems. It is hard to say which could least be spared, whether the fifth satellite circling so closely and so rapidly; or VI and VII, the twins, with their curiously interlocked orbits; or the eighth, which moves at a distance from the planet so great that at first it could hardly be credited.

There is one circumstance, however, which renders J VIII the most immediately interesting of all—it goes round Jupiter backwards. All the other seven go round in one direction; Jupiter himself rotates in the same direction; but the eighth satellite moves round the opposite way. This is a most significant fact; if we are right in our interpretation, it is a clue which reveals a curious chapter in the past history of the solar system. The speculation which it opens out is not a new one, but this fact gives a strong confirmation to what must previously have been regarded as a plausible but daring hypothesis. In 1899 Phœbe, the ninth satellite of Saturn, was discovered, and after considerably puzzling its discoverer, Professor W. H. Pickering, by its unaccountable motion, was at last found by him to have a retrograde motion: that is, to go round Saturn in the opposite direction to his other satellites, just as we now find J VIII behaves. Phœbe is a long way the outermost of Saturn's satellites, just as J VIII is the outermost of Jupiter's.

Pickering, of course, had only the case of Phœbe to account for, but he suggested an explanation which is known as the theory of planetary inversion, and which now receives strong confirmation from the discovery of an outer retrograde satellite attending Jupiter.

On this theory it is supposed that each planet, when it separated from the original nebula, or whatever may have been the material from which the solar system was evolved, had originally a retrograde rotation, i.e. a rotation from east to west, or in the opposite direction to the motion round the sun. The motions of Phœbe, and we may now add that of J VIII, are relics of this original retrograde motion, Jupiter and Saturn having now turned right over so as to spin in the forward direction, and all their inner satellites having turned over along with the planets, or else only having detached themselves after the parent-planets had turned over. Similarly the Earth and Mars must have turned over, for they are now rotating forwards. In the case of the two outermost planets of our system, Uranus and Neptune, although we do not know their direction of rotation, the motions of their satellites lend some support to the idea of an original retrograde direction. Still, until the discovery of J VIII, the evidence for the hypothesis rested very largely on Phœbe alone.

How, then, has it come about that the traces of this original rotation, believed once to have been universal, are so few? The cause suggested is tidal influence. Ever since Sir George Darwin published his classical researches on the Tides, which showed what a remarkable influence tidal effects have exerted in the formation especially of the Earth-Moon system, their importance in all evolutionary processes has become more and more recognised. It is possible that there may be a tendency to press the hypothesis too much nowadays; the theory is to this extent speculative, that we have very little direct evidence as to the magnitude of the tidal forces, or how many million years are required to produce the effects that are assigned to them. But the effects, though certainly small, are steadily cumulative, so that, granted sufficient time, and there seems no reason why time should be denied, these theoretical results must be brought about.

The results of the action of tidal forces are too complicated for us to do more than touch generally on. The tides raised by the sun and each satellite have each to be considered, and then the attractions of the sun and satellites on their own and each other's tides. The viscosity of the planet is also an important factor. So many particular cases arise that it seems necessary to consider every planet and satellite separately. The mathematics have been worked out in considerable detail by Mr. Stratton, who concludes that tidal action appears to be adequate to explain the characteristics of the various systems of satellites attending on the planets. The mode in which tidal action reverses the direction of the rotation is by tilting over the axis. If you turn a planet over so that what was formerly the north end becomes the south end, this is equivalent to reversing the direction

of rotation ; the transition from direct to retrograde motion is thus a perfectly gradual one ; the satellites of Uranus, in fact, illustrate the intermediate stage ; they may be said to move neither backwards nor forwards but sideways. Briefly speaking, the effect of the sun, and the tides on the planet raised by the sun, would be to tilt over the planet until its axis was in the plane of its orbit—the intermediate position I have just mentioned.

In the case of a very viscous planet the tilting may proceed still farther, until, in some cases, the planet goes almost completely over ; that is what seems to have happened in the case of Mars and the Earth. The Moon appears to have split off from the Earth after this somersault had been performed, and to have had no share in helping it. But in the case of the other planets, the satellites have had a large share in driving the plane of the planet's rotation right over. Thus Jupiter was tilted rather more than half way by the action of the solar tides, and then he evolved the inner satellites, and it was by their influence that the inversion was completed. Neptune, on the other hand, being very remote from the sun, began to turn over slowly ; before the process had gone very far, the satellite was evolved, and the plane of rotation began to sink back again to the original position ; thus the motion and direction of rotation remain retrograde.

As to what happens to the satellites when the planet turns over, it is almost more difficult to make a generalisation. In some cases the circumstances are such that a satellite, which separated from the planet when it was rotating backwards, would follow the planet's equator and turn over with it. Probably this is what happened in the case of Iapetus and of J VI and VII. But in other cases, namely, those of Phœbe and J VIII, the satellites are left to retain their original motion.

We see now why it is only the extreme outermost satellites in these two systems that move backwards. They were the earliest to split off, and refer to a time before the tilting of the planet by the solar tides had proceeded very far. When the theory was put forward, it was predicted that, should any very distant satellites of Jupiter and Saturn be found, they also should revolve backwards. An opportunity for testing this seemed soon to occur. The sixth and seventh satellites of Jupiter were found far outside those previously known ; when, therefore, it was found that they moved forwards, the hypothesis received something of a set back. However, it has turned out that these two satellites were not distant enough from Jupiter ; the still more remote J VIII moves backwards, and greatly strengthens the theory.

COMET C 1908.

We must now leave the system of Jupiter and turn to another subject which has been occupying attention at Greenwich, and,

in fact, the attention of the whole astronomical world—Morehouse's comet—the comet which appeared last autumn, and which, although it has long ceased to be visible in these latitudes, is still being followed with great interest by observers in the southern hemisphere. I daresay I am speaking to some here who will remember the great comet of 1858; others will remember the comets of 1862, 1874 or 1882. I feel rather at a disadvantage in speaking of comets to you; comets nowadays are not what they used to be, and compared with these glorious wanderers, the little comet of which I have to speak made a very poor show. It will, I fear, be necessary to make out a very strong case if you are to be persuaded that this little object, which was hardly more than glimpsed with the naked eye by practised observers, and was generally considered very disappointing as seen with the telescope, is worth the attention that has been devoted to it.

Before proceeding further let me introduce the comet under consideration. Here it is as it appeared on September 30 last year. [Two photographs were thrown in succession on the screen.] But I think no one could undertake to recognise it again from its photograph. Here is the same comet a day later. Everything is altered completely; you cannot point to a feature in the tail on this photograph and say that it corresponds to a certain feature on the previous photograph, and that one has changed into the other; you cannot say that this is the tail of the previous day modified. As far as can be judged the tail is an entirely new one, and if these two photographs were all the data available we might, it is true, learn something of the mode in which the activity breaks out and dies away from day to day, but it would not be possible to trace the motions of the particles of the tail, and the most hopeful line of research by which something may be understood of the nature of the comet's tail would be closed. Now that is what has generally happened in the case of the former comets, at any rate, since it has been possible to apply powerful telescopes and the best photographic methods to the study. Most comets, and the same applies to planets and satellites, are well placed for observation for, say, three hours at a time, but, the rest of the night, are too close to the horizon to be satisfactorily photographed. But this comet for a month or so after its discovery chanced to be in the neighbourhood of the North Pole, so that it simply circled in a small path round the Pole during the twenty-four hours, and the whole night long from dusk to dawn it was in a favourable position for being photographed. That is the first factor which rendered this comet an important one—its exceptional position. An observatory in a high latitude like ours is generally at a great disadvantage compared with one at the equator; we, for instance, never have an opportunity of seeing Mars under really good conditions; but for once high latitude was useful.

It was very soon realised that here was an unusual opportunity

for studying the processes going on in a comet, and the Astronomer Royal arranged for the taking of long series of photographs at frequent intervals, so as to obtain, as it were, a cinematograph record of the changes taking place throughout the night. The weather was on the whole favourable, and several times a series of eight or nine photographs on a single night was obtained: in one case the series extended over nearly nine hours, so that there is an opportunity of following step by step the great and striking changes which the comet undergoes in that interval. Altogether the three observers, Mr. Davidson, Mr. Melotte, and Mr. Edney, by dint of constant and continuous watching, obtained 200 photographs; from these are selected the photographs that are being shown on the screen to-night.

Besides its position this comet was from another point of view remarkably favourable for our purpose, namely, of studying the motions in the tail. I think that never before have photographs been obtained showing such an amount of intricate detail and such delicate and contorted streamers. This circumstance can best be appreciated by comparing this comet with others previously photographed at Greenwich under precisely similar conditions. In all our previous photographs the streamers are of a straight or gracefully curved character—never contorted, never resembling wavy locks of hair, such as the name “comet” suggests.* But in the present comet the tail is generally composed of fine twisting or curdled streamers; there are marks which you can seize on, set cross-wires on, and follow through from photograph to photograph. That is just what we require for purposes of investigation. A curious feature is that the head is very faint compared with other comets. It seems clear that this comet had really a very small nucleus, but that for some reason it was extraordinarily explosive, and poured out vast quantities of fine particles to form the abnormally bright tail. The comet was much brighter photographically than visually; its light was unusually strong at the violet end of the spectrum.

The part of the comet's tail which is shown on the photographs, taken with the thirty-inch mirror at Greenwich observatory, is about a degree and a half. You can form a fair idea of what that looks like in the sky by remembering that the apparent width of the moon is half a degree; actually in miles the length of tail shown here would be about four million miles. That sounds a great length, but on suitable photographs a much greater length of tail could be traced; we have some photographs taken with an ordinary camera portrait-lens on which a tail 12° long is shown—eight times as much as is shown on the photographs on the screen. As a matter of fact those taken by other observers all over the world are in general on a smaller scale than these Greenwich reflector photographs and show

* This statement refers to the part of the tail close to the head, which is shown in our reflector photographs.

the outer parts of the tail; so far as I know there are no other series of photographs similar to these, which are specially adapted for studying the fine detail near the head of the comet, and what may be called the first eighth of its tail. Probably those which approach to them most nearly are some by Professor Wolf at Heidelberg, which are on half the scale. Yet one other fact I may mention; owing to the great light-gathering power of the reflector fine photographs of the comet could be obtained with quite short exposures; this is a matter of great consequence. At first we took some plates with exposures of half to three-quarters of an hour as well as shorter ones; but it was found that exposures of ten to fifteen minutes showed finer detail and were better for our purposes. This was not because the longer ones were over-exposed, but because the changes taking place within the comet are so rapid that in a thirty-minutes exposure all the fine lines and streamers become blurred.

I hope it will be understood that I mention these features of the Greenwich photographs, not with any idea of claiming that they are superior to or more important than those taken by many observers elsewhere; to do so would not do justice to the remarkable results obtained by Barnard, Wolf and others; it is rather to show that these photographs cover new ground, and the evidence they afford and the results they lead to will be somewhat different from those elsewhere. And for the reasons already explained this is the first time it has been found possible to thoroughly examine a comet in this way. As regards the results, we are still in the midst of the work of examining the photographs, a great deal of preliminary computation had to be gone through before a serious start could be made; the measures of the details require very much comparing and digesting. By the kindness of the Astronomer Royal I am permitted to speak of them so far as we have progressed at present, but all I can say must necessarily be of a very preliminary character.

Let us first turn attention to the intermittent character of the activity of the comet. The outpouring of matter to form the tail is not at all steady, but periods of great disturbance and quiescence succeed one another at intervals of a few days. The eruption goes through stages which are more or less well defined. [A typical series of slides showing the stages of the eruption was shown.]

But the great outstanding problem in the study of comets, the one on which more particularly we hope to obtain some light by the taking of this extensive series of photographs, is the study of the motions of the particles of the tail and the forces which cause them. The tail streams out from the comet in the direction nearly directly away from the sun. It is not simply left behind by the comet. It is driven away from the sun, and may be either in front or behind the head in its orbital motion according to circumstances. We are accustomed to regard the sun as the centre of an attractive force by which the planets are kept in their orbits, and comets' heads also

move under the same law so far as can be ascertained. But the tail particles of the comet do not seem to recognise this force, or, at least, with them it is more than counterbalanced by a repulsion; for them the sun behaves as a centre of repulsion and urges them away. It was for long a great puzzle to understand how the sun can thus play a double role, but various plausible explanations are forthcoming, some of which I shall consider presently. The difficulty is now not so much to suggest an explanation as to decide between rival explanations.

A great deal of work has already been done on this subject in the case of previous comets. Of modern workers the name of Bredichin stands out pre-eminently. Much of his work was done thirty years ago—of course, derived from visual observations—but it is still of fundamental importance. By studying how much the tail lagged behind the radius vector (by the radius vector is meant the direction from the sun to the comet, which is, of course, the direction in which the solar repulsion acts), and also the curvature of the tail, Bredichin was able to form a measure of the amount of this repulsive force. It is convenient to compare this repulsion with the ordinary attractive force of gravitation of the sun in the same position (the force which the head of the comet is experiencing) and use that as a unit. It was found that the tails of comets could be divided into three classes, for which the repulsive force was respectively 18, 2.2 to 0.5, and 0.3 to 0 in terms of this unit. Very often a comet would have two or three distinct tails, so that all three classes might be represented in a single comet.

The introduction of photography has naturally improved the methods by which the forces may be determined. Perhaps the most remarkable researches are those of Jaegermann; he has, in the case of several comets, proved the existence of repulsive forces in just the same way that the existence of the ordinary attractive force on a planet is proved—namely, by calculating the orbit in space of the matter acted on by the force. To show how this may be done. [Two photographs of the present comet taken three hours apart were shown combined on one plate, so as to show the motion amongst the stars in that interval.] The head of the comet is moving in one orbit; but there is a detached mass, which was evidently projected from the head some hours before, which is moving in a different orbit. Now from four observations of the position of this head, we can calculate the orbit and the force exerted on it by the sun (I say four observations, and not three, because I want to determine the force exerted by the sun, and not assume it as known). Similarly, from four observations of this detached piece we might calculate its orbit and the force exerted on it by the sun. Jaegermann has examined in this way detached portions of former comets, and shown that the detached portions pursue hyperbolic orbits convex to the sun, showing that they are repelled from it.

The results of determinations of the repulsion, whether found in

this way or following Bredichin's method, have generally given repulsive forces according to his rule, i.e. not exceeding 18 times the attractive force of the sun; but there are two or three cases where clear evidence has been attained of a repulsive force 36, and in one case, namely, the case illustrated on the screen, Jaegermann found a force of at any rate over 60; he put it at 89.

These repulsive forces are determined from the motions of large masses of cometary matter, or from the general direction of the streaming of the tail. It is of interest to see whether the small knots or bends or bright patches, which offer a definite mark for measurement, show the same evidence of repulsion. It is necessary in pursuing this inquiry to be very careful to choose for measurement something that is a real material point. We are liable, for instance, to choose as a mark the point of crossing of two streamers; that must be avoided, for the speed of such a point is not generally that of the material of the streamers.

We have to bear in mind the distinction between the two causes of motion, an initial velocity due to explosion of the nucleus, and motion due to the action of a force; a force of repulsion from the sun produces not merely a velocity along the radius vector, but a continually increasing velocity, that is to say, an acceleration. Now one of the most important and perplexing results of studying the present comet is that there is very little evidence of acceleration, at least of the steady acceleration which would indicate a steady force. The tendency seems to be for the velocity to increase rapidly at first, but then to become constant. I showed just now two photographs in which a detached mass had broken away from the comet; it had a velocity relative to the nucleus of 2'·6 per hour in the interval between those two photographs, shortly afterwards, according to Professor Barnard's and our own photographs, it reached a speed of 3'·4 per hour, and remained constant at that for three days. There is a well-marked knot seen on photographs on October 3 which bears every indication of being a material feature, and not an illusion due to the crossing of rays. This can be traced on our photographs for 6 hours, during which time its distance from the head (measured in the direction of the radius vector) increases from 50,000 miles to 200,000 miles. During the first hour there is just a suspicion of an acceleration; but afterwards it moved parallel to the radius vector with a quite uniform velocity of 27,000 miles per hour (nearly 8 miles a second). This is rather a good case, for there are nine photographs in the series, and the point of measurement is very well marked, but similar cases could be multiplied almost indefinitely. As soon as the distance from the head has reached a comparatively small limit, the repulsive force seems to cease. It might be suggested that the uniformity of the velocity shows that it was produced by the initial impulse, and that solar repulsion had little to do with it. But I may remind you that the way in which

all the motion of the tail particles is directed nearly straight away from the sun is sufficient to assure us that a solar repulsion, and not the initial explosion, is predominant in causing the motion. The suggestion that the sun might in some way act indirectly, causing the explosion to expel the particles in the direction away from the sun, cannot be accepted: for the study of the envelopes, of which I shall speak later, indicates that, if anything, the particles are projected more abundantly towards the sun in the first instance. Besides, in the case of the knot on October 3, if we prolong backwards the uniform path we find it does not pass through the nucleus at all.

There is a curious plate taken with the portrait-lens, which, perhaps, represents something exceptional happening, but which certainly confirms the idea that the repulsion does not continue to act indefinitely. In it [shown on the screen] the tail matter is seen to be no longer streaming off, but is hanging about in a cloud and gradually diffusing.

Perhaps the simplest way of explaining this unexpected absence of acceleration is to suppose that there is some resisting material in the space, so that a limiting velocity is reached for which friction and repulsion counterbalance one another. Or we might, perhaps, suppose that the bright patches do not strictly belong to the comet, but are due to something projected from the sun, which causes a luminescence of the tail matter of the comet through which it passes. I rather lean, however, to an electrical explanation. It has been shown by various writers that the repulsion which causes the tail may be due to the action of the sun's electric field on the charged particles shot off from the disintegrating head of the comet. It seems possible that these might, after a short time, encounter particles of the opposite sign and become neutralised. Assume for the sake of definiteness that the sun has a positive charge, but is surrounded by the negative electrons shot off from it, as from all white hot bodies, which form a swarm extending beyond the comet. Then the positive particles of the comet would be repelled, but at the same time they would be bombarded by and encounter negative electrons which might combine with and neutralise them, so that the repulsion would cease to act. I think the recombination might take place much more easily where the tail is densest, because a number of collisions would generally be necessary to capture a negative electron, so that in the fine streamers, which form the tails of ordinary comets, the repulsion might continue to act much longer. This last is a suggestion, not so much based on inherent probability, but put forward as a compromise to save us from too hastily throwing over a great deal of research and study on previous comets which has almost always assumed a constant repulsion.

Of the various theories that have been put forward in order to account for the repulsion of comets' tails, besides the electrical theories, probably the most popular is that which ascribes the

streaming away from the sun to the effect of light-pressure. When radiation of any kind, sunlight or the heat from a fire falls on a surface, it exerts a pressure on that surface tending to drive it back. The light from the lantern which was falling on the screen just now presses against the screen, though the whole pressure is exceedingly minute; the whole pressure of sunlight falling on the earth is something like 150,000 tons weight, a force which is insufficient to make the earth budge so much as one hair's breadth from its path. But on the small particles of a comet's tail its effect may be of importance, because although the force of pressure decreases with the size of the particle, it does not do so so rapidly as the volume or weight, so that the effect on the motion is up to a certain point greater the smaller the particle. In the case of a particle $\frac{1}{200000}$ in. in diameter, the light-pressure would just about balance gravitation; such a body would be neither attracted to nor repelled from the sun. For one whose diameter is $\frac{1}{1500000}$ in., the repulsion of light-pressure would be twenty times gravitation. You will remember that Bredichin found three classes of tails, of which the one most powerfully repelled the repulsion was 18 times gravitation. We have only to suppose that the particles are of this order of magnitude in order to account fully for his results. The existence of light-pressure was deduced from theoretical considerations, but it does not depend on theory alone. The repulsion can be shown in the laboratory. Hull and Nichols actually tried to make an artificial comet, using the fine particles of lycopodium powder to show the repulsion of the tail. Unfortunately, although a repulsive force was shown, it was due mainly not to light-pressure, but to another effect.

With a particle about $\frac{1}{1500000}$ in. in diameter, the repulsion is 20 times the attraction of gravity. Can we proceed to still smaller particles and account for forces of 36, 90 units or still higher? It appears not. The size mentioned is about the limit, and for small particles, we find, instead of increasing repulsion, less repulsion. Very minute particles offer practically no obstacle to the passage of light which, instead of pressing against them, bends freely round. I daresay by suitable assumptions, as to the density of the material, forces of 36 units might be accounted for, but the hypothesis of light-pressure seems hardly competent to account for the greater repulsive forces. It must not, however, be supposed that the theory of light-pressure is thus discredited: light-pressure must act, and probably acts powerfully, on the minute particles which constitute a comet's tail, but a careful analysis of the strange motions and transformations taking place has convinced many astronomers that other forces are at work modifying and in some cases increasing the repulsion. In this connection the evidence that the repulsion is by no means a constant force has obviously a most important bearing.

At first it is with an almost overwhelming sense of the complexity of the problem that we sit down before this mass of material, striving

to see through the twisting streamers and changing features to the few simple forces that govern it all. There are, however, a few signs of regularity in the photographs to which it seems most hopeful first to turn. I would therefore draw your attention to the envelopes. It is very difficult to reproduce these clearly on lantern slides, although they are plain enough on the original negatives. The envelopes are wreaths or veils thrown out towards the sun and flowing away on each side. They are not like the streamers from the nucleus, for they seem quite detached, forming an arch over the head. The mode of formation may be illustrated by a well-known analogy. If you have a fountain consisting of a large number of jets of water in different directions, the limiting surface is a sort of dome in the form of a paraboloid, which, when seen sideways, exactly imitates the envelope of a comet. It is not merely a bounding surface beyond which none of the water is projected; the arch is thickened along this surface. When the water is turned on fuller, the arch rises; if it is turned off gradually it sinks, but if it is turned off suddenly the arch does not subside, but vanishes; the water of course subsides, but the thickening vanishes.

It can hardly be doubted that the envelopes of a comet are formed in this way; the explosion, from which the envelope results, throws out matter with fairly uniform speed in all directions, this matter being under the influence of solar repulsion, just as in the analogous case the water was under gravitation. By studying them we can learn something of the explosions that produce them; further in them we are concerned with the general mass of fine particles, so that the study of the rather exceptional knots and luminous patches is supplemented; and finally in them we have to deal with the repulsion of the particles, very shortly after they are projected, which is of special importance in the light of the recent evidence that the repulsion may cease to act.

The best defined and most regular envelopes on the Greenwich plates are those of October 27: the envelopes approach the parabolic form so closely as to confirm our hypothesis as to their formation, and to indicate that any disturbing forces are small. I give below some measures of two of the envelopes shown on that night on plates taken at various times. The first column shows the height of the arch deduced from measures made at the apex, that is to say, by direct measurement; the second column from measures made of the direction of the envelope near the ends of the *latus rectum*, and therefore deduced indirectly. We have thus two independent determinations of the height.

The figures (p. 575) show very characteristically the transitory nature of the envelopes and of the explosions. The large one, for instance, formed at about 8 h. 30 m. (it was hardly formed in the first photograph), and in the space of two hours subsided from its original height of 70,000 miles to 40,000 miles; that is the typical behaviour

ENVELOPES OF COMET C 1908. Oct. 27.

(Distance of the vertex of the envelope from the nucleus of the comet.)

Time.		Outer Envelope.		Inner Envelope.	
h.	m.	(1)	(2)	(1)	(2)
		miles.	miles.	miles.	miles.
8	23	71,000	71,000	43,000	35,500
9	3	64,000	61,000	38,000	30,500
9	32	51,000	50,000	26,500	30,000
10	2	48,000	44,000	16,000	14,000
10	28	42,500	41,000	21,000	19,000

(1) Deduced from measurements made at the vertex.

(2) Deduced from measurements of the course of the envelope near the ends of the latus rectum.

of envelopes; it indicates that the explosion is strongest at first, and then dies down rapidly. But we can make another deduction: the envelope was beginning to form at 8 h. 23 m.; by 9 h. 3 m. the complete arch was visible. Now it can be shown theoretically that the formation of an envelope does not take place instantaneously along the entire arch; if, for example, the material forming the apex left the nucleus an hour previously, that forming the ends of the latus rectum left an hour and twenty-five minutes previously, and so on in proportion. Conversely, we can argue from the fact that the whole arch appeared in so short a time, and that the ends of the latus rectum begin to collapse very little, if at all, after the apex, that the time taken by the matter to travel from the nucleus to the apex must be very small. I dare not trust the figures in the Table so far as to calculate that time from them, but probably it will be a very safe outside limit if we say that that time is not more than two hours. A simple calculation shows that, for that to be the case, the solar repulsion acting on these particles must be at least 800 units—far larger than any repulsion calculated by Bredichin, Jaegermann, or others; the velocity of projection would need to be 70,000 miles per hour. These figures are far too startling for us to immediately accept them, though, of course, if it is admitted that the repulsion acts only for a short time, instead of continuously, it must be correspondingly more powerful during that time. We are at the beginning not at the end of an investigation; but I am convinced that a great deal is to be learnt from the study of these envelopes. Unfortunately they are often very complicated. Sometimes two of them will intersect, or they may be all askew. That need not be considered surprising, for the simple parabolical form can only occur when the force of the explosion is equal in all directions.

Another feature sometimes possessing some degree of regularity is the waving of the streamers proceeding from the head. This is a feature which will certainly repay a much more careful examination than we have yet had time to give. The most immediately striking

feature is that two or more of the streamers often run parallel to one another, their undulations exactly corresponding with the crest of one fitting over the crest of the other. As might be expected the undulations become larger, both in length and in amplitude, as we proceed outwards from the head. Professor Wolf has published some interesting data on this point. The thought suggests itself that the curves may be spiral curves produced by a rotation of the nucleus of the comet whilst it is discharging the streamers. The hypothesis is one which can probably be tested by careful measurement; meanwhile it appears to be more probable that the undulations, like so many other features, must be accounted for by changes, perhaps rhythmic changes, in the force of expulsion of the material.

Whatever may be the true cause of the phenomena of comets' tails, it is at least clear that the source of the power which forms them and which directs them is to be found in the sun. I am not sure that the exceptional activity of this comet is not due to the physical state of the sun at the time rather than to the constitution of the object itself. Certainly progress in explaining the phenomena is hampered by our partial ignorance of the electrical and physical conditions of the sun's surface. Therefore I cannot close this discourse without alluding to what is perhaps the greatest result of any that recent years have afforded to astronomy: Professor Hale's photographs of Solar Vortices and investigations of their magnetic fields. With the great Tower telescope in the clear atmosphere of Mount Wilson Observatory he obtained photographs of the remarkable structure, revealing to our eyes the gigantic whirlwinds raging over the solar surface, above the sun-spots. These photographs are taken with the light of one particular wave-length the $H\alpha$ line of hydrogen. He has gone farther and shown, I believe, to the satisfaction of physicists, that the light passing up through these vortices bears the sure marks of having passed through a strong magnetic field, whose lines run perpendicular to the solar surface, and that, according as the vortices rotate clockwise or counter-clockwise, the lines of magnetic force run from or into the sun. The chain of evidence seems to show that the field is produced by the rotation of negatively charged material in these vortices.

It would be hard to exaggerate the value of these latest revelations as to the condition of the sun, far though they are from satisfying our inquiries or enabling us to realise that power which over all the millions of miles convulses the comet, and scatters its trailing débris to the remotest parts.

[A. S. E.]

WEEKLY EVENING MEETING,

Friday, April 2, 1909.

SIR WILLIAM CROOKES, LL.D. D.Sc. F.R.S., Honorary
Secretary and Vice-President, in the Chair.

PROFESSOR SIR J. J. THOMSON, M.A. LL.D. D.Sc. F.R.S. *M.R.I.* ;
Professor of Natural Philosophy, Royal Institution.

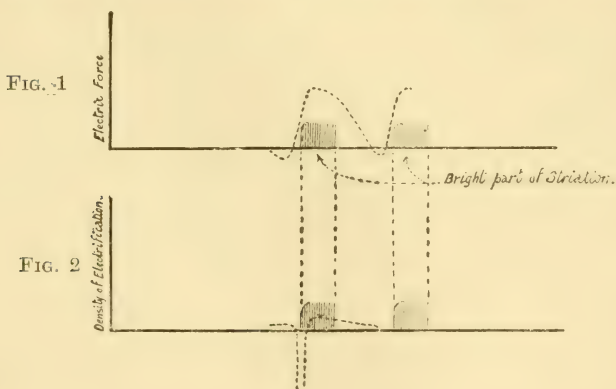
Electrical Striations.

ONE of the most conspicuous features of the electric discharge through gases, when the pressure is within certain limits, is the exceedingly well-marked alternations of light and darkness which occur in the positive column. These alternations, which are called striations, are so varied and beautiful that since their discovery by Abria in 1843 they have attracted the attention of many physicists. Grove, Gassiot, Spottiswoode and Moulton, De la Rue and Müller, Crookes, Wood, Skinner, H. A. Wilson, and Willows, have published important researches on the conditions under which the striations are produced ; on the influence upon them of such things as the nature and pressure of the gas, the size of the tube, the current passing through it ; and on the distribution of the electric force in the neighbourhood of a striation. The investigations described in the following paper relate for the most part to the last of these questions, and were made with the object of testing a theory of the striations which I gave in my Treatise on the Conduction of Electricity through Gases. For these experiments I used tubes fitted with Welmelt cathodes, i.e. the cathode was a strip of platinum-foil heated to redness, and having on it a spot of lime or barium oxide.*

With these cathodes large currents can be sent through the tube, and remarkably bright and steady striations obtained at lower pressures and with smaller potential-differences than with the ordinary type of discharge. The pressure has, however, to be low, considerably less than 1 mm. of mercury, to get the full advantages from these cathodes. The first point to which attention was directed was the distribution of electric force along the line of the discharge. Investigations on this point have already been made by Skinner and H. A. Wilson, but it seemed to me that the steadiness of the striations

* My assistant, Mr. Everett, has found that these cathodes can be very easily made by letting a drop of sealing-wax fall on the foil, and then burning away the combustible matter by heating the foil to incandescence. Sealing-wax seems to contain large quantities of some salt of barium.

with the Wehnelt cathode made this method of investigation particularly suited for these investigations. The first method used to measure the variations in the electric force along the discharge was to find the variation in the difference of potential between two platinum wires 1 mm. apart as the wires were moved from the cathode to the anode. Several devices were used for this purpose: in some the platinum wires (surrounded up to about a millimetre from their tips with glass rods) were carried on a sort of railroad and moved from cathode to anode. The electrodes in the discharge-tube in this case were fixed. The measurements of the potential-differences made by this method at low pressures gave the very remarkable result that just on the cathode side of the bright part of a striation the electric force was negative (i.e. that the force on a positive charge was in the direction from *cathode to anode*): on crossing over the bright boundary to the anode side the electric force at once became positive, and rose to



a high value. It soon, however, began to diminish, and went on diminishing up to the cathode side of the bright part of the next striation on the anode side. The distribution of the electric force in the striation is represented in Fig. 1, and the corresponding distribution of positive and negative electricity in Fig. 2, the ordinates representing the distribution of the electrification both as to magnitude and sign. Thus if these measurements of the electric field can be relied on we have intense negative electrification at the bright head of a striation (by head is meant the side next the cathode), and a weak positive electrification through the rest of the field. The transition from positive to negative force was very abrupt and well marked, so much so indeed that the position of the platinum wires in the striation could be ascertained with great accuracy without looking at the discharge, by observing the deflexions of the electrometer by which the potential-difference between the wires was measured.

Many changes were made in the way in which the wires used as detectors were arranged; thus, to prevent any screening of the one wire by the other, an apparatus was used in which the two platinum wires were brought in from opposite sides of the tube, so that there should be no overlapping; exactly the same results were obtained with this apparatus as with the other. Again, another arrangement, similar to one previously used by Professor H. A. Wilson, was tried, in which the exploring electrodes were kept fixed, and the anode and cathode kept at fixed distance apart were, by means of a float, moved relatively to the exploring electrodes *a b*, so that these could occupy all positions from the anode to the cathode. The arrangement is represented in Fig. 3. The exploring electrodes in some of the experiments protruded about a millimetre beyond the ends of the glass tubes into which they were sealed; in other experiments very fine hollow glass tubes were used to cover the wires, and the wires instead of protruding beyond the glass stopped short at about a millimetre from the end of the tube; this arrangement was adopted with the idea of protecting the wires against streams of corpuscles coming down the tube: these by giving up their charges to the wire might cause this to acquire potentials different from those of the gas at the tips of the wire. The results obtained with all these modifications were exactly the same as those obtained with the first type of apparatus, i.e. there was always when the pressure of the gas was low a negative electric force just in front, i.e. on the cathode side of the bright part of a striation; this changed to a large positive force as soon as the bright boundary of the striation was passed; at a short distance from the front of the striation this force began to diminish and went on diminishing until the front of the next striation on the anode side was reached.

Though the indications of all these wire explorers agreed in pointing to the existence of a negative force in front of these striations,

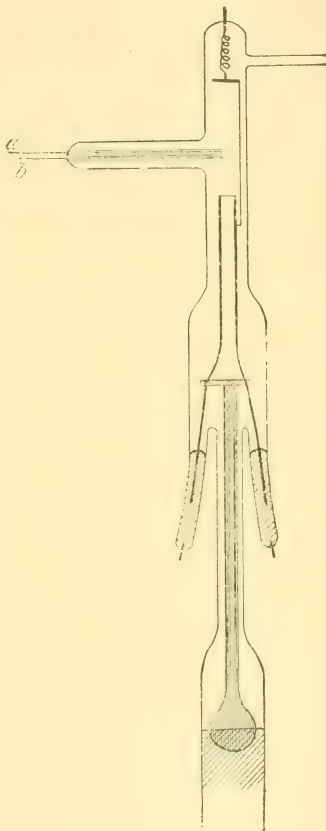


FIG. 3.

yet I felt that the existence of a negative force could never be proved by the use of wire detectors.

For let us consider what the existence of a negative force implies. The electric current is always in the same direction throughout the tube, and therefore the average movement of the ions is in the same direction at all parts of the tube; thus, whenever the electric force is negative, there must be ions moving against the electric force instead of with it. Now the validity of the method of the wire electrodes depends upon the assumption that the ions in the neighbourhood of the tip of these electrodes follow the lines of force, that if, for example, the tip were at a higher potential than the gas so that the force on a positive ion were away from the tip, negative ions would follow the direction of the force acting upon them, and run into the tip and lower its potential until it became the same as that of the gas in its neighbourhood. But if the ions do not follow the electric force, and the existence of a negative force implies that some of them at any rate do not, we have no right to assume that the potential of the wire is the same as that of the gas. In some simple cases it is evident that it would not be so. Thus suppose the wire were exposed to a stream of cathode rays, and that there were no positive ions in its neighbourhood, then it is evident that the wire would acquire the potential of the cathode from which the rays started and not that of the gas around the wire.

For these reasons I felt that the existence of a negative force could not be established by means of wire electrodes, and I adopted an entirely different method of measuring the electric force along the discharge-tube. The principle of this method is as follows: Imagine a very fine pencil of cathode rays, travelling at right angles to the line joining the cathode and anode, to pass through the discharge-tube. As it crosses the discharge at any place it will be acted upon by the electric force at the point of the discharge, and will be deflected by an amount proportional to the electric force. The deflexion will be from the cathode of the discharge-tube if the force is positive, towards it if the force is negative. If very small pencils of cathode rays are used the disturbance of the electric field in the discharge-tube due to the negative charge on the rays is quite insignificant, and there is none of that distortion of the striations which, to a greater or less extent, always occur when exploring metallic electrodes are used.

The arrangement by which this principle is carried out in practice is shown in Fig. 2. The cathode and anode are fastened together by a piece of glass-rod and fastened to a float, floating on the top of a mercury column. By raising or lowering the column the anode and cathode can be moved up and down the discharge-tube. This arrangement is the same as that used with the wire detectors and shown in Fig. 3. The wires *a b* (Fig. 3) were replaced by cathode rays generated in the side tube *S* (Fig. 4) by a small induction-coil:

the cathode C is at the end of the tube, the anode is the metal plug A connected with the earth; a very fine hole was bored in this plug and through it a pencil of rays passed across the discharge and then entered the side tube T. In this tube there was a screen W, covered by a phosphorescent substance, in some cases willemite; in others the screen was a zinc sulphide one procured from Mr. Glew. The place of impact of the cathode rays against the screen is marked by a luminous spot, and by measuring with a cathetometer the deflexion of this spot the magnitude and direction of the electric force acting on the cathode rays as they pass across the discharge-tube can be determined. Tinfoil was wrapped round the outside of the discharge-tube to neutralize the effect of electric charges on the glass walls of the tube. The use of cathode rays not only avoids the disturbance due to the presence of the wires, but inasmuch as the cathode rays are negatively electrified particles it enables us to measure the effect

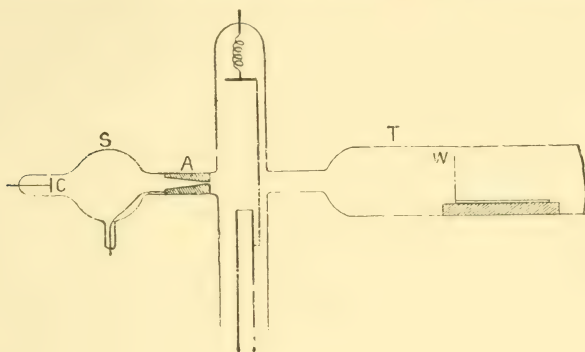


FIG. 4.

of the field on such particles, and as it is the corpuscles which carry practically all the current in the discharge, the method enables us to observe in a very direct way the most important factor in the discharge.

The method is, however, limited to the case where the pressure in the discharge-tube is low, as it is only at low pressures that the cathode rays produce a well-defined spot on the screen.

Observations made with this method showed unmistakably the existence of negative forces in certain parts of the discharge, and in fact the distribution of electric force along the tube as determined by this method agreed remarkably well with that determined by the method of the exploring wire. When the discharge was striated it was generally found that where the cathode rays passed underneath a striation, i.e. on the cathode side of the bright part of the striation, there was a small deflexion of the cathode rays towards the cathode

of the discharge-tube, showing that in this part of the discharge the electric force is negative, while when the path of the cathode rays passed through the bright part of a striation there was a large deflexion of the cathode rays from the cathode of the discharge-tube, showing that in this part of the discharge the electric force was strongly positive. The change from the small negative deflexion to the strong positive one was exceedingly abrupt, so much so that when the anode and cathode were moving downwards, owing to the sinking of the float supporting them, and one striation after another was thus being brought across the path of the cathode rays, the phosphorescent spot moved as abruptly as if it had been struck by a blow when the bright head of a striation crossed its path. At the low pressures at which these observations are made the potential-difference between the electrodes when the current is large enough to produce striations is exceedingly small, often not exceeding 60 or 70 volts. Under these circumstances the negative forces although unmistakable are small: when, however, the current through the tube is reduced until the discharge is no longer striated, the potential-difference between the electrodes is very much increased, and now large negative forces can be observed in the neighbourhood of the anode. Sometimes the region in which the force is negative extends a considerable distance from the anode: in one case I observed a negative force for two-thirds of the distance from the anode to the cathode.

As the corpuscles in the cathode rays have an exceedingly small mass they are able to follow very rapid variations in the electric field; by means of them we can observe the gradual establishment of the steady state of the discharge and the change in the direction of the electric force at certain places from positive to negative. Thus suppose the steady current through the tube is small and the potential-difference is considerable, and that the pencil of cathode rays is passing through the discharge near the anode, then if we watch the behaviour of the phosphorescent spot in the interval immediately following the application of the potential-difference to the tube, we shall find that when the current first starts through the tube the spot is repelled from the cathode, showing that at this stage the electric force is positive throughout the tube. This repulsion of the cathode rays is however only momentary: the spot jumps back, and after a very short interval the spot is *attracted* towards the cathode, showing that the force in this region is now negative. Thus during this interval the ions in the gas and those clinging to the walls of the tube have rearranged themselves in such a way as to reverse the force in the field. This momentary deflexion is much more perceptible near the anode than some distance away from it; the rearrangement seems to spread from the cathode, and to be established so rapidly close to that electrode that there is no time to observe it, while as we travel away from the cathode the steady state

is reached after longer and longer intervals, and there is time to observe the initial distribution of the electric field.

We see that the result of the experiments with the cathode rays is to confirm the indications of the wire detector, even when the main current is travelling against the electric force. That the wires in this case should indicate the potential is very remarkable, and must be due I think to the presence in the discharge of slowly moving ions in addition to the swiftly moving ones which carry the main portion of the current, having acquired in other parts of the field sufficient impetus to carry them for some distance against an opposing electric force. The slowly moving ions would be produced by the collisions of the quick ones, and those produced near the tips of the wire electrodes would follow the lines of electric force near the wire and equalize the potentials between the wire and the gas.

The great change in the electric force which occurs at the bright fronts of the striations shows that in these regions we have a great accumulation of negative electricity, while the distribution of the electric force in other parts of the striations and in the dark parts between two striations shows that in those regions there is a slight excess of positive electricity. The magnitude of the charges in the electric force is shown by the following numbers, which indicate the electric force in volts per centimetre at different parts of the striation. x in the following table is the distance in millimetres from the bright head of the striation: x is taken positive when measured towards the anode, negative towards the cathode, thus $x = -1$ denotes a place 1 millimetre from the bright head of the striation on the cathode side. X is the electric force in volts per centimetre at x . The gas was hydrogen at a low pressure.

x	X
- .5	- 9
+ .5	+ 67
+ 1.5	+ 33
+ 3.5	+ 30
+ 7	+ 10
+ 9	- 10

The last reading was at a point just in front of a second striation.

The distance between the bright heads of successive striations was 9 mm. and the thickness of the dark space 2 mm.

From the preceding table we see that in the space of 1 mm. at the head of a striation we have a change in the electric force of 76 volts per cm. By means of the equation

$$\frac{dX}{dx} = 4\pi\rho$$

we see that the density of the negative electricity at the head of the striation is about $\frac{1}{5}$ of an electrostatic unit per c.c. The density of the positive electricity in the other portions is very much less than this. With Wehnelt electrodes there is frequently only a small potential-difference between corresponding points in adjacent striations: in some cases this difference was only 2·7 volts.

The changes in the electric force are much more abrupt at low pressures than at high ones; though there is always a large increase in the force at the bright head of the striation. I have not observed the existence of the negative forces when the pressure was more than a fraction of a millimetre of mercury.

I have found other cases in which the negative forces are even more pronounced than those I have already considered; perhaps the most striking of these is one where the anode and cathode are connected together and with earth by stout metallic connexions, so that the two are at the same potential, and therefore the average negative force between them is as great as the average positive force. The anode is perforated by a very fine hole, and through this hole a stream of Canal rays, i.e. positively electrified particles, passes into the tube: this produces when the pressure is suitable a fully developed discharge, with striations, Faraday dark space, a well-developed negative glow and dark space; and in spite of the anode and cathode being at the same potential there is in this case the normal cathode fall of about 300 volts at the cathode: the negative forces in a tube of this kind must be very considerable, as they have to balance the cathode fall.

The heaping up of the negative electricity at the head of the striations seems to me to be the most important factor in the production of striations.

This concentration of the negative electricity at regular intervals along the discharge may be explained as follows. Consider a stream of negative corpuscles projected from the neighbourhood of the cathode with considerable velocity: they will collide against the molecules of the gas, and thereby lose velocity: if the electric field acting on them is not sufficiently intense to restore the velocity lost by the collisions, the corpuscles will lose velocity as they travel through the gas, thus the corpuscles in the rear will gain on those in front, and therefore the density of the corpuscles and therefore of the negative electricity will be greater in the front, and by the equation $\frac{dX}{dx} = 4\pi\rho$,

when X is the electric force, x the distance from the cathode, and ρ the density of the electricity, the electric force will increase rapidly in consequence of this concentration. This increase in the force will increase the velocity of the particles in front. If the increase in velocity is not sufficient to make the corpuscles ionize the gas by collision, the congestion will be relieved by the gradually increasing velocity of the corpuscles in front, and there will be no periodicity either in the density of the electricity or the electric force.

If, however, the force increases so that the corpuscles produce ions by collision quite a different state of affairs will occur ; suppose that when the corpuscles get to a place P, their velocity is sufficient to produce ionization. On the anode side of P positive and negative ions will be produced, the positive ones will crowd towards P, the negative ones will move away from it ; the consequence will be that there will be an excess of positive electrification on the anode side of P : now positive electrification implies a diminution in the electric force as we move towards the anode, thus the electric force will fall. When it has fallen below the value required for ionization the negative electricity will as before begin to accumulate in the front of the stream, and the electric force will again increase to the value required for ionization when the process will be repeated. We shall in this way get a periodicity in the electric force such as is observed in the striated discharge. Thus on this view the concave side of the bright head of the striation acts as a cathode, the corresponding anode being the convex side of the bright head of the adjacent striation on the anode side. Between these two places we have a complete discharge, forming a unit by the combination of which the whole discharge is built up. The ions which carry the current through any unit are for the most part manufactured in the units themselves, so that these units will behave, as Goldstein and Spottiswoode and Moulton have observed the striations to behave, as if they were to a considerable extent independent of each other. The effect of pressure on the distance between the striations can easily be understood from this point of view, for the lower the pressure the greater will be the distance which particles projected with high velocity will travel before their velocity is destroyed. Again, the variations in the electric field are due to the accumulation of electrical charges in the tube. These accumulations may be regarded as electrified disks whose cross-section is that of the tube ; the distance from the disk at which these forces fall to a certain fraction of their maximum value will depend upon the diameter of the disk ; the larger the diameter the greater this distance, so that when the diameter of the tube is small the fluctuations in the intensity of the electric force will be much more rapid than when it is large, and thus we should expect the striations to be much nearer together in a narrow tube than in a wide one.

To explain the variations in the luminosity which accompany these fluctuations in the electric field we must consider the variation in the kinetic energy possessed by the positive ions when they recombine. The recombination of ions does not in general seem to be accompanied by luminosity, unless the ions possess a definite amount of kinetic energy. We certainly can have a gas with great electrical conductivity, and in which a large number of ions are recombining without any visible luminosity ; it seems as if the ions must have a definite amount of kinetic energy for visible light to be developed on their recombination. Now in the space between two striations the

electric field in the part near the cathode side of the bright head of a striation—the dark part—is weak ; here the ions have not got the minimum amount of energy requisite for them to be luminous when they recombine ; in the bright part of the striation the electric field is strong, and here the ions get sufficient kinetic energy to enable them to give out light when they combine. If the energy required for an ion to give out visible light is greater for light at the blue end of the spectrum than at the red, we might get blue light at one part of the striation, red at another, an effect often observed when we have a mixture of mercury vapour and hydrogen in the tube ; a similar separation of the spectra of the two gases in a striation might also be produced if one of the gases were more easily ionized than the other.

I wish to thank my assistant, Mr. Everett, for the assistance he has given me in these investigations.

GENERAL MONTHLY MEETING,

Monday, April 5, 1909.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S.,
President, in the Chair.

Laurence J. Baker, Esq., J.P.

Mrs. S. G. Brown,

Miss Carvill,

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Hugo Müller, Esq., Ph.D. LL.D. F.R.S., for his Donation of £100 to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Honorary Secretary reported, That the Managers had, at their Meeting held this day, elected Sir James Dewar, M.A. LL.D. D.Sc. F.R.S., Fullerian Professor of Chemistry for a period of three years.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

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WEEKLY EVENING MEETING,

Friday, April 23, 1909.

THE RIGHT HON. LORD RAYLEIGH, O.M. P.C. M.A. D.C.L.
LL.D. F.R.S., in the Chair.

ALEXANDER SIEMENS, ESQ., M. INST. C.E., *M.R.I.**Tantalum and its Industrial Applications.*

WHEN the announcement was made in the year 1878 that "the division of the electric light had been successfully accomplished," many people believed that the days of lighting by gas had come to an end, and acted accordingly, much to their own disadvantage: for the competition of the glow-lamp served only to stimulate its rival to new life.

Burners of improved construction, regenerative burners, and finally gas mantles helped to restore to gas the ground it had lost, and until a short time ago even threatened to check the spreading of electric lighting.

Not only this growing competition of gas, but the universal necessity of cheapening the production of commodities that are for general use, forced electrical engineers to study in all its aspects the question of improving the efficiency of electric lighting.

As a guide in their researches they had the well-known principle that the illuminating power of a solid body increases at a much greater ratio than its temperature, or, in other words, that with the increase of temperature a greater percentage of the energy expended for heating the body is converted into light.

There is plenty of room for improvement; for even the most economical source of light, the electric arc lamp, converts only about one per cent. of the energy of the electric current flowing through it, into light, the rest appearing as heat, so that in reality all methods of lighting devised by men are, to a much greater extent, methods of heating.

The first successful incandescent lamp consisted of a carbon filament; and for a long time carbon appeared to be the only suitable substance, although the temperature to which such a filament can be raised is limited to about 1600°C. , as above this point the carbon begins to disintegrate rapidly.

At this temperature the lamp consumes from three to three and a half watts per candle-power, while any attempt to produce light more economically by raising the temperature of the filament results only

in shortening its life, and destroying thereby its power of competing with gas lighting.

An improvement on this result was introduced by Prof. Nernst, of Göttingen, who suggested, as the source of light, refractory earths, similar in character to those used for gas mantles, which, however, conduct electricity only when they are hot.

Lamps constructed on Prof. Nernst's principle have, therefore, to be fitted with contrivances for heating their filaments when starting, which complicate the construction of the lamp.

Another step forward was made by the invention of the osmium lamp, which is produced in a somewhat similar manner to the carbon lamp, by squirting a plastic mixture of metallic oxide and a reducing agent into the shape of a filament, which is gradually heated in a glass bulb by the passage of an electric current, while the bulb is being exhausted by an air pump, or an equivalent device.

As far as utilisation of energy goes, these lamps are a great improvement on carbon lamps, but their filaments are very brittle; and the total production of osmium per year is only about 8 kg. for the whole world, of which 5 kg. are wanted for medical purposes.

In January 1905 Dr. W. von Bolton, the head of the chemical laboratory of the firm of Siemens and Halske, announced in a lecture to the *Elektrotechnische Verein* of Berlin that he had succeeded in producing pure tantalum, and his discourse was followed by Dr. O. Feuerlein, describing how tantalum had been utilised for filaments in the lamp works of the firm.

These discourses presented the result of long years of research work, based on the general principle already alluded to, that that filament would give the best economical results which could be maintained for the longest time at the highest temperature.

The number of substances capable of conducting electricity and of sustaining such high temperatures is very limited, and platinum, the most refractory of the well-known metals, had been tried and found wanting.

It became, therefore, necessary to start the research by devising methods for producing the rare metals in a commercially possible manner, and then try one after the other as filaments of incandescent lamps.

While working on these lines, Dr. von Bolton succeeded, in the first instance, in producing a vanadium filament by heating a mixture of vanadium pentoxide and paraffin to 1700°C. , and thereby producing sticks of vanadium trioxide, which in their turn were heated by electric currents in a glass bulb exhausted by an air-pump, and so converted into metallic filaments.

As it was found that vanadium melts at about 1680°C. , such filaments were no improvement on carbon filaments; and the next substance to be investigated was niobium, which belongs to the same group of elements, but has nearly double the atomic weight.

Treated in a similar manner, the niobium filament gave somewhat better results, but still its melting-point, estimated at 1950°C. , was too low for practical purposes.

In this connection it should not be forgotten that, at a temperature considerably below their melting-point, all these metals begin either to soften or to disintegrate, so that their "working" temperature is not identical with their melting temperature.

Turning his attention to tantalum, which has an atomic weight of 181, Dr. von Bolton experimented with the black metallic powder produced by the method of Berzelius and Rose, and found that it could be rolled into a fairly coherent mass in the form of ribbons.

Alternative experiments, conducted on the lines by which vanadium and niobium had been obtained, resulted in the production of pure tantalum in the form of a metallic button, which was found to be tough and malleable like steel.

These and other qualities convinced Dr. von Bolton that nobody before him had handled pure tantalum, although Berzelius had first obtained the metal by a chemical process in 1824, and later Moissan succeeded, in 1902, in producing it in his electric furnace.

The latter describes tantalum as a hard brittle metal of the specific gravity of 12.8 and a non-conductor of electricity, but he adds that the substance obtained by him contained about a half per cent. of carbon.

Considering the high atomic weight of tantalum, this admixture of carbon evidently exercises a great influence on the physical qualities of tantalum, and explains the differences between the observations of Dr. von Bolton and those of his predecessors.

In nature ores containing tantalum are found in many places, principally in Scandinavia, North America, South-west Africa, and Western Australia.

A specimen of columbite from South Dakota and another of tantalite from Western Australia are exhibited.

The columbite contains from 10 to 40 per cent. of tantalum pentoxide (Ta_2O_5), and a good deal of niobium combined with iron and manganese in various proportions.

As the separation of tantalum and niobium is somewhat troublesome, it is preferable to utilise the tantalite, which consists almost entirely of iron and manganese combined with tantalum pentoxide.

From these ores tantalum is separated in the form of a fluoride in combination with potassium (K_2TaF_7), and subsequently reduced by metallic potassium to the black powder already mentioned, which, however, still contains some oxide and some hydrogen.

In order further to purify the product, the powder is pressed into the form of small cylinders (one of which is on the table), which are melted in a vacuum by an electric current under certain precautions into small buttons of pure tantalum such as are exhibited.

Since the production of tantalum has been carried out on a com-

mercial scale, it has been possible to improve many details of the process, so that the tantalum produced by it at the present time is even purer than that shown in 1905 at the discourse of Dr. von Bolton and Dr. Feuerlein.

Some specimens of this latest tantalum have been submitted to Sir James Dewar, who has very kindly made experiments with reference to its specific heat and to its thermal conductivity.

He ascertained the specific heat by plunging small spheres of tantalum, which had been heated to the temperature of boiling water, into water of 14°C. , then transferring them to melting carbonic acid (-78°C.), and finally to liquid air (-183°C.), and as an average of several experiments the specific heat was found to be between

$$\begin{array}{rcl} 100^{\circ}\text{C. and} & 14^{\circ}\text{C.} & = 0.033 \\ 14^{\circ}\text{C.} & \text{,,} & -78^{\circ}\text{C.} = 0.032 \\ -78^{\circ}\text{C.} & \text{,,} & -183^{\circ}\text{C.} = 0.028 \end{array}$$

while Dr. von Bolton in 1905 gave the specific heat as 0.0363.

Multiplying these results by the atomic weight (181) it will be seen that Dr. von Bolton's value (6.57) is slightly higher, and Sir James Dewar's value (5.97) lower than 6.4, which, according to Dulong and Pettit is the atomic specific heat.

By the kindness of Sir James, his experiment for showing the relative thermal conductivity of iron, copper and tantalum can be repeated here by dropping three short rods, made of these metals, in liquid air, while their tops, above the cardboard cover, are exposed to the air of the room.

In a short time the moisture of the air condenses on the rods and freezes to a distance, which depends on the conductivity of each metal.

The results of Sir James Dewar's experiments prove tantalum to have about three-quarters the conductivity of iron, and about one-eighth the conductivity of copper.

At ordinary temperatures, say below 300°C. , pure tantalum resists the action of all acids, except fluoric acid, of all alkalies, and of moisture, so that it is an ideal material for chemical apparatus which do not require high temperatures, and for any implements which, when made of steel, are liable to rust.

It has already been stated that pure tantalum is rough and malleable, so that it can be hammered out into thin sheets or drawn into fine wire, the diameter of the filament wire being 0.03 mm. or about one-eight-hundredth of an inch; all the same, it is elastic and as hard as soft steel, and has a tensile strength of 93 kg. per square mm., which is equal to 57 tons per square inch.

This means that the filament wire is capable of supporting about 80 grammes, or 2.8 ounces, as can be shown by actual experiment.

Tantalum sheet can be stamped into various shapes, and out of

bars of tantalum springs can be bent, as the specimens on the table show.

Another use made of tantalum is as material for writing pens, manufactured in the usual way.

When it was first offered for this purpose, it was found that the material could not pass the test prescribed for pens made of steel.

These are pressed by a weight of 180 grammes on writing paper, which is moving at the same speed as ordinary writing, and while 10 km. ($6\frac{1}{4}$ miles) of paper are passing, the loss by abrasion must not exceed 0.7 mg. (0.01 grains).

At first the tantalum pens lost more than double the permitted weight, but it was found that slightly oxidising the surface of the pens hardens them so much, that they only lose 0.8 mg. by the 10 km. test.

By weight this is still more than permitted for steel pens, but having regard to the specific weights of the two substances, the actual volumetric abrasion of the tantalum pen is the lesser of the two.

Although only the surface of the pens had been oxidised, it was found that the rate of abrasion remained the same for the whole length of 10 km., when it was expected that this rate would increase materially after the skin of oxide had been ground off.

Advantage was taken of this circumstance when an inquiry was received from India whether it would be possible to manufacture cataract knives for oculists out of tantalum.

The qualities demanded of such a knife are that its blade should be—

1. Intensely hard, so as to be able to acquire a very sharp edge of great smoothness, and to retain this fine edge for a long time.
2. Very tough without any tendency to bend.
3. Chemically and mechanically stable, so that it can be easily sterilised and that it is not liable to rust.
4. Capable of acquiring a high polish.

Manufacturing such a blade out of pure tantalum and slightly oxidising it, before polishing it, appears to fulfil these stringent conditions, but as the knife which is on the table has not yet been actually tried for an operation, it can only serve to demonstrate the similarity of tantalum to steel for such purposes.

Another field for the application of tantalum may be found in the supply of dental instruments, owing to its immunity from chemical changes, but beyond showing two cases of such appliances, there is no necessity to go further into details.

While possessing all these qualities of a true metal, tantalum has some others which rather limit its usefulness.

When heated to a dull red it absorbs gases greedily, especially hydrogen and nitrogen, and by combining with them it loses its tensile strength and becomes brittle.

Here are three pieces of tantalum wire taken from the same coil ;

one of them has been heated in an atmosphere of nitrogen, the other in hydrogen, and the third has not been interfered with. The consequence is that the latter has retained its strength, while the former have become brittle and useless.

On heating tantalum in air, it shows first a yellow and then a blue tint like steel, but when the heating is continued it burns to pentoxide.

The black powder and thin wires can even be lighted by applying a match to them, as the experiment shows.

Its melting-point, in vacuo, lies between 2250°C. and $2300^{\circ}\text{Celsius}$, which makes it particularly suitable for electrodes in vacuum-tubes, especially as it does not disintegrate—for example, Ta electrodes are extensively used in Roentgen tubes—and its specific weight is 16.6 .

Turning now to the electrical qualities of tantalum, its specific resistance was stated by Dr. von Bolton, in 1905, to be on the average 0.165 , with a temperature coefficient of 3 per cent. between 0° and $100^{\circ}\text{Celsius}$.

Further experiments conducted by Dr. Pirani in the laboratory of Siemens and Halske revealed the fact that wires of various thicknesses varied in their specific resistance from 0.173 to 0.188 ; but after they had been heated to $1900^{\circ}\text{Celsius}$ in a high vacuum for from 100 to 200 hours, they all possessed the same specific resistance, viz.: 0.146 , and their temperature coefficient between 0° and $100^{\circ}\text{Celsius}$ had risen to 0.33 per cent.

As a temperature of the tantalum filament, when consuming 1.5 watt per candle-power, is about 1850°C. , and its resistance about six times its resistance at 100°C. , the temperature coefficient between 100° and 1850°C. may be taken, on the average, as 0.29 per cent.

No doubt the difference between these results is caused by alterations in the structure of the wires during their manufacture, and the heating in vacuo served a similar purpose to the annealing of steel, so that Dr. Pirani's results published in 1907 may be taken as standards.

At present, the most important industrial application of tantalum is its use for the filaments of incandescent lamps, which may be said to date from July 1903, when Dr. Feuerlein had succeeded in producing a tantalum wire one-twentieth of a millimetre in diameter. Of this wire he made a glow lamp with a filament 54 mm. long, using a current of 9 volts 0.58 amps., and giving a light of 3.5 candles (Hefner), at the rate of 1.5 watt per candle power.

A simple calculation shows that for a current of 110 volts, 660 mm. of the same wire would be required giving at the same rate of consumption of energy a light of 43 candles.

In carbon lamps for 220 volts the length of filament is only 400 mm., and the filaments remain hard until they disintegrate. Tantalum filaments, like other metallic filaments, soften, however, to such a degree that they cannot be used in the same shape as carbon filaments.

After trying various methods of housing the long Ta filament in a glass bulb of approximately the same dimensions as the carbon glow lamps, the present form was arrived at during the year 1904. In this lamp, which was adopted as standard, the length of the filament was 650 mm., its diameter 0.05 mm., and its weight 0.022 grammes, so that about 45,000 of these lamps contain 1 kg. of Ta.

Since then these dimensions have been modified to a certain extent, for instance the diameter of the filament is now only 0.03 mm., but the external shape has not been altered.

It was soon found that after burning a short time the filament underwent certain structural changes and lost its great tensile strength. Examination under a microscope revealed the fact that in about 1000 hours the smooth cylindrical filament shows signs of capillary contraction, as if the cylinder was going to break up into a series of drops, and the surface, from being dull, commences to glitter. This contraction of the filament after being heated is readily recognised by comparing a new lamp with an old one. On the stars of the new lamp the filament hangs loosely, while in the old lamp the filament is evidently in tension.

The characteristic difference between carbon filaments and tantalum filaments is shown by a diagram representing the influence of temperature on the electric resistance of the two filaments in proportion to each other.

In order to have the differences at once shown in per cents., the normal pressure and the normal resistance of both filaments, when giving the light of 1 candle for 1.5 watt, is marked as 100, and it is immediately seen that the resistance of Ta alters directly and that of carbon inversely as the temperature. Owing to this quality, a Ta filament is better able to resist overheating than a carbon filament, as the following experiment shows, where two lamps, one Ta and one C, burning normally at 110 volts with 1.5 watt per candle power, are gradually exposed to higher voltages. The C lamp breaks while the Ta lamp stands up to 200 volts, the highest voltage available to-night. Of course its useful life will be shorter than at its normal voltage.

As stated at the beginning of the discourse, the primary object of all the research was to find a filament more economical in the consumption of electrical energy than the C filament, and the following experiments will show that the Ta filament is, in this respect, a great improvement on the C filament.

To begin with, a comparison can be made by burning a Ta and a C lamp under water, each being immersed in a vessel containing the same quantity of water.

Owing to the C lamp requiring more energy to give the same light as the Ta lamp, the temperature of the water in the C vessel rises quicker than in the other vessel.

Another way of showing the difference is by measuring the current taken by each of the two lamps when giving approximately the same

light, or by sending the same current through both lamps in series and noting the difference in candle power.

In conclusion, two interesting qualities of Ta should be noted—

The first is that when a Ta filament is heated in a high vacuum it will expel any oxygen that has combined with it. It is possible to detect whether a filament contains any oxide by very gradually heating it up, when the parts containing oxide will appear brighter than those consisting of pure Ta, owing to the greater electrical resistance of the oxide.

These lamps have been purposely exposed to the air while they were being exhausted and have become “spotty” in consequence, but if they are raised a little above their proper voltage and left burning for a few minutes, their filaments become quite uniform by the expulsion of the oxygen.

The second is that Ta will act as a rectifier when used in an electrolyte, that is to say, it will allow of the passage of the positive current only in one direction. In the apparatus shown the positive current passes through the lamp to a Ta anode, thence to a Pt cathode, but in a very short time the Ta anode covers itself with a film of oxide which stops the current. When the current is reversed the lamp lights again and continues to burn. When an alternate current is connected to the lamp it will also continue to burn, but with reduced light.

All these experiments are intended to show the remarkable qualities of this material, and when they are fully appreciated and its limitations are properly understood, there appears to be a great field open to tantalum and its industrial applications.

[A. S.]

WEEKLY EVENING MEETING,

Friday, April 30, 1909.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S.,
Treasurer and Vice-President, in the Chair.EDMUND GOSSE, ESQ., M.A. LL.D., Librarian to the
House of Lords.*The Pitfalls of Biography.*

[ABSTRACT.]

THE first consideration in the writing of biography, after that which is involved in "the strict and scrupulous veracity" of which Boswell boasted, is that of obtaining a characteristic portrait of the subject within such proportions as are exactly suitable to its value and size. The element of discretion enters largely into this matter, and it is necessary to consider with unusual care what biographical discretion really is, and how far its dictates are to be obeyed. There are, however, several definitions of "discretion" given by the best lexicographers, and the man who does not dare to take the balance of those meanings into his own hands, and to decide by the light of his own sense of decorum, what should and what should not be printed, is liable to prove himself an indiscreet biographer, in what he timidly omits no less than what he ventures to say. The art or practice of biography defined. In early times, this art was universally misconceived, and it is even now necessary to insist upon what it is *not*. A biography is not a philosophical treatise, nor a sermon in religion or morals. The celebration of virtuous and dignified qualities was in earlier ages the sole aim of a biographer, and not the faithful portrait of the man, in his habit as he lived. There is a curiosity which is now recognised as being legitimate, and this is satisfied almost to excess in the best biography which exists in any language, the "Life of Dr. Samuel Johnson." This satisfaction of curiosity presupposes an observation of life which is not often possessed by those whose vision is clouded by moral passion or social prejudice. Hence there is always proceeding, in the field of biography, a struggle between those who wish to instruct and those who wish to amuse. One of the crying faults of most modern "Lives" is their unwieldy length, and this, strange to say, seems mainly due to the haste of their authors and lack of leisurely care expended on their preparation. The biographer is in too great a hurry to spare time in winnowing his material, so, as a concession to breathless haste, the documents are flung together in a rough heap, without

selection or arrangement. In considering what details should be retained and what dismissed, the biographer shows whether he is actuated by sound or unsound Boswellism, that is to say, whether he uses detail recklessly, or whether he selects only what will illustrate the career or the character of his subject in a vital degree. One great cause of difference of opinion on the subject of the limits and direction of biographical portraiture is the radical hostility of the two groups of people for whose behoof each portrait is made. Besides the majority, whose curiosity is to be entertained and whose legitimate interest is to be stimulated, a respectable minority will always be found whose object is to curtail intimate revelations, and to defy all curiosity so far as it is possible to do so. We must, therefore, accept a compromise. The biographer must start prepared to meet with opposition of a legitimate and natural kind, but he should determine to reveal as much as may, without want of decorum, be revealed. Indeed, we must venture to say that the first theoretical object of the biographer should be indiscretion, not discretion. A conviction of the uniformity of human character is one main cause of dull and false biography. A man is labelled "good" or "great," and he is presented to us as great or good all through and upon every side. Another dangerous pitfall is the habit of concealing with scrupulous inexactitude facts and conditions which were not admirable, but which were essential, and left a lasting mark on the character and the career of a man. Those are often made the subject of direct, although pious, falsehood. The moral aspect in biography is involved in difficulty, because each individual instance needs a law unto itself. But the proper attitude to adopt in considering the treatment of a body of personal history is to be tactful, but not cowardly; to cultivate delicacy, but to avoid its ridiculous parody, false delicacy. There should be no unnecessary pandering to the snobbishness, weakness, or blindness of survivors. If the portrait is to be painted at all, it must be true, and the biographer must not be unduly alarmed, if some people call him indiscreet.

[E. G.]

ANNUAL MEETING,

Saturday, May 1, 1909.

THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S.,
President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1908, testifying to the continued prosperity and efficient management of the Institution, was read and adopted, and the Report on the Davy Faraday Research Laboratory of the Royal Institution, which accompanied it, was also read.

Forty-eight new Members were elected in 1908.

Sixty-three Lectures and Nineteen Evening Discourses were delivered in 1908.

The Books and Pamphlets presented in 1908 amounted to about 228 volumes, making, with 708 volumes (including Periodicals bound) purchased by the Managers, a total of 936 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year :—

PRESIDENT—The Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S.

TREASURER—Sir James Crichton-Browne, M.D. LL.D. F.R.S.

SECRETARY—Sir William Crookes, D.Sc. F.R.S.

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The Rt. Hon. Sir John Fletcher Moulton,
P.C. M.A. F.R.S.

Sir Andrew Noble, Bart., K.C.B. D.C.L.
D.Sc. F.R.S.

The Hon. Lionel Walter Rothschild, M.P.

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F.R.C.S.

Major-General Sir Coleridge Grove, K.C.B.
Charles Edward Groves, Esq., F.R.S.

John List, Esq., M.Inst.C.E.

Sir Philip Magnus, M.P. J.P. B.Sc.

Robert Mond, Esq., M.A. F.R.S.E.

Colonel Sir Frederick Nathan, R.A.

The Hon. Charles A. Parsons, C.B. J.P.
M.A. LL.D. D.Sc. F.R.S.

James Swinburne, Esq., F.R.S.

Arthur James Walter, K.C. LL.B.

GENERAL MONTHLY MEETING,

Monday, May 3, 1909.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S.,
President, in the Chair.

The Honorary Secretary announced that His Grace The President had nominated the following gentlemen as Vice-Presidents for the ensuing year :—

Donald William Charles Hood, Esq., C.V.O. M.D. F.R.C.P.
Alfred Bray Kempe, Esq., M.A. D.C.L. Treas.R.S.
Sir Francis Henry Laking, Bart., G.C.V.O. M.D. LL.D.
George Matthey, Esq., F.R.S.
The Right Hon. Sir John Fletcher Moulton, P.C. M.A. F.R.S.
Sir Andrew Noble, Bart., K.C.B. D.C.L. D.Sc. F.R.S.
Sir James Crichton-Browne, M.D. LL.D. F.R.S. (*Treasurer*).
Sir William Crookes, D.Sc. For.Sec.R.S. (*Honorary Secretary*).

Horace Stansfield Collier, Esq., F.R.C.S.
James Benson Kennedy, Esq.
Henry Alexander Miers, Esq., M.A. D.Sc. F.R.S.
Thomas Packard Warren, Esq.
Sir Almroth Edward Wright, M.D. Sc.D. F.R.S.

were elected Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

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- Cambridge Philosophical Society*—Transactions, Vol. XXI. No. 7. 4to. 1909.
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- Chemical Industry, Society of*—Journal, Vol. XXVIII. Nos. 7-8. 8vo. 1909.
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- Editors*—Aeronautical Journal for April, 1909. 8vo.
- Agricultural Economist for April-May, 1909. 4to.
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WEEKLY EVENING MEETING,

Friday, May 7, 1909.

SIR FRANCIS LAKING, Bart. G.C.V.O. M.D. LL.D.
Vice-President, in the Chair.

MAJOR RONALD ROSS, C.B. LL.D. D.Sc. F.R.S. F.R.C.S.
Nobel Laureate.

The Campaign against Malaria.

MORE than nine years ago I had the privilege of addressing the Royal Institution* on the subject of my researches on the mode of infection in malarial fever; and I am now called upon to describe what has been done, or not done, in various countries to utilise for the alleviation of the disease the information then obtained.

The ancients appear to have recognised, not only the principal symptoms of malarial fever, but the fact that it is often connected with marshes: and more recently many authors ascribed this fact to the existence of poisonous vapours which they supposed are given off by stagnant waters, or even by the soil. Still later, a series of pathological studies led to the discovery by Laveran in 1880 that the malady is produced by vast numbers of minute protozoal parasites of the red blood-corpuscles; and students of the subject now conjectured that these organisms originally inhabited the marshes, and infect man through air or drinking water. My own studies, however, commenced eighteen years ago, and confirmed and extended by many workers, showed that the parasites are carried from man to man by certain species of Culicidæ (gnats or mosquitoes); and that it is these carrying agents, and not the parasites themselves, which live in the marshes. Thus malarial fever was now proved to be merely a parasitic disease, the infection of which is carried from man to man by the agency of certain water-breeding insects.

As described in my previous lecture, the broad principles of this theorem were really fully established by the end of the year 1898. Although numerous minor details still required study—such as the precise species of mosquitoes which carry the infection in various countries, the exact habits of each species, and so on—yet I held that these questions could now be elucidated without difficulty in the ordinary course of work, and that we were already in a position to apply the discovery at once to the saving of human health and life. I propose, therefore, to take up the story again from this point.

* March 2, 1900.

First let me emphasise the great importance of this practical side of the subject. Malarial fever is spread over nearly the whole of the tropics, abounds in many temperate climates, and has been known to extend as far north as Sweden. In vast tracts of tropical Africa, Asia, America, and of Southern Europe, almost every town and village is infested by it; millions of children suffer from it from birth to puberty; and native adults, though they tend to become partially immune, still remain subject to attacks of it. Although it is not often directly fatal, yet it is so extremely prevalent, so endemic in locality, so persistent in the individual, that the total bulk of misery caused by it is quite incalculable. More than this, its special predilection for the most fertile areas renders it economically a most disastrous enemy to mankind. Throughout tropical life, it thwarts the traveller, the missionary, the planter, the soldier, and the administrator. From one-quarter to one-half the total admissions into military hospitals are returned as being due to it; and it is often the most formidable foe which military expeditions have to encounter. There are reasons for thinking that it indirectly increases the general death-rate of malarious countries by something like fifty per cent.; and I venture to say that it has profoundly modified the history of mankind by doing more than anything else to hamper the work of civilisation in the tropics. Only those who have studied the disease from house to house, from village to village, can form any true notion of the total effect which it must produce throughout the world.

Next let us recall briefly the various methods which we possess for preventing and reducing the disease. The oldest of these—known to us since the time of the Romans—is *drainage of the soil*. The reason why it succeeds became quite obvious after 1898—because it tends to remove the terrestrial pools and marshes in which the Anophelines, that is, the family of mosquitoes which carry malaria, breed. But the new discoveries not only explained the old method, but also rendered it more simple, cheap, and yet precise, by showing us exactly what waters, namely, those in which the larvæ of the Anophelines actually occur, are to be drained away, or filled up, or otherwise treated. But science has given us other methods as well. Thus we have known for a long time that *quinine* is a preventive as well as a cure—that if, for example, a body of men are given quinine with regularity they will suffer less from fever in consequence. Still further, the old saying that the use of *mosquito-nets* at night will keep off malaria was now fully justified—not because the nets exclude any aerial poison, but simply because they exclude the infecting insects. This simple precaution can moreover be extended by protecting all the windows of a house by *wire-gauze*, as already frequently done in the Southern States of America. *Punkas* and *electric-fans* also serve to keep away the insects; and, lastly, *segregation* of Europeans from native quarters, as used so largely in India, will help to keep them

from mosquitoes infected by native children (who suffer so frequently from the disease). It was thus apparent that if the inhabitants of malarious countries could be persuaded to protect themselves by mosquito-nets or quinine, or if the governments of such countries could be persuaded to undertake suitable drainage and other measures against mosquitoes, much improvement in the public health was likely to accrue.

But how precisely was such persuasion to be undertaken? Of course, I do not allude to utterly barbarous peoples, to areas far beyond the influence of civilisation—which are happily shrinking in magnitude every day. I allude to independent or dependent states professing themselves civilised, and to the numerous colonies of the great civilised nations. Here we already possess the requisite machinery. Such states or colonies are administered by governors and councils, and for the most part possess medical and sanitary departments controlled by well-paid officials whose special duty it is to attend to such affairs. Many dependencies, moreover, such as some of those of Britain, are placed under the central government of the nation concerned, and can be influenced by it. It might be supposed, then, that at the period referred to, all such administrations would have gladly interested themselves in the prevention of a disease which produces so much mischief, and of which the cause had been so clearly elucidated; that they would at once have set about collecting preliminary information and commencing at least some experimental trials. So far as I can see there is no real reason why this was not done everywhere nearly ten years ago.

Unfortunately, though science may provide us with facts, humanity is slow to credit them, and still more slow to take advantage of them. History is full of examples of this. For instance, years elapsed before the discovery of Jenner was fully utilised—it is not fully utilised even yet. Another instance, closely connected with malaria is that of filiaris, a parasitic disease of which elephantiasis is one manifestation. More than thirty years ago very good evidence was given to show that it is carried by mosquitoes: and, considering the horrible and widespread deformities which it produces, one would have thought that strong efforts would have quickly been made to control it by reducing the carrying agents. So far as I can ascertain, however, scarcely anything has yet been even attempted against it. No one has interested himself seriously in the matter, and consequently nothing has been done.

It was therefore early apparent to me that, though the machinery for extensive antimalarial work existed in many countries, yet it would not easily be got to work unless someone could be found who would devote himself to the task—neither a pleasant nor a profitable one—of urging it forward, and I felt that the duty devolved on myself in the absence of others, as regards British territory. Happily Angelo Celli and Robert Koch occupied themselves similarly as re-

gards Italy and Germany ; and the creation of the Schools of Tropical Medicine in Liverpool and London in 1899 did much to popularise the recent discoveries. At my inaugural lecture the same year at the former institution, I described my proposals for the prevention of malaria by mosquito-reduction ; and a few months later, accompanied by Dr. H. E. Annett and Mr. E. E. Austen, I left England for Sierra Leone in order to perfect the details.

Sierra Leone is a small British colony long notorious for its extreme unhealthiness. We determined rapidly the malaria-bearing species of Anophelines there, and their breeding places and habits ; and drew up a series of proposals for their reduction. These have since become the basis of similar work elsewhere ; but simple as they were, we could not get the local authorities to understand them or act upon them. Two years later, I again twice visited the Colony, and, assisted by Dr. Logan Taylor and a sum of money presented to me for the purpose by a private gentleman, attempted to give an object-lesson on the subject. Though the result was successful at the time, we again failed in inducing the authorities to take up the work properly ; and I can obtain no adequate information as to what has been done there during the last seven years, and may perhaps be excused for not wishing to inquire.

In the meantime the Liverpool School of Tropical Medicine and the Royal Society had sent a series of expeditions to West Africa, which did much good work there. As a consequence, Sir William MacGregor, Governor of Lagos, and one of the most enlightened of British administrators, took up the task in that colony with great intelligence and energy, but unfortunately was shortly forced to leave by ill-health—a serious blow to anti-malarial work throughout the world. From that time, though much appears to have been done by energetic individuals in West Africa, and though, to judge from popular statements, public health has been decidedly improved there, yet the official reports and returns are too inadequate to enable us to form any reliable opinion of the results. The recent statements of Professor Simpson on the subject are not encouraging, and to my mind, judging from many facts known to me, the sanitary administration of the West African colonies has been generally wanting in leadership and organisation, and the campaign against malaria has been constantly thwarted by administrative indifference and professional jealousy.

Turning elsewhere, I must now mention with great pleasure the early and successful campaign of Koch at Stephenson, in New Guinea. The method of Koch does not depend on mosquito reduction, but on the detection and treatment of cases of malaria by quinine, until they cease to spread the disease among their healthy neighbours. It is allied to the similar method used in other diseases ; has been successfully followed in the German colonies and in Italy ; and will always be a valuable weapon in the antimalarial armoury. The great work

of Celli and the Italian Anti-malaria Society, commenced early in 1899, has been based on the same, but also on a wider principle of distribution of quinine, together with mechanical protection from mosquito bites. Working onward step by step against political and local indifference, they have gradually made, during the last ten years, a great reduction in the amount of the disease throughout Italy. An independent witness, Professor Osler, has recently written as follows to the Times: "In Professor Celli's lecture-room hangs the mortality chart of Italy for the past twenty years. In 1887 malaria ranked with tuberculosis, pneumonia, and the intestinal disorders of children as one of the great infections, killing in that year 21,033 persons. The chart shows a gradual reduction in the death-rate, and in 1906 only 4871 persons died of the disease, and in 1907, 4160." I should be unable to hang a similar chart for British possessions in my lecture room.

In 1900-01, a great discovery, closely connected with our subject, was made by the Americans in Havana—I mean the discovery that yellow fever, the scourge of tropical America, is also carried by mosquitoes of the kind called *Stegomyia*. With the Americans, however, there was no delay in turning this fact to practical account, and under General Wood and Colonel Gorgas they got rid of the disease from that large city in a few months. Since then, Colonel Gorgas has been conducting the magnificent sanitary work of the Americans in the Panama canal zone—work the success of which is too well known to require illustration by figures, but which has enabled the Americans to do what the French, before the date of these discoveries, failed in doing, namely to continue the construction of the canal. It is not too much to say that the canal is being made with the microscope. Colonel Gorgas has repeatedly stated that the measure upon which he principally relies, against both yellow fever and malaria, is the general reduction of mosquitoes.

For three years my original proposals to remove malaria by this means had not been thoroughly and formally applied by any government; but I have now to record the first classical successes obtained by it in Ismailia and in the Federated Malay States. The former is a town founded by Ferdinand de Lesseps on the Suez Canal. For many years it has suffered extremely from malaria, the cases amounting ultimately to about 2000 a year among a small population. In 1902 I was asked by Prince Auguste d'Arenberg to advise on the matter; and my advice was acted upon loyally and intelligently by his officers in the town. The result was that the cases fell to 214 next year, and to 90 in 1904, and that since then there has been no endemic malaria in the town at all, while mosquitoes of all kinds have been practically banished from it. The work in the two small towns of Klang and Port Swettenham in the Federated Malay States was begun about the same time, chiefly by Dr. Malcolm Watson, under the orders of the Government, and of Dr. E. A. O.

Travers, and has been equally successful. No one who has studied the facts published with regard to both of these campaigns can for a moment deny the success obtained.

Since then excellent campaigns on similar lines have been conducted at Durban, Hong Kong, Khartoum, Candia and St. Lucia. Most striking has been the anti-mosquito work conducted at Port Said under the orders of Sir Horace Pinching, recently head of the Egyptian Sanitary Service, by my brother, Mr. E. H. Ross. The town has been so completely cleared of mosquitoes that, as at Ismailia, the ladies no longer use mosquito nets for their children. I may add that I have just recently visited both localities, and was able to verify this statement by conversations with a number of people. Fuller accounts of some of these campaigns will be found in a paper by me published in the *Lancet* of September 28, 1907. Excellent and extensive work has been done for many years in Algeria by Drs. Edmond and Etienne Sergeant* by all methods, and by Drs. Savan and Kardamatis and the Greek Anti-malaria League.† The Italian work published is in the *Atti della Societa per gli studi della malaria*; and Dr. Laveran gives much information on the subject in his last book on malaria, *Du Paludisme*, 1907.

Two years ago I was asked by the Government of Mauritius to advise regarding malaria in that ancient island colony. The War Office associated Major C. E. P. Fowler, R.A.M.C., with me; and after three months' studies, warmly assisted by the Governor, the officials, the planters and everyone, we drew up our scheme for a general campaign against the disease. There is no doubt that this scheme will be followed when the present financial situation is rectified; but in the meantime I hope and trust that our reports, which were written with great care and have been published by the Colonial Office and the War Office respectively, will prove of value in other parts of the tropics.

When I left India in 1899 I hoped that that great dependency of the British Crown, with its powerful government and well-appointed medical and sanitary services, would lead the way against malaria, a disease which causes untold sickness and possibly some millions of deaths annually in the country; but though many local campaigns have been started by individual medical men, and though there has been a steady fall in the malaria rate of the army, I can find no evidence of a generalised effort against the disease. Less than three months ago I attended the Medical Congress at Bombay, largely for the purpose of inquiring into the reason of this, and concluded that though many capable officers both of the Indian Medical Service and of the Royal Army Medical Corps had done their best, yet that the necessary leadership and organisation were wanting in India as in

* *Annales de l'Institut Pasteur*, 1909.

† *Annals of Tropical Medicine and Parasitology*, Liverpool, June 1908.

West Africa. An ill-judged and ill-conducted experiment at Mian Mir has done much to paralyse all efforts in this direction, and I gathered that anti-malarial campaigns were not popular among certain officials. Neither the Indian Government nor the Medical Services can be congratulated on the result.

Some years ago the Secretary of State for the Colonies issued a circular to the Governors of Crown Colonies asking for information as to what had been done in each against malaria and other mosquito-borne diseases, and statements on the matter from twenty-one Colonies were published in the Report of the Advisory Committee of the Tropical Diseases Research Fund for 1907. I have criticised these statements in detail elsewhere. Only those furnished by seven Colonies, namely, Southern Rhodesia, Papua, Mauritius, British Central Africa, Gambia, Ceylon, and Southern Nigeria, showed evidence of any real interest in the matter. Those from Bahamas, Barbadoes, Jamaica and St. Kitts-Nevis showed, to my mind, nothing but neglect of public duty, while those from Northern Nigeria, St. Lucia, British Honduras, Grenada, Somaliland, Straits Settlements, and Sierra Leone gave no decisive evidence of the result.

For a number of years I have had very good opportunities of learning the truth as to what is really being done in many of these, and other, dependencies. It may generally be summed up in two words—very little. Festering pools which might have been cleared years ago for a few shillings or pounds are left in the heart of important towns to poison all around them; quinine prophylaxis is neglected, and house-screening forgotten. Few efforts are made even to estimate the local distribution of the disease, much less to organise any serious efforts against it, although it may be causing, perhaps, half the sickness in the place.

Want of funds is always an excuse which is urged, and is always a false excuse. Much can be done at almost no expense, and the men who have actually carried out the work successfully in Panama, Ismailia, the Federated Malay States and Italy have expressly declared the cheapness of it. Many a town could be kept clear of malaria for the amount, say, of the salary of a single European official. I estimate that a sixth of the medical and sanitary budget should generally suffice to reduce a disease which often causes half the sickness. But instead of doing really useful work which would benefit everyone, the authorities too often fritter away their funds on trifling schemes. I maintain that the health of the people has the first claim on the public purse.

Another excuse is that the possibility of preventing malaria has not been proved; but when one questions the sceptics one generally finds that they have not troubled to study the literature.

The fact is, that the neglect of which I complain is due to quite other causes. I do not think that, as a rule, the blame is to be attached to the rank and file of the medical profession in the tropics. Men on

the clinical side generally have enough to do with their hospitals and medical practice ; while those on the sanitary side frequently complain that their recommendations are not seriously attended to. The immediate responsibility lies with the heads of the sanitary services of the colonies—men who are specially paid to organise such work. Now, though many capable individuals are to be found in such medical services, there is always a percentage of men in them, as in other services, who, to be frank, are not at all capable ; men who from the date of receiving their medical qualifications, take no further real interest in their work, read no literature, undergo no further courses of instruction, undertake no scientific researches, and make no addition to our knowledge, either of medicine or of sanitation, and yet who manage to obtain the highest medical or sanitary appointments, either by seniority or by the well-known arts of self-service and wire-pulling. I am sorry to have to express such an opinion, but I think that this type of person is much too common in all branches of British administration. Worse heads of departments cannot be found. They scoff at the knowledge and efforts of others in order to cover their own ignorance and apathy. To them all new discoveries are frauds, and all new proposals are charlatanism. They repress every kind of honest endeavour among their juniors ; they fill the best appointments with their own friends ; and they truckle to their official superiors in the hope of obtaining further preferment. At last, decorated and pensioned, they leave the field to others of their own stamp—men without an idea or an ideal, except such as refer to their own advancement. These are the persons who are really responsible for the state of things which I have described.

As a rule Colonial governments are far too careless in the selection of the men to whom they entrust the health of the public. It is openly said that they often either choose mediocrities or men who they know will be too subservient to them to assert the demands of sanitation—which is never a popular theme. At home, no one may be the medical officer of health, even of an English village, without possessing a proper diploma entitling him to practise as such ; but it appears that anyone is good enough to be the chief sanitary officer of a whole country. The most amazing appointments are often made. Men of known and approved ability are passed over in favour of others who are supposed to possess special administrative qualifications—which frequently means nothing but a capacity for self-advancement ; and both senior and junior sanitary officers complain that their representations regarding anti-malaria work often receive no intelligent attention, either from the civil or the military authorities.

Root and branch reforms are required in all these respects. The failure of most of our tropical dependencies, during ten long years, to understand and act upon modern discoveries in connection with malaria, and indeed with other diseases, demonstrates that their sanitary services no longer fulfil the purpose for which they are paid and

appointed. Reconstruction, similar to that which has revived the Royal Army Medical Corps, is urgently demanded. It is not too much to ask that no man shall be appointed to an administrative post without previous examination as to his fitness—that no man shall be entrusted with the post of chief sanitary officer unless he can show evidence of having really worked at the subject, of having mastered scientific details, and of having obtained the qualifying diplomas of Public Health and Tropical Medicine. He should be placed on the executive council—which is now so frequently managed by the heads of less important departments. Proper arrangements should be made for expert inspection and supervision; and much more science, work and discipline should be demanded, not only in the services but in those who control them.

I have now outlined the general course of events. The immediate success which we had hoped for ten years ago has not been attained. The battle still rages along the whole line; but it is no longer a battle against malaria. Malaria we know, we understand fully, we can beat down when we please. The battle which we are now fighting is against human stupidity. Those of us who have taken part in it—not too numerous—know what it has been. We have written and lectured *ad nauseum*; we have interviewed ministers, members of Parliament and governors; we have appealed to learned societies; we have sought the support of distinguished people, and we have received—sympathy. We have reasoned, and been ridiculed; we have given the most stringent experimental proofs, and been disbelieved; we have protested, and been called charlatans. I think that not one of those young men who have pioneered this important work in the field has ever received thanks for his labours. On the other hand, I know of several who have been actually punished for it. An example which I am free to mention is that of my brother, Mr. H. C. Ross, who was driven from the Egyptian Sanitary Service out of spite and jealousy simply because he undertook such work. I know that all new movements have to face opposition of this kind; but surely the world is becoming too old for it. We talk much of science, and collect funds for research and teaching, and hold conferences and congresses, and blow trumpets over our doings; but when a useful discovery really is made, when the cause and methods of prevention of the most important of human diseases have been discovered, taught and tried for ten years, this is the way we employ it for the good of humanity! Of what use is it to make discoveries, if when they are made they are neglected? And remember that all this time, while we are questioning facts that are proved and methods that are established, hundreds of thousands, nay millions, of poor people are suffering from our dullness. I conclude with an appeal. The matter must be taken up in Parliament and in the press, as vigorously as possible. If some of the officials at fault could be persuaded to accept their pensions and decorations before the usual time, room

might be made for more capable men. The few persons who have fought the fight and failed are scarcely able to continue it. If no stronger influences can be exerted, the future of malaria prevention in British Dominions will certainly be as barren as the past has been. Our only hope lies in persuading the home Government to urge the governments of our malarious possessions to more determined efforts.

[R. R.]

WEEKLY EVENING MEETING,

Friday, May 14, 1909.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C.
D.C.L. LL.D. F.R.S., President, in the Chair.

PROFESSOR GEORGE E. HALE, LL.D. Sc.D. For. Mem. R.S.,
Director of the Mount Wilson Solar Observatory of the
Carnegie Institution of Washington.

Solar Vortices and Magnetic Fields.

I HEARTILY appreciate the privilege of describing in this lecture-room some of the recent work of the Mount Wilson Solar Observatory. Like so much of the scientific research of the present day, it goes back for its origin to the fundamental investigations of English men of science. The spectroheliograph, which tells us of the existence of solar vortices, is a natural outcome of the application of the spectro-scope in astronomy, where Englishmen were foremost among the pioneers. The detection of a magnetic field within these vortices followed directly from Zeeman's beautiful discovery of the influence of magnetism on radiation—a logical extension of the earlier work of Faraday—and from the classic investigations of Crookes and Thomson on the nature of electricity. In reviewing these great advances, investigators in other lands must again and again wonder at the exceptional ability of the English mind to make fundamental discoveries. When these discoveries have been made, it is a comparatively simple matter to utilise them in many departments of science. Americans cannot fail to rejoice that they may share in the traditions of a race which counts among its members the men who have given the Royal Institution its fame.

It is customary to distinguish sharply between the observational and experimental sciences, including astronomy in the former. In physics or chemistry the investigator has the immense advantage of being able to control the conditions under which his observations are made. The astronomer, on the other hand, must be content to observe the phenomena presented to him by the heavenly bodies and interpret them as best he may. I wish to emphasize the fact, however, that the distinction between these two methods of research is not so fundamental as it may at first sight appear. In 1860 a laboratory in which experiments were conducted for the interpretation of astronomical observations was established by Sir William Huggins on Upper Tulse Hill. The advantage of imitating celestial phenomena under laboratory conditions was thus appreciated half a century ago. I shall indicate later how important a part such a

laboratory plays in the work of the Mount Wilson Solar Observatory. I shall also show that in other ways the astronomer may advantageously follow the physicist, particularly in the choice of observational methods, and in the design of instruments of research.

Sun-spots were discovered as soon as Galileo and his contemporaries directed their little telescopes to the sun. In fact, ancient Chinese records indicate that spots of exceptional size had been detected by the naked eye many centuries before. Long after their discovery, the most diverse views were held as to the nature of sun-spots. Sir William Herschel mentioned the uncertainty which had existed prior to his time, remarking that the spots had been variously described as solid bodies revolving about the sun, very near its surface; the smoke of volcanoes: smoke floating on a liquid surface: clouds in the solar atmosphere; the summits of solar mountains, uncovered from time to time by the ebb and flow of a fiery liquid, etc. In Herschel's own view, the spots are to be considered as the opaque body of the sun, seen through openings in the luminous atmosphere which envelops it. Indeed, he considered that the sun should be regarded as the primary planet of our system, and even suggested the probability that it is inhabited. "Whatever fanciful poets might say, in making the sun the abode of blessed spirits, or angry moralists devise, in pointing it out as a fit place for the punishment of the wicked, it does not appear that they had any other foundation for their assertions than mere opinion and vague surmise; but now I think myself authorised, *upon astronomical principles*, to propose the sun as an inhabitable world, and am persuaded that the foregoing observations, with the conclusions I have drawn from them, are fully sufficient to answer every objection that may be made against it."*

Sir John Herschel did not abandon the idea of an opaque solar globe, but suggested that hurricanes or tornadoes might account for the piercing of the two strata of luminous matter which ordinarily conceal this globe. "Such processes cannot be unaccompanied by vorticose, motions, which left to themselves, die away by degrees and dissipate—with this peculiarity, that their lower portions come to rest more speedily than their upper, by reason of the greater resistance below, as well as the remoteness from the point of action, which lies in a higher region, so that their centre (as seen in our water-spouts, which are nothing but small tornadoes) appears to retreat upwards. Now, this agrees perfectly with that which is observed during the obliteration of the solar spots, which appear as if filled in by the collapse of their sides, the penumbra closing in upon the spot, and disappearing after it."

We now know that sun-spots are brighter than the brightest arc

* William Herschel, 'On the Nature and Construction of the Sun and Fixed Stars,' p. 20.

light, and that their apparent darkness is merely the result of contrast with the intensely brilliant surface of the photosphere. We also know that the sun is a gaseous globe, attaining a temperature of about 6000° at its surface, and perhaps millions of degrees at its centre. If we examine a large-scale photograph of a sun-spot we see that it consists of a dark central region, called the umbra, and a surrounding area, decidedly less dark, called the penumbra. The structure of a spot, as this admirable photograph by Janssen shows, is granular, like that of the photosphere. In the penumbra these granulations seem to group themselves more or less radially, as though under the influence of some force directed toward or away from the umbra. Unfortunately, direct photographs of the sun have not yet attained such perfection as to show the most minute details of sun-spots. To appreciate these, we must have recourse to the exquisite drawings of Langley, the truthful quality of which is recognised by every astronomer who has observed sun-spots under favourable conditions. We shall see that the characteristic structure represented by these drawings is repeated, on a far greater scale, in the higher regions of the solar atmosphere disclosed on recent spectroheliograph plates.

Since the time of Sir John Herschel, many astronomers have proposed vortex theories of sun-spots. One of the first of these is the theory of Faye, who supposed the whirling motion to be the direct result of the peculiar law of the sun's rotation. This law was discovered by Carrington, who found from observations of spots near the equator that the sun completes a rotation in about 25 days, while the motion of spots at a latitude of 40° indicated the time of rotation to be nearly two days longer. Thus, as the rotation period increases toward the poles, the photosphere at the northern and southern boundaries of a sun-spot must move at different velocities (assuming the law of the sun's rotation to be the same as that of the spots). This difference in velocity would tend to set up whirling motions, clockwise in the southern hemisphere and counter-clockwise in the northern hemisphere. Sun-spots, in Faye's opinion, are the visible evidences of such whirls.

This theory has had many supporters, but it is now generally agreed that the difference in the rotational velocity of adjoining regions of the photosphere is not nearly sufficient to account for the observed phenomena. Secchi, one of the most assiduous observers of solar phenomena, was strongly opposed to Faye's theory. He pointed out that about 6 per cent. of the spots he observed gave some evidence of cyclonic action, but in the vast majority of cases such forms as Faye's theory seemed to demand were lacking. We nevertheless owe to Secchi a most striking drawing of a sun-spot vortex.

When the spectroheliograph was first systematically applied to solar research in 1892, many rival theories of sun-spots occupied the field. Since the function of this instrument is to photograph the phenomena of the invisible solar atmosphere, it might be hoped that

the results would throw much light on the nature of sun-spots. For many years, however, this hope was not realised. The first monochromatic images of the sun were made with the K line of calcium. If we compare such an image with a direct photograph of the sun, made in the ordinary way, we see that the sun-spots are surrounded and frequently covered by vast clouds of luminous calcium vapour. These attain elevations of several thousand miles above the sun's surface, but they must not be confused with the prominences, which ascend to much higher elevations. When observed at the sun's limb, the bright calcium flocculi, as these luminous clouds are called, are so low, in comparison with the prominences, that they can hardly be detected as elevations. Thus our knowledge of the calcium flocculi must be derived mainly from the study of spectroheliograph plates, which show them in projection on the disk. I must not omit to mention, however, that the calcium vapour rises to the highest parts of the prominences, and that this higher and cooler vapour frequently indicates its presence on spectroheliograph plates in the phenomena of dark flocculi. These are relatively inconspicuous, however, and need not be discussed here.*

It soon appeared that the average photograph of bright calcium flocculi could not be counted upon to indicate the existence of definite streams or currents in the solar atmosphere. In 1903 the hydrogen flocculi were photographed for the first time. By comparing these flocculi with the corresponding calcium flocculi we see that, in general, dark regions on the hydrogen image agree approximately in form with bright regions on the calcium image. This might appear to indicate that hydrogen is absent in the regions where calcium is most abundant. An investigation of the question, however, does not lead to this conclusion. Dark hydrogen flocculi seem to mark those regions on the sun's disk where hydrogen is present as an absorbing medium, which reduces the intensity of the light coming through it from below. In certain areas, where the temperature is higher or the condition of radiation otherwise different, the hydrogen flocculi are bright. In many cases eruptions are in progress at these points, but in others the difference in brightness is apparently not the direct result of eruptive action.

The hydrogen flocculi, thus photographed with the lines $H\beta$, $H\gamma$, or $H\delta$, differ in many respects from the calcium flocculi. Not only do they usually appear dark, where the calcium flocculi are bright: their forms exhibit striking peculiarities, which are absent or much less conspicuous in the case of calcium. The appearance of the calcium flocculi resembles that of floating cumulus clouds in our own atmosphere, whose capricious changes in form reveal the operation of no simple law. But the hydrogen flocculi, on the contrary, exhibit a definiteness of structure in striking contrast to this appearance.

* Eruptive prominences are also recorded on the disk as bright flocculi.

Some of the photographs strongly remind us of the distribution of iron filings in a magnetic field, and suggest that some unknown force is in operation.

Such was the condition of the subject when the red $H\alpha$ line of hydrogen was first applied to the photography of the flocculi, on Mount Wilson, in March 1908. The calcium and hydrogen flocculi had been studied for several years, and much had been learned as to their nature and their motions. It had been found, for example, that the calcium flocculi observe the same law of rotation that governs the motions of sun-spots, while the hydrogen flocculi apparently follow a different law, in which the decrease in the angular rotational velocity from the equator toward the poles is much less marked. The latter result is in harmony with the investigations of Adams, whose accurate measures of the approach and recession of the hydrogen at the eastern and western limbs of the sun offer but little evidence of equatorial acceleration on the part of this gas. For this and other reasons it had been concluded that the hydrogen shown in such photographs reaches a higher level than the vapours of the bright (H_2) calcium flocculi. The region of the atmosphere previously explored with the spectroheliograph was nevertheless confined (except in the case of eruptions and dark calcium flocculi) to a comparatively low level, lying within a few thousand miles of the photosphere. What might be expected if a still higher region could be satisfactorily photographed in projection on the disk?

The red line of hydrogen offered the means of disclosing the phenomena of this higher atmosphere. As it may not immediately appear why different lines, caused by the radiation of the same gas, should not give precisely similar photographs, a brief reference to the aspect of a prominence in the red and blue hydrogen lines may be advantageous. Here are two photographs of the same prominence, seen in elevation at the sun's limb, one made with $H\alpha$, the other with $H\delta$. As the red line is very bright, even in the highest regions, the photograph taken with its aid shows the entire prominence. $H\delta$, on the other hand, is relatively weak at the higher levels, and consequently only the lower and brighter parts of the prominence are well recorded when this line is used. If, now, we suppose ourselves immediately above such a prominence, at a point where we observe it in projection against the disk, it is evident that the character of the hydrogen lines must depend upon their brightness at different levels. As we know that, speaking generally, absorption is proportional to radiation, the amount of light absorbed in the upper part of the prominence will be much greater for $H\alpha$ than for $H\delta$. Hence the average level represented by the absorption of $H\alpha$ will be higher than the average level represented by $H\delta$, since the higher gases play a more important part in the production of the former line. We may therefore expect that photographs of the sun's disk, taken with the light of $H\alpha$, will show the dark areas corresponding to absorption in

the prominences much more clearly than photographs taken with $H\delta$. Moreover, since $H\alpha$ is stronger than $H\delta$ in the upper chromosphere, in regions where no prominences are present, the *average* level represented by this line will, in general, be higher than that represented by $H\delta$. A comparison of two photographs of the sun's disk, made with the lines in question, will suffice to make this clear. This enormous group of prominences, stretching for several hundred thousand miles across the sun, is much more clearly indicated by $H\alpha$ than by $H\delta$. In general, the hydrogen flocculi are stronger and more distinct when photographed with $H\alpha$, and there are some regions which appear bright with $H\alpha$ and dark with $H\delta$. This latter peculiarity probably has an important bearing upon the similar behaviour of hydrogen in certain stars and nebulae, but a discussion of this question cannot be undertaken here.

The first of the $H\alpha$ photographs gave strong hopes of a substantial advance in our knowledge of the solar atmosphere. The sharpness and comparatively strong contrast of these flocculi, and the evidences of definite structure and clearly defined stream lines which they revealed were highly encouraging. The work was begun during the disturbed weather of the rainy season, when the definition of the solar image is never of the best. On April 30, 1908, the first photographs were secured under the fine atmospheric conditions which prevail in the dry season. This direct photograph (Fig. 1) shows a small and insignificant group of sun-spots, which would not seem, without other indications, to merit special attention. The next photograph (Fig. 2) shows that an enormous calcium flocculus occupied this region of the sun, but its form was in no wise remarkable, and afforded no evidence of the phenomena brought to light by the $H\alpha$ photograph (Fig. 3). The structure recorded with the aid of the latter line recalls Langley's sun-spot drawing, and suggests the operation of some great force related to the sun-spot group. The same cyclonic structure had been less satisfactorily recorded on the previous day, but a comparison of the two photographs failed to indicate such changes as motion along the apparent stream lines might be supposed to produce.

The close of the rainy season now permitted an active study of the $H\alpha$ flocculi to be undertaken. Many photographs were made daily, and the almost constant association of apparent cyclonic storms or vortices with sun-spots became evident. During several months of the year in California an unbroken succession of clear days can be counted upon, so that the changes of a given vortex can be followed without interruption. The cyclonic storms were found to be of two principal types: the first associated with groups of spots and represented in such photographs as those of April 30 and September 2; the second associated with single spots, and resembling a simple vortex, as illustrated in the photographs of September 9 and October 7, 1908 (Fig. 4). The appearance of these simple vortices is such as

to indicate rotation in a clockwise direction in the southern hemisphere, and in a counter-clockwise direction in the northern hemisphere (assuming the direction of motion to be inward towards the spot). However, this cannot be taken as a general law, corresponding to the law of terrestrial cyclones. Indeed, many instances have been found of closely adjoining spots, in the same hemisphere, and frequently in the same spot-group, having magnetic fields of opposite polarity, produced by vortices rotating in opposite directions.

In some cases, at least, these vortices seem to exercise a powerful attraction on the surrounding gases, as a series of photographs taken on June 3, 1908, illustrates. A long dark hydrogen prominence, first photographed in elevation at the sun's limb on May 28, had advanced half-way across the solar disc. It lay at the outer boundary of a well-defined vortex, centred on a sun-spot. This spot had been gradually separating into two parts, and on June 3 the separation was complete. The first photograph of a series of nine was made on this day at 4 h. 58 m. Several successive photographs indicated no appreciable change, but one taken at 5 h. 7 m. showed that the prominence was developing an extension toward the spot. At 5 h. 14 m. this had assumed the appearance illustrated in the next photograph, and 8 m. later, when the last photograph of this series was taken, the extension had almost reached the spot. It will be seen that it divided into two parts, which indicates that each umbra was a centre of attraction. The average velocity of the motion toward the spot was over 100 km. per second. Later photographs, made on the following days, show a ring of bright hydrogen surrounding the spots, suggesting that the comparatively cool hydrogen carried down into the spots was reheated and returned to the surface, after escaping from the lower end of the vortex. We thus seem to be observing some of the phenomena of an actual vortex in the sun. But it must not be supposed that cases of this kind are common. In many instances the hydrogen flocculi do not appear to move toward or away from spots, but undergo changes of intensity, as though the physical condition of the gas was constantly changing. But before proceeding further with a discussion of these sun-spot vortices, let us turn to another phase of the subject, which will afford much new information indispensable for this purpose.

We are all familiar with the effect produced by passing an electric current through a wire helix. The lines of force of the resulting magnetic field are parallel to the axis of the helix, and its intensity is determined by the diameter of the helix, the number of turns of wire and the strength of the current. We also know, from Rowland's experiment, that the rapid revolution of an electrically charged body will produce a magnetic field. Thus if a sufficient number of electrically charged particles were set into rapid revolution by the solar vortices a magnetic field should result. What warrant have we for assuming the existence of charged particles in the sun, and how could such a field be detected?

Let me pass rapidly in review a series of phenomena with which you are all familiar. Sir William Crookes showed in this lecture-room as long ago as 1879 that the negative pole of a vacuum tube sends out a stream of particles capable of setting a light wind-mill in rotation, and deviated from their straight path when under the influence of a magnetic field. He has kindly consented to show the same tube again to-night; you now see the effect upon the screen. The recent work of Sir Joseph Thomson and others has proved that these are negatively charged particles, called "corpuscles" or "electrons," and that their mass is about $\frac{1}{1800}$ of the mass of an atom of hydrogen. Moreover, Thomson has shown that at low pressures these corpuscles are given off from a hot wire or from the carbon filament of an incandescent lamp. He has also demonstrated that this property of emitting corpuscles at high temperature is common to carbon and to metals, whether in the solid or in the vaporous condition. Thus we have warrant for the belief that the sun, composed of just such elements as constitute the earth, must emit great numbers of these corpuscles. As Thomson has estimated that the rate of emission of a carbon filament at its highest point of incandescence may amount to a current equal to several amperes per sq. centimeter of surface, we can hardly be mistaken in assuming the existence of still more powerful currents in the sun. The emission of negatively charged particles implies the emission of positively charged particles, but in laboratory experiments, because of unequal rates of diffusion or other causes, charges of one sign are always found to be in excess. We thus have reason to believe that powerful magnetic fields may result from the revolution of these particles in the solar vortices.

In seeking a means of detecting such fields, let us first recall Faraday's discovery of the effect of magnetism on light, made at the Royal Institution in 1846. This discovery relates to the rotation of the plane of polarisation of light when passed through a plate of dense glass in a strong magnetic field. Although Faraday, in what was said to be his last experiment, endeavoured to detect the effect of magnetism on the lines of the spectrum, he failed because the apparatus then available was not sufficiently powerful. In 1896, Professor Zeeman examined with a large spectroscope the two yellow lines emitted by sodium vapour in a flame between the poles of a powerful magnet. Observing in the direction of the lines of force, he saw that the sodium lines widened when the magnet was excited. Subsequently with more powerful apparatus, he found that a single line, when observed under the above conditions, is split into two components by a magnetic field. The distance between the two components is a measure of the strength of the field. But the most characteristic quality of these double lines, which distinguishes them from double lines produced by any other known means, is the fact that the light of the two components is circularly polarised in opposite directions. If, then, we encounter a double line in the spectrum of any substance,

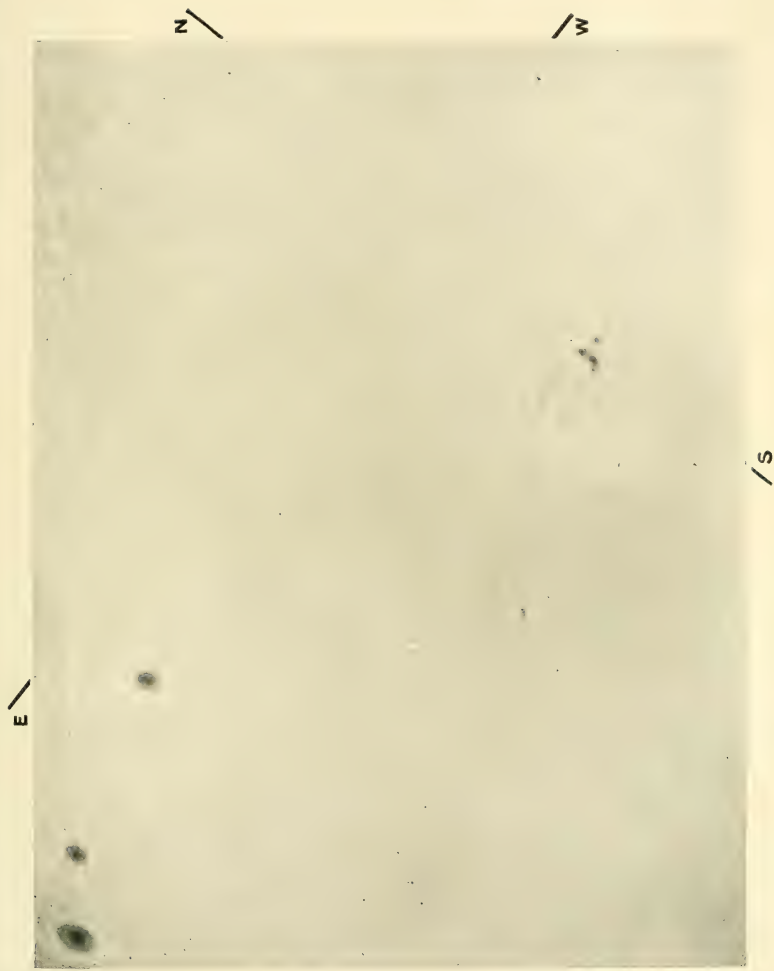


FIG. 1.—DIRECT PHOTOGRAPH OF SUN-SPOT GROUP.
1908, April 30, 6h. 25m. A.M. Pacific Standard Time.

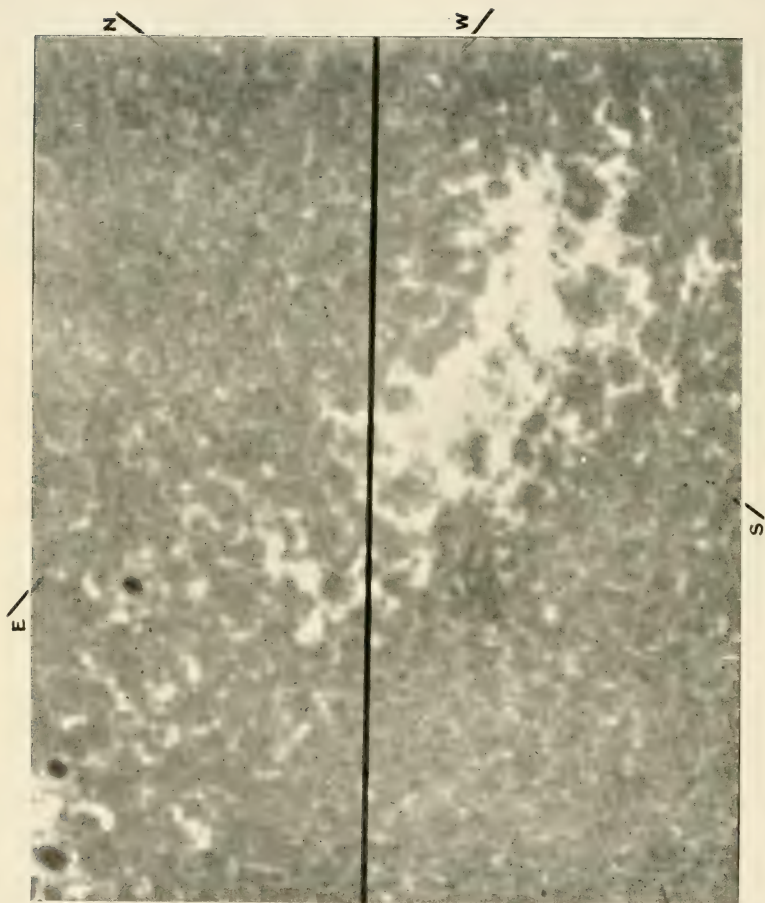


FIG. 2.—SAME REGION OF THE SUN, SHOWING THE CALCIUM (H_2) FLOCCULI.
1908, April 30, 4h. 43m. p.m. P.S.T.

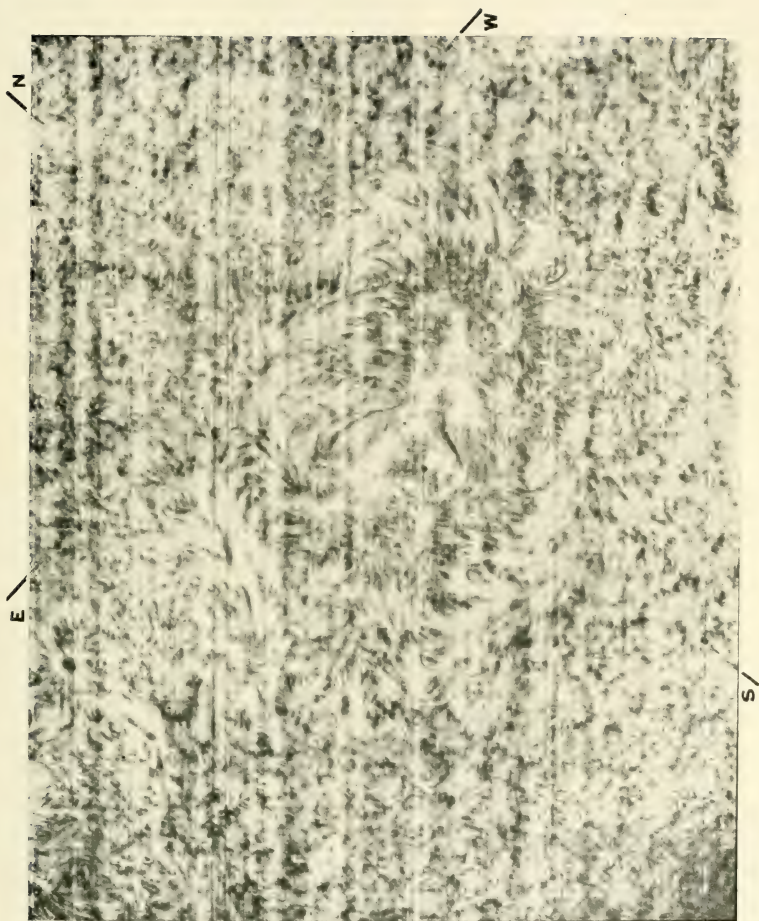


FIG. 3.- SAME REGION OF THE SUN, SHOWING THE HYDROGEN ($H\alpha$) FLOCCULI.
1908, April 30, 5h. 06m. p.m. P.S.T.

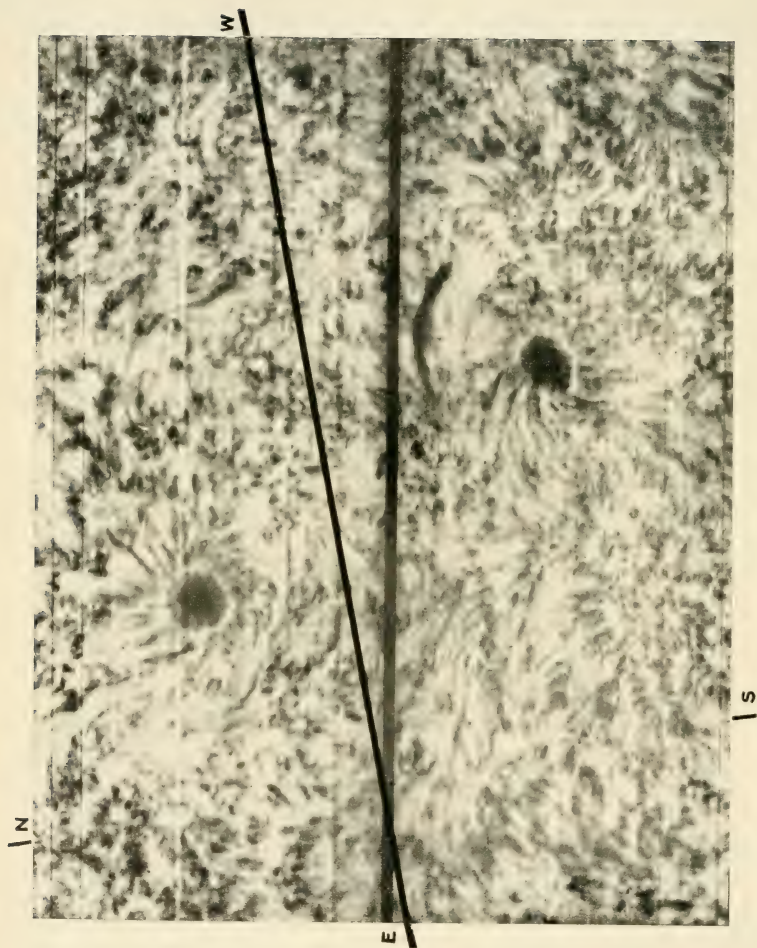


FIG. 4.—SUN-SPOTS AND HYDROGEN FLOCCULI, SHOWING RIGHT- AND LEFT-HANDED VORTICES,
1908, October 7, 7h. 02m. A.M. P.S.T.

and suspect it to be due to a magnetic field, we must apply the test for circular polarisation.

The simplest means of testing for circularly polarised light is to transform it into plane polarised light by passing it through a quarter-wave plate or a Fresnel rhomb. In the case of a Zeeman doublet, we would then have issuing from the rhomb the light of the two components, polarised in planes at right angles to one another. A Nicol prism, standing at a certain angle, will transmit one of these plane polarised beams and cut off the other. Turning the Nicol through 90° will cause the component previously cut off to be transmitted, and the other to be stopped.

Consider a sun-spot at the centre of the solar disk, and suppose it to be produced by a vortex, the axis of which lies on the line passing from the eye of the observer through the spot to the centre of the sun. Under these circumstances, if a strong magnetic field is produced by the vortex, the spectral lines due to vapours lying within this field should be widened or transformed into doublets. Moreover, the light of the components of these doublets should be circularly polarised in opposite directions. This would be true if the spot vapours were emitting bright lines, identical in character with those emitted by a radiating vapour between the poles of a magnet. The experiments of Zeeman, Cotton, König, and others, show, however, that dark lines, produced by the absorption of the spot vapours, should behave precisely in the same way as bright lines.

The spectrum of a sun-spot was observed for the first time by Lockyer in 1866. He found that many of the lines of the solar spectrum were widened where they crossed the spot, and the observation of these widened lines has been carried on systematically by many observers ever since. Conspicuous among these observers was Young, whose last observations were made with a powerful grating spectroscope attached to the 23-inch Princeton refractor. This instrument showed that some of the spot lines are close doublets. Dr. Walter M. Mitchell, who at first worked in conjunction with Professor Young and later by himself, gave special attention to these double lines, which he found to be particularly numerous at the red end of the spectrum. He called them "reversals," and the existing evidence favoured the view that they were produced by the radiation of a hotter layer of vapours overlying the spot, which would give rise to a narrow bright line at the centre of the widened dark line. True reversals of this kind actually seem to occur in the case of H and K and other lines in the spot spectrum, and it was therefore natural that Mitchell should attribute the similar phenomena of the spot doublets to a similar cause. It was generally supposed that the widening of the dark lines was due to the increased density of the spot vapours. The diverse character of the lines in the sun-spot spectrum is well illustrated by this drawing, which is due to Mitchell. In addition to the ordinary widened and "reversed" lines, we find

cases where a dark central line is accompanied by wings, others in which lines are thinned or completely obliterated, etc.

I have already referred to the importance of applying in astronomical research the methods of the physicist. During the last quarter of a century the study of spectroscopic phenomena in the laboratory has been completely transformed. It may well be said that this transformation, which has involved such discoveries as spectral series, the effect of pressure on wave-length, and the Zeeman effect, has been directly due to the use of Rowland's concave gratings, of great focal length, arranged for photography. In astronomical spectroscopy great advances have also been made, but the spectroscope has continued to occupy the place it formerly held as an attachment of the telescope. Although Rowland used a long-focus concave grating for his classic study of the solar spectrum, the heliostat and lens employed with this instrument gave so small a solar image on the slit that the investigation of sun-spots and other details was impossible. We thus see that while in the observatory the spectroscope continued to be used as an accessory of the telescope, in the laboratory the parts were exchanged, and the telescope was employed simply as an accessory of the spectroscope. It seemed obvious that a great opportunity for advance lay open to the investigator who would combine a long focus spectroscope with a long focus telescope. As it would be difficult or impossible to use for photography a sufficiently long spectroscope attached to the tube of an equatorially mounted telescope, some form of fixed telescope was plainly essential.

The tower telescope on Mount Wilson (Fig. 5) is designed to accomplish this purpose. It consists essentially of a 12-inch refracting telescope, of 60 feet focal length, mounted in a fixed position, pointed directly at the zenith. The ordinary telescope tube is replaced in this case by a light steel tower, firmly held in position by steel guy ropes. The 12-inch objective lies horizontally at the summit of the tower, and sunlight is reflected into it from the second of two adjustable plane mirrors. The first of these mirrors is mounted as a coelostat, and is rotated by an accurate driving-clock about a polar axis at such a rate as to counteract the apparent motion of the sun. Thus a beam of sunlight is reflected from the coelostat mirror to the second mirror, which sends it vertically downward through the objective. In the focal plane, 60 feet below the objective, an image of the sun, about 6.6 inches in diameter, is formed on the slit of a spectrograph, at a height of about three feet above the surface of the ground. After passing through the slit, the light of any desired portion of the solar image (a sun-spot, for example) descends vertically into a well about 30 feet deep, excavated in the earth beneath the tower. Thirty feet from the slit the diverging rays encounter a 6-inch objective, through which they pass. After being rendered parallel by the objective, the rays fall upon a Rowland plane grating, ruled with 14,438 lines to the inch. The grating



FIG. 5.—TOWER TELESCOPE ON MOUNT WILSON.

breaks up the light into a series of spectra, and the rays are returned through the same objective, which brings the spectra to a focus at a point near the slit. By inclining the grating at a slight angle, the image of the spectrum is made to fall at a point slightly to one side of the slit, and here the photographic plate is placed. Thus a portion of the spectrum 17 inches in length can be photographed in a single operation. In the work on sun-spots, most of the photographs are taken in the third order of the grating, where the dispersion and resolving power are very high. When the spot spectrum is being photographed, only the light from the umbra is admitted to the slit. At the end of the exposure this portion of the slit is covered, and light from the photosphere, at a point removed from the spot, is admitted to the slit on either side. Thus the narrow spot spectrum is photographed between two strips of solar spectrum, used for comparison.

The advantages of this combined form of telescope and spectrograph are considerable. On account of the great thickness (12 inches) of the mirrors, the height of the coelostat above the heated earth, and the use of a vertical beam, the definition of the solar image is always better than with the Snow (horizontal) telescope. Another important advantage is the nearly constant temperature at the bottom of the well, where the grating is placed. This permits long exposures to be given, when necessary, without danger of such displacements of the spectral lines as would be caused by expansion or contraction of the grating. The grating used in this spectrograph is a small one, which I have employed in most of my work since 1889, but the unusual focal length of the spectrograph permits the full visual resolution of the grating to be utilised in photographic observations. Thus it has become possible to photograph the widened lines and doublets, as well as a host of narrow lines, most of them due to chemical compounds, which had not previously been recorded in the spot spectrum.

Lack of time prevents me from discussing in this lecture the various studies of sun-spot lines carried out with this instrument before the attempt to detect a magnetic field in spots was undertaken. An extensive catalogue of these lines is nearly complete, a preliminary map has been issued and a better one is in preparation, and a series of investigations with the arc and electric furnace has suggested that the strengthening and weakening of certain lines is due to a reduction in the temperature of the spot vapours. At present we are concerned with the cause of the widening and doubling of spot lines, and the method of testing this question must now be described.

A Nicol prism was mounted above the slit of the spectrograph, and just above this a Fresnel rhomb. If the components of a spot doublet were circularly polarised in opposite directions, passage through the rhomb should give two plane polarised beams, the planes of polarisation making an angle of 90° with each other. Thus in

one position of the Nicol one of the components should be photographed alone, and by turning the Nicol 90° this should disappear and the other component come into view.

When this test was applied with the tower telescope, in June 1908, the true character of the spot doublets became apparent (Fig. 6). One or the other component of the doublet could be cut off at will by rotating the Nicol, precisely as Zeeman had done in the laboratory. On account of the unique character of the Zeeman doublets, this test alone was almost sufficient to prove the existence of a magnetic field in sun-spots. But one of the great beauties of the Zeeman effect is its many-sided character, which permitted the test to be multiplied and extended. From Zeeman's first experiments it was known, for example, that if the strength of the magnetic field is insufficient to separate completely the components of a doublet, the edges of the resulting widened line should be circularly polarised in opposite directions. Thus those lines which are widened, but not doubled, in spots might be expected to shift in position when the Nicol is rotated. This was found to be the case. Again, the lines which constitute the flutings of the spectra of compounds are not, in general, affected by a magnetic field. Hence such lines in the spectrum of a sun-spot should not be shifted when the Nicol is rotated. This, also, was found to be true. But a still more satisfactory test was suggested by another laboratory phenomenon. When a doublet is observed along the lines of force, with one of the components extinguished by the Nicol, reversal of the current through the magnet should extinguish the visible component and cause the invisible one to appear. In the sun, according to our hypothesis, reversal of the direction of revolution in a vortex should correspond to reversal of the current through the coils of a magnet. Hence the red component of a doublet should appear in the spectrum of a vortex rotating in one direction, the violet component in that of a vortex rotating in the reverse direction. Fortunately, the appearance, on opposite sides of the solar equator, of two spot vortices rotating in opposite directions (Fig. 4), made this test possible. The results were perfectly in accord with the hypothesis.

So far we have been considering only such phenomena as are observed parallel to the lines of force of a magnetic field. But a spectral line which, under such circumstances, appears as a doublet, is usually transformed into a triplet, when the observation is made at right angles to the lines of force. The circularly polarised side components of the doublet give place to plane polarised components, occupying the same position, while another line appears centrally between them. The light of this line is also plane polarised, the direction of the vibrations being parallel to the field, while the vibrations of the side components are in a plane at right angles to the field. Thus when a spot is carried by the solar rotation to a point near the limb, we might expect the double lines in its spectrum



FIG. 6.

- (1) SOUTHERN SPOT, SHOWING RED COMPONENTS OF DOUBLETS, NICOL 29° W.
- (2) NORTHERN SPOT, SHOWING VIOLET COMPONENTS OF DOUBLETS, NICOL 29° W.
- (3) NORTHERN SPOT, SHOWING RED COMPONENTS OF DOUBLETS, NICOL 61° E.
- (4) SPOT SPECTRUM WITHOUT RHOMB OR NICOL, SHOWING BOTH COMPONENTS OF DOUBLETS.

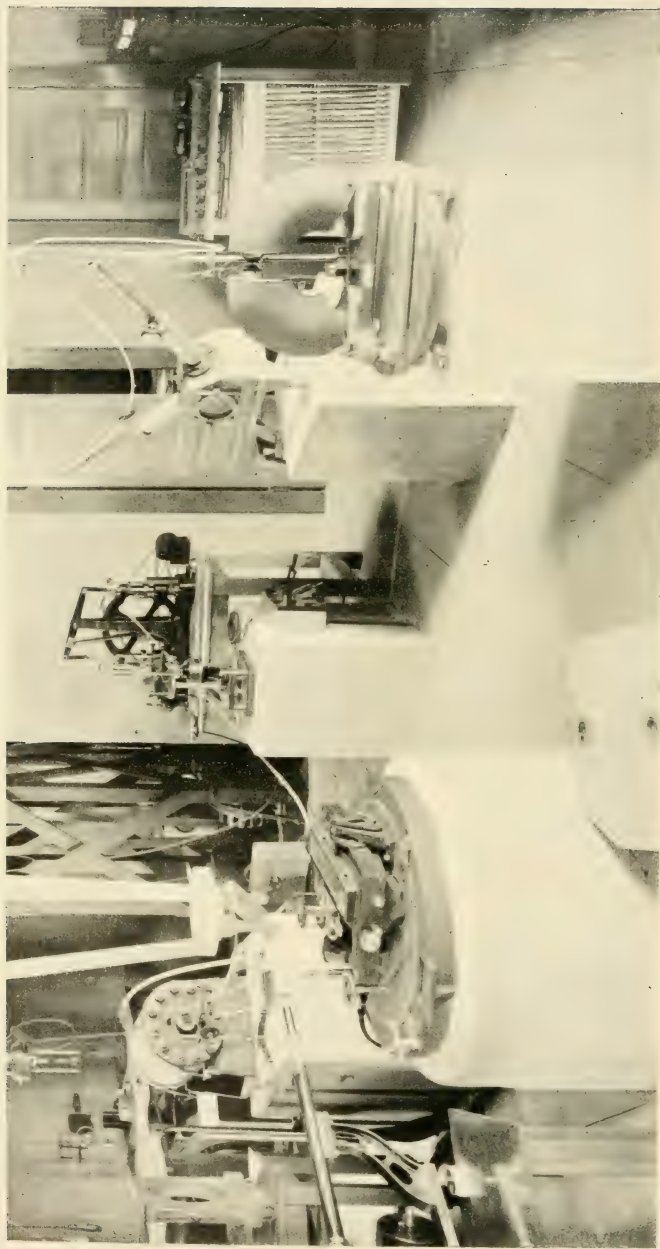


FIG. 7.—INTERIOR OF PASADENA LABORATORY, SHOWING SLIT-END OF VERTICAL SPECTROGRAPH AND
MAGNET USED IN STUDY OF ZEEMAN EFFECT.

to be transformed into triplets, if produced by a magnetic field. The failure of the central line to appear seemed to raise an important argument against the magnetic hypothesis.

At this point the necessity of conducting laboratory investigations in immediate conjunction with astronomical observations is well illustrated. Fortunately, our laboratory was already well equipped for work of this nature (Fig. 7). In anticipation of the possibility that observations of the Zeeman effect would be needed in the interpretation of solar and stellar phenomena, a powerful electro-magnet, with suitable accessory apparatus, had been provided. A brilliant spark produced between metallic electrodes in the field of the magnet furnished the source of light. As many of the double lines in sun-spot spectra are due to iron, this metal was selected for the first experiments. The spectrum was photographed, at various angles with the lines of force, with a powerful spectrograph, like the one used with the tower telescope, similarly mounted in an underground chamber.

The difficulty of accounting for the behaviour of the iron doublets in the sun was removed by these investigations. It appears that these lines do not become triplets, when observed across the lines of force. In reality they are changed to quadruplets, or doublets in which each of the components is a close double line. In the magnetic field of sun-spots, which is much weaker than the field used in the laboratory, the closely adjoining lines which constitute the components of the doublets cannot be separated. Thus these sun-spot lines should appear double at whatever position the spot may occupy on the sun's surface.

The distance between the components of doublets or triplets separated in the magnetic field varies greatly for different lines. Some exceptional lines are not affected in the least, others are merely widened, and others are clearly and sometimes greatly separated. It is therefore important to compare the widening and the separation of lines in a sun-spot spectrum with the corresponding phenomena in the magnetic field. With few exceptions, most of which may be accounted for by the presence in the spot spectrum of closely adjoining lines of other elements, the solar and laboratory results were found to be in good agreement. The following table gives a comparison of certain iron lines in the spot and laboratory.

Wave-Length	$\Delta\lambda$, Spark	$\Delta\lambda$, Spark 5.1	$\Delta\lambda$ Spot	
6213.14	0.703	0.138	0.136	-0.002
6301.72	0.737	0.144	0.138	-0.006
6302.71	1.230	0.241	0.252	+0.011
6337.05	0.895	0.175	0.172	-0.003

The column headed " $\Delta\delta$, spark" gives the distance between the components of the lines as observed in the laboratory. As the strength of the magnetic field used in the laboratory was about 5.1

times that of the spot, the quantities obtained by dividing the separations in the second column by 5.1 are given in the third column. These separations are directly comparable with the separations of the corresponding lines in the spot, which are given in the fourth column. The fifth column shows that the differences between the solar and laboratory results are very small. As the strength of the field in the laboratory was about 15,000 gauss, the strength of the field in this spot would be about $15,000 \div 5.1 = 2900$ gauss. The strongest field hitherto measured on our photographs of spot spectra is about 4500 gauss, corresponding to a considerably greater separation of the lines (Fig. 8).

When a similar comparison was made for various lines of titanium and chromium, a much less perfect agreement between the spot and laboratory results was found. It had already been observed that such lines as D of sodium and *b* of magnesium, which undoubtedly represent a much higher level than the great majority of lines in the spot spectrum, are but very slightly widened. As these lines are strongly affected by a magnetic field in the laboratory, it appeared evident that the strength of the field in spots must fall off rapidly in passing outward through the spot vapours. Under these circumstances lines of other elements, which represent levels higher than the average, should show small separations in the magnetic field of the spot. It seems probable that in this way the lack of perfect agreement between the laboratory and solar results, observed in the case of titanium and chromium, can be accounted for.

A further important test was afforded by the well-known phenomenon exemplified in Preston's law. According to this law, the distance between the components of the lines split up by a magnetic field varies directly as the square of the wave-length. This we found to be true even in the case of a metal like iron, the lines of which cannot be grouped into series, if the average separations of a sufficient number of lines were considered. We should therefore expect that the widening of lines in spots would rapidly decrease toward the violet, and that the separation of spot doublets should diminish in a similar way. A study of the spot spectrum shows that this actually occurs.

It soon appeared that the normal spot spectrum always contains triplets as well as doublets (Fig. 8.) These are less easily recognised, because the presence of the central line crowds the components so closely together that they are not readily separated with the resolving power available. As these triplets are photographed even when the spot is very near the middle of the sun, it is evident that the spot always sends out light which makes a considerable angle with the lines of force. In a normal triplet the central line is of twice the intensity of the side components, when observed at right angles to the lines of force, and disappears altogether when observed parallel to the lines of force. Thus, by determining the relative intensities of the central

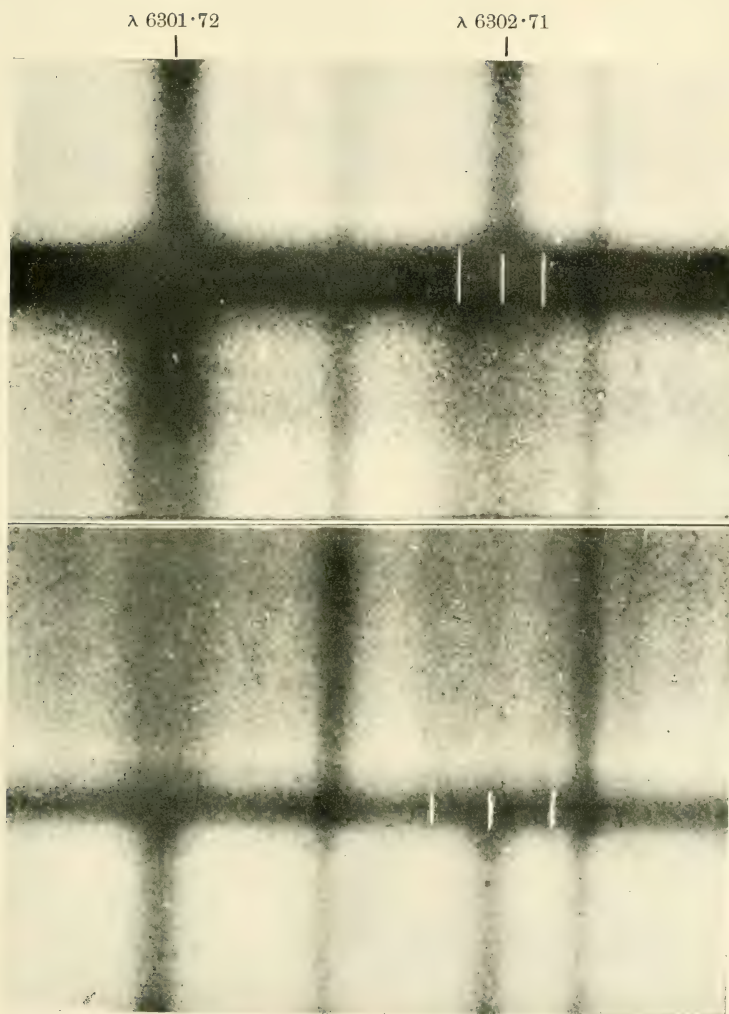


FIG. 8.—IRON DOUBLET ($\lambda 6301.72$) AND TRIPLET ($\lambda 6302.71$) IN
TWO SPOT SPECTRA, SHOWING FIELD STRENGTHS OF
2900 AND 4500 GAUSSES RESPECTIVELY.

and side lines of such a triplet, the angle between the lines of force and the line of vision can be obtained. In the case of sun spots, the data at present available are not sufficient for the accurate determination of this angle, but it seems to lie between 30 and 60 degrees, when the spot is near the centre of the sun. On the hypothesis that the magnetic field is produced by the spot vortex, it would then follow that the axis of the vortex, instead of being radial, as we at first assumed, makes an angle of much less than 90° with the surface of the photosphere.

The time at my disposal permits me to describe briefly only a few other phases of this investigation. In the laboratory the central line of triplets is polarised in a plane parallel to the magnetic field. Hence, if the light is passed through a Nicol prism, used without a rhomb, it should be possible to extinguish this line at certain positions of the Nicol, in which case a spot triplet would appear as a doublet. This test has also been applied to the spot triplets, with the expected result. In fact this method supplies a convenient means of recognising close triplets, the components of which are too closely crowded to be seen separately before the central line is cut out. Indications have also been obtained of what may prove to be unequal rotation of the plane of polarisation of this central line in different parts of spots. The gradual decrease in the strength of the field from the umbra to the outer limit of the penumbra has been studied, and magnetic fields have been detected on the sun's disk in certain regions outside of sun-spots. It is evident that many new phases of the subject are likely to be developed in the future, especially if larger images of the sun and more powerful spectrographs are employed. In this connection it may be stated that a tower telescope of 150 feet focal length, to be used on Mount Wilson, with a spectrograph of 75 feet focal length, is now under construction. This will give a focal image of the sun about 16 inches in diameter, in which small spots, as well as large ones, can be studied.

Although it now seems to be demonstrated that sun-spots are electric vortices, judgment should be reserved as to the various theories which have been advanced to account for their origin. Many of the results I have described appear favourable to Emden's solar theory, but it seems to be opposed by the important investigations of Evershed, who has found that the metallic vapours in sun-spots flow radially outward from the umbra, parallel to the photosphere. The further development of Evershed's work, and the continued study of solar vortices and magnetic fields, should soon permit a reliable theory of sun-spots to be formulated.

It is evident that the rapid decrease upward of the strength of the field in spots would prevent it from having an appreciable influence on the higher solar atmosphere. At the distance of the earth, as Schuster has shown, the combined magnetic effect of several spots, all assumed to be of the same polarity, and not taking into

account such rapid decrease in strength at higher levels as is actually observed, would be altogether incompetent to account for terrestrial magnetic storms.

In concluding, I wish to express my appreciation of the assistance I have received from my colleagues at Mount Wilson. I am particularly indebted to Messrs. Adams, Ellerman, King, Nichols and St. John for aid in connection with the present investigation.

[G. E. H.]

WEEKLY EVENING MEETING,

Friday, May 21, 1909.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L.
Sc.D. F.R.S., President, in the Chair.

THE HON. IVOR CHURCHILL GUEST, M.P.

Afforestation.

You are probably aware that for many years past far-seeing and practical men have been engaged in pleading the case of afforestation in the United Kingdom—I regret to add without much success. That there is a *prima facie* case for afforestation may be gathered from the fact that as far back as 1885 they succeeded in getting a committee appointed to investigate the subject. Since then a further committee took evidence in 1902, and recently the Irish Board of Agriculture appointed a committee on the same subject. Those committees, which represent collectively a considerable volume of opinion, expert and practical, have reported favourably on the proposals, but up to the present time nothing or next to nothing has been done. Perhaps this is due to the fact that from the very nature of the case, a long while must elapse before the talents which it is urged should be buried in the ground, can come back to the coffers of the community, while for reasons which I shall presently adduce, this is a deterrent which acts with even greater force in the case of the private landowner, who, in the vast majority of instances, can never hope to recover a single farthing of the sums he may invest in the planting of trees, or loss from the dedication of rent-producing acres to this use.

About fifteen months ago the King was pleased to direct the Royal Commission over which I have the honour to preside, that on Coast Erosion and Afforestation, to report as to whether it is desirable to make an experiment in afforestation as a means of increasing employment during periods of depression in the labour market, and if so, how, and by whom, this should be done. The Commission have recently completed their labours as to this branch of the inquiry entrusted to them, and issued a report—a practically unanimous report, I beg to observe, not without pride—with the substance of which I propose to deal to-night, hoping sincerely that you will not find the subject too technical or dull.

Until well on in the Middle Ages it appears certain that enormous stretches of Great Britain and Ireland were covered with trees sown by the hand of Nature. In those days facilities for transport were

limited, and timber had only a local value. Moreover, it was hard to cut and reduce to convenient shapes and sizes with the primitive tools which our forefathers possessed; so it came about that only those lands that were actually needed for the purposes of agriculture were cleared. Further, it was actually the policy of the Norman kings to protect and increase forests because they harboured the deer which they loved to hunt. Thus it came about that, until generations comparatively recent, there was wood and to spare of all the sorts that are indigenous to our climate, and notably of oak and ash, elm and beech. A few of these ancient trees are still left in parks and chases. Many of them are huge pollards, whereof in bygone ages the tops were cut every forty or fifty years to serve as firewood, where sea-borne coal was not obtainable. Perhaps you would like to see a couple of specimens? One of them, a famous oak, still grows in Sherwood Forest; and another, a yew from Crowhurst Churchyard in Surrey, believed to be the largest in the world: at any rate, it has a girth of 33 feet, and very likely was a seedling a thousand years or more ago [two slides shown].

Now if Great Britain was so well stocked, imagine the state of other lands. Whole provinces in Europe were green with trees. Caesar, if I remember right, tells us something of the woodlands of Germany, and, indeed, of Britain. Strabo, too, writing in the first year of our era, throws some light on the matter as it was in the Island of Cyprus. He mentions that Eratosthenes speaks of the copper mines there as being of "some little service," since they caused timber to be cut down to smelt the ore, and adds that ship-building was also useful in this respect. And now see the contrast. The Turk has swept away the forests of Cyprus; and one of the great aims of our Government there is to re-create them, not only for the sake of the wood, but because in their absence the rainfall of the island runs to waste.

In the New World also, and in Australasia, gigantic and primeval forests covered hill and plain. There they grew in solemn grandeur till at last, their allotted life accomplished, they crashed to earth, whereon instantly their successors filled the place which they left vacant, drawing nutriment from their rotting substance. Then came the new era, and all was changed. In the United States and Canada, for example, and I believe that the same may be said of Australia and other countries, the destruction of tree-life during the last fifty years can only be called appalling. Millions of them that had taken anything up to a thousand years to grow, have been ruthlessly hacked down to suit the convenience or fill the pockets of the enterprising settler and dealer in lumber, while for every tree that has fallen by the axe, probably two have perished by fire, since the lucifer match has proved the greatest enemy of forests. One morning the wanderer or the sportsman leaves his careless fire burning in the heart of some mighty wood, and the next the flames are roaring through hundreds

of square miles of timber, leaving behind them nothing but black fingers pointing to the sky, and beneath, a bed of ashes.

In our own country we have been spared these fires which are so rapidly destroying the tree-life of great areas of the world, but other causes have been at work to lessen our home supply of timber, the chief of them being its gradual but sure destruction for the various purposes of human industry, and especially of shipbuilding. Thus, in Nelson's day, and long before and after it, the wooden walls of Old England were built almost entirely of English oak, and it must have taken a great many oaks to make a single three-decker. Moreover, these oaks were not re-planted, at any rate to a great extent. That is the trouble both here and in other lands. Man is very ready to cut down trees, but he thinks a long while before he sets others to take their place.

The long-sighted and business-like Germans, also the French and some other peoples are, it is true, exceptions to this rule, since so far back as a couple of centuries ago they, or some of them, began to re-afforest systematically, with the result that their descendants now possess magnificent stretches of woodland. But large as these are, they are totally insufficient to meet the demands of Europe, especially since it has become common to manufacture paper from wood-pulp.

The result of our prodigal consumption of timber, unaccompanied by any systematic re-afforestation, is to place us in the unenviable position of possessing the smallest percentage of woodlands of any country in Europe.

The area of woodland in the United Kingdom is only three million acres or 4 per cent. of the total area, Germany has thirty-four million acres or 25 per cent. of her total area, France twenty-two million acres or 17 per cent., whereas Sweden heads the list with fifty million acres or 51 per cent. of her total area [slide shown].

One word more about the character of such woods as remain to us before I pass on to the wider aspect of our subject.

The shipbuilders of a hundred years ago and before that period needed bent boughs to form the "knees" of their vessels. These "knees" are produced by the branching limbs of the oak, and can only occur where trees grow sparsely and have plenty of elbow room and air-space about them; therefore they thinned their oak woods with no sparing hand. Now such bent timber is no longer needed, but the tradition remains, and oak is still grown, where at all, in the same fashion. For this spaciousness there is another reason. We are a sporting people, and pheasants will not thrive in a dense wood; what they like are isolated trees joined up with coppice or undergrowth. This bad example has spread, moreover, to the cultivation of other species besides the oak. Thus in the photograph which I now show to you [slide shown], you see an English beech wood with the trees growing rough and crookedly, and in these [slides shown]

a beech wood near Hanover with the trees growing clean and straightly, as they should do, if they are to be used for commercial purposes. Yet a further consideration comes in. Here we practise Arboriculture not Sylviculture. We think of the beauty of the individual tree in our parks or game-preserves, not of the value and utility of a crop of such trees.

Now with your permission I will pass on to some of the practical aspects of Afforestation as ascertained and determined by the unanimous Report of the Royal Commission. That Report, I may state, has, like every other Report, been exposed to criticism at the hands of sundry experts, but for my own part, whatever those critics may think—and, I may observe, that many of their comments seem to be mutually destructive—I do not consider that it has sustained much damage from this ordeal.

The Royal Commission has found on the evidence submitted to it that Afforestation in the United Kingdom is both practicable and desirable; but, perhaps you will ask, if this is so, how is it that it has been neglected in the past? I have already stated that the traditions of shipbuilding, the interests of game, and the preference for arboriculture, in a word, the absence of scientific methods of sylviculture, are, in the main, responsible for this neglect. But to this a further cause must be added. That cause is the complete reliance on private enterprise in forestry. I am an individualist, and from an individualistic standpoint I assert that there would be more justification in leaving the postal or telegraph service (as in America) to private enterprise, than Afforestation. The fact is that sylviculture is an enterprise which rarely appeals to the private land-owner or capitalist. The prolonged time for which capital must be locked up before any return can be expected, the loss of rent and burden of rates over the whole period, and the absence of security for continuous care and management, act as deterrents. None of these objections apply to the State, whose corporate life and resources lend themselves in an especial degree to an undertaking of this character. If the State plants, it will certainly reap, which the individual owner can rarely hope to do. With the exception of lands administered by the office of woods and forests—and these have always been regarded as public recreation grounds and treated accordingly—and I think no one who knows the beauties of the New Forest for instance, would care to reverse this treatment—the State has no suitable land for Afforestation, and has deliberately abstained until quite recently from taking any part in the pursuit of an object of national utility. This is all the more regrettable when we hear from competent judges in the timber trade of the superiority of British timber, and when we are asked to observe examples of successful plantations yielding good returns [slide shown], and when we are assured that the soil and climate of these Islands are especially favourable to the production of commercial timber, and are led to

believe that storms and tree pests are less destructive here than elsewhere.

There is one objection which is often raised by people who have woods or forests on their properties, and that is, that the prices obtained are not, on the whole, remunerative, but when we consider the small quantities, the irregular supply, want of business connection, and entire absence of all organisation for conversion or transport which are characteristic of present conditions, the wonder is that in these days of narrow margins timber is worth cutting at all.

That forests can be remunerative is evident if we look no further than France or Germany. In Germany, where vast areas are scientifically managed, the State derives a substantial return from the forest areas, although prices run considerably lower than those obtainable at home, and generally speaking, the natural conditions are not so good. The State of Würtemberg shows a net annual return of one pound five shillings and four pence per acre; while Saxony, which is said to most closely resemble our economic and physical conditions, shows over an area of 429,300 acres of forests, a net annual return of one pound two shillings per acre, after deducting outgoings. The official valuation of this forest is over nineteen millions sterling. Continental forests are therefore a successful State enterprise, and it seems impossible to escape the inference that we may do likewise [slides shown].

Neither in theory nor in practice is there any reason why forestry should not be profitably pursued in the United Kingdom. Some people imagine that this can only be true if good agricultural land is acquired and planted with oak or ash, and they point out that the displacement of the agricultural community, and economic waste involved in the conversion, would be out of all proportion to the gain, however sanguine the prospect. With regard to this objection, I may say at once that we do not propose to take agricultural land, and we do not propose to plant oaks, though perhaps some of the heavy clay land in the Eastern counties which is at present out of cultivation might be given up to this.

The conifer is a tree of commerce *par excellence*, and it will grow on relatively poor and unproductive land. The amount of land of this character suitable for afforesting has been very variously estimated, and without what is called a cadastral survey it is impossible to form a very accurate idea of what is available, but I think you may take it from calculations based upon information supplied to us by the Board of Agriculture, that there are, without materially encroaching upon agricultural land, some nine million acres below the fifteen hundred feet contour line which could profitably be employed for the purpose of growing timber crops [slide shown].

At the present moment we import from countries that have a climate more or less similar to our own, and therefore produce the same classes of timber, wood to the value of something over twenty

million pounds a year—that is exclusive of all exotic woods, such as teak and mahogany. Now on the basis of nine million acres being available at home for the production of such wood, and on the assumption—which, according to the evidence, seems to be reasonable—that each acre would produce annually one ton or one load of timber, when the crop came to maturity in eighty years' time, and if proper methods were adopted to assure its continuance, this country would produce about the same amount of wood which it now annually imports; in other words, exotic products always excepted, it would become self-supporting in the matter of its timber supply [slide shown]. I think you will agree with me that this would be a great gain for obvious reasons (*cf.* coal supply and timber props). But there are other and further advantages: thus, if 150,000 acres were afforested annually, which would be about the proper proportion in dealing with a total of nine million acres over the 80-year rotation, our conclusion is that it would afford employment each year for about eighteen thousand men during the winter months, which of course means that very many more would be benefited, seeing that a great number of these people would be married and have children or other dependents. Also perhaps about as many more would derive employment indirectly in the incidental and subsidiary occupations connected with forestry.

I may add here that the Small-Holding movement would certainly also receive a great stimulus. Nowadays there is a considerable amount of loose thinking and talking upon this matter of Small Holdings in England. While most people agree as to their desirability, those who have made a study of the subject are well aware that they cannot be made to pay in all our various conditions of soil, climate, and market. Many hold indeed that except on really rich and easily worked land, such as that of the Fen districts, or where particular and highly intensive industries, for example, French gardening, or bulb cultivation, or strawberry-growing, celery raising, and so forth are practised, their success remains problematical; but in the neighbourhood of great woods this would be almost assured, since such woods must afford a steady source of employment in the winter months, during which the Small Holder's wife and family can generally attend to the requirements of his little farm, while he himself is employed in earning good money in the forest.

Just now I spoke of the temporary employment which such a scheme of afforestation would furnish, but all employment connected therewith is not included in that term. On the contrary, for every hundred acres afforested, permanent work would be provided for one man, which means that if the whole nine million acres were ultimately put under trees, that area would provide a living for about ninety-thousand men and their families. Nor is this all, since in the train of the commercial cultivation of woodlands spring up many subsidiary industries; thus, timber is more profitably converted into useful

shapes and sizes on the spot where it grows, whereby much cost of carriage is saved, which conversion means the establishment of saw-mills in the centre of each block of forest. Where saw-mills are, engineers and lumber-men must be also, and probably carpenters and joiners who would manufacture the parts of articles of domestic use, such as doors and their frames or window-sashes—things, be it remembered, that now come in by the thousand from abroad, ready-fashioned for use by foreign labour. Again, manufactories would arise for the production of wood pulp which is now used in enormous quantities for the making of paper. Of these factories, I believe there are at present only two in the United Kingdom, and both of them make use of foreign wood. Such works in time would mean that population must arise about them to supply the needs and minister to the comfort and convenience of the workers; in short, they would retain a great number of people in the country districts who, as it is, drift into the towns, there, but too often, to swell the mass of misery and pauperism. This alone, I submit, is an end well worth attempting and much to be desired.

This mention of pauperism brings me to another and a very important question. You will remember that the terms of reference directed us to inquire into the expediency of Afforestation "as a means of increasing employment during periods of depression in the labour market," which means that our investigation was concerned not only with the possibilities of forestry but also with the fact, the sad and patent fact, of unemployment. I am well aware that from this circumstance have been hewn stones, whole cartloads of them, to pelt us and our conclusions. "What has the great cause of Afforestation to do with the pitiful business of tramps and won't-work loafers?" some of our critics have asked and continue to ask. I answer, with tramps and "won't-works" and other unemployables, little or nothing at all; but these wretched classes who present to Statesmen and Philanthropists one of the greatest problems of civilisation do not comprise the whole body of the unfortunates, who for one cause or another are among the unemployed. Much evidence on this point exists, but I may state that the upshot of it is that there exist thousands of men, most of whom have some acquaintance with labour on the land, who, if only the chance were given to them, are competent and willing, under proper supervision, to do most of the work directly and indirectly connected with the planting of trees upon a large scale; yes, as many or more of such men as could be employed under any scheme of afforestation that is at all likely to be set on foot in this country, and, even if I were wrong upon this point, even if such men did not exist, still the enterprise of afforestation conducted upon a large scale would have a very considerable remedial effect in connection with the prevalence of unemployment. For what is one of the great causes of this persistent lack of work? Is it not that the strong young people from the land who can find

no satisfactory opening there, no prospect of rising, are continually deserting the villages where they were born and flocking into the cities? And when they reach the city, do they not, by the operation of an economic law, displace and tread under those who are not quite so strong or quite so young, and take for themselves their share of the total store of sustenance available?

Here I wish to make it clear that my Commission, on which I believe every shade of political opinion is represented, was absolutely decided in the view that any scheme of afforestation financed by the State should be carried out on a strictly economic basis, although incidentally it, and we believe, would further philanthropic ends. We contemplate a business, and not a philanthropic venture, from which, according to our calculations, the State would reap a handsome profit, direct and indirect; that it must benefit those who need work is an extra advantage, which may, it is true, recommend it to the country, and on this ground alone we hope that it will be put into force. Let it be clearly understood, however, that we do not propose a new system of poor law relief, or that any man should be employed who is not willing and able to give a fair day's work for a fair day's wage. But independently of all outside considerations, the evidence to which we have listened and the actuarial estimates that we have made, bring us to the conclusion that afforestation in the United Kingdom will stand on its own feet as a commercial enterprise. Now I do not propose to inflict many figures upon you, and indeed those involved in this matter might frighten you or even empty this hall were I to read them all, so I will only state the end of the matter, which, unhappily none of us who are gathered here can possibly live to see, since a crop of timber takes eighty years to grow, and ere it can be planted and reaped, this generation will long have been gathered to its fathers.

If the full scheme that we have propounded were accepted—and here I may state the Treasury may, with confidence, be relied upon to cut it down if it wishes, in which case the receipts and expenditure must be reduced *pro tanto*—the annual sum required would be two million pounds. This, we suggest, should be raised by loan, since it is not fair that one generation should bear all the burden whilst the next and subsequent generations reap all the profit. If the capital required were obtained in this fashion, the net deficit, inclusive of the interest on the loan—which, we hold, should be defrayed out of taxation—would, in the first year, amount to ninety thousand pounds. By the fortieth year the figures would be much more formidable, the deficit in that year totalling something over three million pounds. After that the transformation scene begins, for the forests become self-supporting—or, in other words, all outgoings are met by incomings; and, in the eightieth year, the Chancellor of the Exchequer of the day, if such an official then exists, will rejoice as men rejoice in harvest, for he will enter into an income of about

seventeen and a half millions a year which, if the forests are properly cared for, each breadth being replanted as it is cut over, should go on for ever. Now this seventeen and a half millions represents $3\frac{3}{4}$ per cent. on the net cost calculated at accumulated compound interest of 3 per cent.—compound interest, I beg you to observe. Therefore, on this basis, plus the other advantages that have been touched on, the State putting money into afforestation would make almost a 4-per-cent. investment, a rate of interest with which business corporations are generally satisfied.

Or, we may look at the results in another way. At the eightieth year the State forests would be worth a capital sum of five hundred and sixty-two millions, that is, about one hundred and seven millions above the total cost involved in their creation—again calculated at 3 per cent. compound interest. Some of you may think these returns visionary; I can assure you, however, that to the best of our belief they are nothing of the sort. Every expense has been considered and allowed for, and every figure checked and re-checked by competent actuaries who enjoy the confidence of the Government offices. Moreover, there is another point to which I must call your attention. This ultimate profit is calculated on the assumption that timber will remain at its present price, which, I should add, is already a good deal higher for most kinds than it was a few years ago. But the evidence to which we listened, I think without exception, all went to show that there is the strongest probability, I might almost say the absolute certainty, that the woods most extensively used in commerce, such as those of the *Coniferae*, must grow very largely in value during the next half century. How can it be otherwise, indeed, when the world's supply is being so ruthlessly destroyed, and comparatively speaking, so little is being planted? If you have any doubt upon this matter, I beg you to consult the official returns which have recently been published in America, where the rapid wastage of the timber resources of the country is viewed with great alarm by experts and all thinking men. True, certain countries have faced the situation and begun to re-afforest extensively, and others, including America, may do so in the future. But we are assured and believe that nothing which is at all likely to be done at this late hour will appreciably affect the almost inevitable rise in prices, as the woods which Nature planted progressively disappear beneath the blows of the axe and before the breath of fire. In short, it appears certain that commercial timber will rise in price, unless the world lessens its timber consumption, and this seems extremely improbable, inasmuch as owing to this cause or to that, and notwithstanding the ever-increasing use of iron and concrete, the consumption develops from year to year. Therefore our estimates of future profit from State-conducted forests based upon present prices, appear to be very moderate. At any rate it would be fair to set this most probable but uncalculated profit against any possible loss that may have escaped our foresight, such

as the occurrence of periods of war or anarchy, when the woods were neglected, or the accident of great fire, or the advent of diseases fatal to tree life at present unknown to us, at any rate in our climate. Writing off the one possibility, or rather probability, of increase of returns, against the other possibilities of loss, my own opinion is that the estimates that are set out in our Report may be accepted as sound and accurate, and that if afforestation is carried out on the lines we have advocated, the resulting benefits will, in fact, be realised by the State.

A further point remains to be considered. If afforestation is practicable and desirable, by whom, and in what manner should it be undertaken? Some people think that private landowners should be encouraged by precept or example with the further inevitable help of State credit to afforest their own land, but I think I have already disposed of that contention, and in the absence of any extensive and systematic planting by private owners it seems unlikely that substantial progress on a large scale is to be expected from this source.

Others have proposed a scheme of co-operation between the private owner and the State whereby the owner furnishes the land and the State the money. It is not inconceivable that in rare instances some progress might be made on these lines, but if we consider the inevitable difficulties which arise from divided ownership and dual control, to say nothing of other obvious objections, it does not do to regard this proposal with optimism.

There remains therefore, the only alternative of direct State Afforestation. No doubt State Afforestation would necessitate powers for the compulsory acquisition of land. I do not mean that most of the land needed could not be acquired by voluntary negotiation, but some compulsion would be necessary where negotiation broke down. I may mention the fact that the acquisition of a large area on Salisbury Plain by the War Office, although in the main the result of private negotiation, was not accomplished without a Bill in Parliament. Our proposal is that the State should take such powers as they possess in dealing with the provision of Small Holdings, and that subject to the safeguards to private owners which exist in that Act, they should proceed on the same lines. The price paid should be a fair price and no more.

I have now outlined the case for British Sylviculture; it remains to consider the present situation. It is true the destinies of Afforestation are still on the knees of the Gods, but the Government seem inclined to make a start in an experimental manner. There is much to be said for being tentative, but an experiment in tree-growing is likely to be rather a lengthy operation. On the other hand, it is probably prudent to begin on a small scale, and on carefully selected ground. A forest of moderate dimensions would not put too great a strain on our resources or supply of men and material. We shall doubtless learn much as we proceed. We shall test the

estimate of cost and prove the capacity of labour. An official review of the world's timber supply would do much to remove the plausible timidity of national financiers, and a careful survey of lands in the United Kingdom would demonstrate our potentialities. Most of all, such experiment may be hoped to educate public opinion and enlist popular sympathy. I venture to appeal to you and to all interested in the welfare of the land and the rural community to give the subject your attention. A broad view of economics cannot exclude from its cognisance the grave national charge which unemployment, with all its concomitant results, involves, to say nothing of the personal deterioration by which it is often accompanied. No other proposal with which I am acquainted offers so fair a prospect of healthy and wholesome occupation. No other device for dealing with slack trade and want of work can be provided at so little ultimate cost to the community. Even if you think under these conditions afforestation will not pay, there will be some return. Contrast this with the two millions of local loans sanctioned last winter by the Local Government Board. No one can say that this expenditure is in any sense reproductive. An analysis will reveal the fact that, save where work was anticipated, which in itself will have an injurious result on employment in the future, no gain except those of local amenities, such as parks and gardens, was achieved.

To sum up, a national scheme of afforestation claims that it will contribute towards the solution of unemployment, if only by checking the rural exodus to the towns. It will provide a safe and reasonably remunerative investment for capital, and it will, in days when the world's timber supply is sensibly diminished, make our home industries to a great extent independent of foreign supplies. Were it to achieve only one of these results, it would still be well worth undertaking.

[I. C. G.]

WEEKLY EVENING MEETING,

Friday, May 28, 1909.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C.
D.C.L. Sc.D. F.R.S., President, in the Chair.

J. EMERSON REYNOLDS, Esq., M.D. Sc.D. F.R.S. *M.R.I.*

*Recent Advances in our Knowledge of Silicon and of its Relations
to Organised Structures.*

I HAVE placed on the table before you a magnificent natural crystal of the colourless mineral *quartz*, which is the property of the Royal Institution. This is the oxide—dioxide—of the element Silicon, about which I have the honour to address you this evening. This oxide of Silicon is—as you are doubtless well aware—commonly called Silica, and is met with in Nature in many conditions, either colourless as in this “rock crystal” or coloured in the black quartz, in common topaz and amethyst, and uncrystalline in agate and flint.

Not only is Silicon widely diffused in Nature in the many forms of its oxide, but it also constitutes between one-third and one-fourth of the original and non-sedimentary rocks—of which the solid crust of the earth largely consists—in these cases being chemically combined with Oxygen and various metals forming natural *Silicates*. In this diagram we have a necessarily very rough estimate of the relative proportions in which the chief constituents are present.

THE EARTH'S CRUST.

Approximate average Composition of non-sedimentary Rocks.

Oxygen	about 47 per cent.
Silicon	28 "
Aluminium	8 "
Iron	7 "
Calcium and Magnesium	6 "
Alkali Metals	4 "

The crust of the earth is in fact a vast assemblage of silicon compounds, and the products of their disintegration under the influence of water and other agents produces the various forms of clay, sand and chalk which constitute so large a portion of the earth's surface.

The solid crust of the earth is actually known to us for but a very few miles down—thirty at most—our deepest mines being mere scratchings on its surface; but, so far as known, practically all its constituents are fully oxidised, and this is probably true at much greater depths. During æons past oxygen has been absorbed as the

earth cooled down, and the product is the crust on which we live.* It is probable that the proportion of oxygen diminishes away from the surface until it disappears almost wholly. What of the deeper depths? Are the comparatively light elements arranged more or less in the order of density? Are we to suppose that silicon and some carbon, aluminium, calcium, the elements chiefly comprising the crust, are those nearer the surface, and iron, copper, and the heavier metals nearer the centre?

Until recently we knew little more than that the earth is some 8000 miles in diameter: that its mean density is 5·6–5·7, and that its relatively thin outer skin, or crust, has approximately the composition assigned to it in the diagram. By a very skilful use of earthquake observations, the eminent geologist, Mr. Richard D. Oldham has, however, lately† given us something like a glimpse within the ball, and concludes from his observations that about five-sixths of the earth's radius includes fairly *homogeneous* material and that the remaining sixth at the centre consists of substances of much higher density. Assuming this to be even roughly true, we conclude that silicon forms probably as great a proportion of this large mass of the earth—whether in the free state or in the forms of silicides—as it does of the crust.

Having thus magnified the office of the important element of which I wish to speak to you, I shall pass to my next point which is how the element can be separated from quartz, or other forms of the oxide, for it is never met with unless combined with oxygen in any of the rocks known to us.

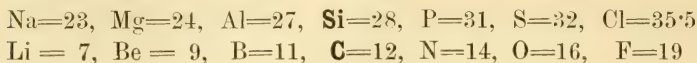
I have already mentioned that quartz is a dioxide of the element—in fact it is the only known oxide—hence if we remove this oxygen we should obtain free silicon. This is not a very difficult matter as it is only necessary to heat a mixture of finely powdered quartz with just the right proportion of metallic magnesium. The metal combines with the oxygen of the quartz, and forms therewith an oxide of magnesium, while silicon remains. If the material be heated in a glass vessel the moment of actual reduction is marked by a bright glow which proceeds throughout the mass. When the product is thrown into diluted acid the magnesium oxide is dissolved and nearly pure silicon is obtained as a soft dark brown powder which is not soluble in the acid. This is not crystalline, but if it be heated in an electric furnace it fuses and on cooling forms the dark crystalline substance on the table, which, as you see, resembles pretty closely the graphitic form of carbon, though its density is rather greater. (2·6, graphite being 2·3.)

* An interesting calculation has been made by Mr. Gerald Stoney, from which it appears that a stratum only 9 feet in depth of the surface of the earth contains as much oxygen as the whole atmosphere. See *Phil. Mag.*, 1899, p. 566.

† R. D. Oldham, F.G.S., "Constitution of the Interior of the Earth." *Quarterly Journal of the Geological Society*, vol. lxii. (1906) pp. 456–475.

SILICON ANALOGUES OF CARBON COMPOUNDS.

The points of physical resemblance between silicon and carbon are of small importance compared with the much deeper rooted resemblance in chemical habits which exists between the two elements. This is expressed in the periodic table of the elements as in the following diagram :—



where silicon is represented as the middle term of a period of seven elements of increasing atomic weights, just as carbon is the middle term of the previous period. The fact is these two electro-negative or non-metallic elements play leading parts in the great drama of nature, silicon dominating that which has to do with dead matter, while carbon is the great organ-building and maintaining element of all living things. While each carries on the work to which it is best suited under existing terrestrial conditions, they both go about it in somewhat similar ways and each one shows tendencies to overstep the border line and perform the other's part. This tendency is for various reasons much more marked in the case of carbon, but I hope to show you presently that silicon is by no means out of touch with living things, and further that it exhibits capacities which render it a potential element of life under other conditions of our planet, but more especially at a much higher level of temperature.

I do not propose to dwell in much detail on the remarkable parallelism of some silicon and carbon compounds, but must refer shortly to a few of them, and the oxides naturally come first.

I have just stated that we know with certainty only one oxide of silicon, the dioxide SiO_2 . This is analogous to the highest oxide of carbon—the well-known CO_2 which plays so important a part in the lives of animals and plants. This familiar carbon compound is a gas under ordinary conditions, but here is some of it in the form of snow. Alongside of this is a vessel containing some finely divided SiO_2 or silica. They are rather like in appearance but they differ greatly in volatility. The CO_2 snow speedily resumes the gaseous state at ordinary temperature, but silica requires a very high temperature indeed even for fusion, however when heated in an electric furnace, the oxide can not only be fused but volatilized. In the fused state it can be fashioned into various shapes and affords most convenient vessels for many purposes, as they are not liable to crack on sudden heating or cooling, and are not attacked by any acid except hydrofluoric acid.

As the difference between the atomic weights of the elements carbon and silicon is only 16 units, silicon dioxide should not differ in volatility nearly so much as it does from carbon dioxide. This and

other considerations lead us to the conclusion that silica as we know it is a molecularly condensed substance represented by the expression $(\text{SiO}_2)_n$, where the value of n is probably at least $=6$.

Before leaving the consideration of the simple oxide, I should like to show you the effect of radium emanations on a disc of colourless quartz. This disc has been exposed by Sir William Huggins to radium for a considerable time, and the brownish discoloration about the centre of it is due to the action of the rays. Whether the latter reduce a portion of the oxide and separate a minute film of brown silicon, or merely attack some trace of impurity in the quartz is not yet known. I have to thank Sir William Huggins for allowing me to show you this interesting specimen.

The chemical analogies of CO_2 and SiO_2 are very close in many respects, for both act as acid anhydrides and combine with metallic oxides and form similar salts. On the one hand we obtain the well-known carbonates, such as common soda crystals, and on the other *silicates*, such as sodium *silicate*. I need scarcely remind you that ordinary window and bottle glass are mixtures of silicates of such metals as calcium and sodium.

When a soluble carbonate is treated with any moderately strong acid CO_2 gas is evolved; but when a soluble silicate such as Na_2SiO_3 is similarly treated no gas is evolved but a gelatinous substance separates. Now this consists for the most part of the feeble acid H_2SiO_3 , which parts with the elements of water gradually, if exposed to air, and affords various lower hydrates, one of the most beautiful being that which we meet with in Nature as the precious *opal*.

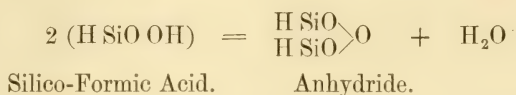
Chloride and bromide of silicon are easily obtained by heating the free elements in the respective halogens, and precisely correspond in composition to the analogous carbon compounds, but, unlike the latter, are intensely reactive to water, and in so far resemble the chloride and bromide of a metal.

If, however, hydrochloric acid gas, instead of chlorine, be passed over heated silicon a very volatile liquid is obtained which is similar in composition to ordinary carbon *chloroform* :—

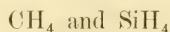
Ordinary Chloroform	CH Cl_3
Silicon Chloroform	SiHCl_3

Silicon chloroform has no anæsthetic effects for a reason you will easily appreciate, when I compare the action of water on the two substances. Under ordinary conditions carbon chloroform is not affected by moisture, hence its vapour can be taken into the lungs unchanged and passes into the system producing its characteristic effects. Silicon chloroform on the other hand is instantly destroyed by moisture, producing free acid, therefore it cannot be inhaled. Nevertheless, the products of the action of water upon it at ordinary temperature are very similar to those which can be obtained by the

prolonged action of water on ordinary chloroform at *high* temperatures. The latter can afford formic acid along with hydrochloric acid. Silicon chloroform affords precisely similar products at ordinary temperatures, but the soluble silico-formic acid immediately changes into the anhydride, and that is the white insoluble substance which has separated in the tube before you. This change may be represented thus :—



Again, both silicon and carbon form gaseous compounds with hydrogen of similar composition :—



Neither of these hydrides can be obtained by direct union of the respective elements, though they are easily obtained by indirect means, with the details of which I need not trouble you. Both are colourless gases as you see. The carbon hydride, or marsh gas, is combustible, but requires to have its temperature raised considerably before it takes fire in air, and its flame is only slightly luminous. It produces on complete oxidation water vapour and carbon dioxide gas. The analogous silicon hydride takes fire much more easily in air, and when not quite pure is even spontaneously combustible under ordinary conditions, and it burns producing water vapour and solid silicon dioxide.

“SILICO-ORGANIC CHEMISTRY.”

Now, just as marsh gas may be regarded as the starting point of that great branch of science which is usually spoken of as Organic Chemistry, so the analogous hydride of silicon is the primary compound from which many substances which are often termed silico-organic compounds can be derived by various means, and these were discovered in the course of the classical researches of Friedel, Crafts, Ladenburg and others.

I wish to avoid using many chemical formulæ, which probably would convey but little meaning to some of those whom I address ; it will suffice to merely indicate the *lines* on which investigations have proceeded in this direction.

In the older work of Friedel, Crafts and Ladenburg, they produced complex substances by the substitution of various radicles (always carbon groups), for one atom of hydrogen in SiH_4 , and ultimately replaced another atom of hydrogen by the OH or hydroxyl group. The substances so formed were silicon *alcohols* which may be

represented in the following manner—A, B and C, being used to indicate the different complex replacing radicles :—



In this way silicon alcohols were built up which proved to be analogous to well-known carbon alcohols, and which afforded analogous acids, etc., on oxidation. These discoveries laid the foundations of a silico-organic chemistry and have been further extended in later years. For example, it has been found possible to pursue the analogy with known carbon compounds in the direction of replacing all the hydrogen in silicon hydride by different radicles, and these changes, which can be effected in successive stages, may be represented in harmony with those just given :—



The two last of these are *asymmetric*, since all four radicles are different. Consequently they should exist in two isomeric modifications, if really analogous to known carbon compounds of the same order, and each form should be capable of acting differently on polarised light.* Dr. F. Stanley Kipping, who has specially investigated this kind of substitution with much success, finds that the analogy between these asymmetric silicon and carbon compounds is complete in regard to optical activity as to other general characters.

SILICON COMPOUNDS INCLUDING NITROGEN.

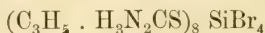
This was all good so far as it went, but some highly important information was still wanting. As you know well the various compounds including carbon and *nitrogen* play by far the most important parts in building up organised structures under the influence of vital energy, but in the silicon series we were almost wholly ignorant of the existence of such compounds until within recent years when I undertook to definitely investigate this branch of the subject.

All that was known at the period of which I speak was that silicon forms a white nitride of uncertain composition when strongly heated in an atmosphere of nitrogen gas; and that when silicon chloride is brought in contact with ammonia and similar substances violent action occurs, but the nature of the products formed was not known owing to special practical difficulties in separating them.

The first step taken was to examine the action of silicon halides

* These changes are represented above as having been effected through the silicon alcohols in order to avoid complicating the general statement, other compounds have in fact been found more convenient for the purpose.

(i.e. chloride, bromide, etc.) on substances free from oxygen, but rich in nitrogen. The earliest of these worked with were Thio-carbamides, but in all these cases the silicon halide merely united with the nitrogen compound as a whole, in some instances producing very curious substances of which the one with Allyl-thio-carbamide



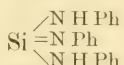
is a good example. This is a liquid which flows so slowly at ordinary temperature that it requires nearly a month in order to fall from the top of its containing tube and find its level at the bottom. Several similar substances have been obtained and examined and their products of decomposition studied, but they do not belong to the class of which I was really in search.

It would weary you to give the details of scientific prospecting which one has to go through in order to attain definite results in a new line of work like this, suffice it to say that success attended the efforts at last, and a finely crystallised and perfectly defined compound was obtained in which silicon is wholly in direct chemical combination with nitrogen, and a specimen of that substance I now show you. Its composition is represented by the expression



where Ph stands for the phenyl group, and its name is Silicophenylamide.

This substance when heated undergoes some important changes, which resemble rather closely similar changes that can be effected in analogous compounds of carbon with nitrogen. Thus it first affords a *guanidine*



analogous to the well-known carbon guanidine, and further a diimide, $\text{Si} (\text{N Ph})_2$, which only needs the addition of a molecule of water to convert it into a silicon *urea*, $\text{SiO} (\text{NH Ph})_2$. Many other substances have been produced similar to silicophenylamide, and they afford analogous products to these just mentioned; but these have been fully described elsewhere, and need not be dealt with here.

SILICON IN RELATION TO ORGANISED STRUCTURES.

The general results of these researches are that we now know a considerable number of silicon compounds including nitrogen, which resemble those of carbon with nitrogen both in composition and in the general nature of the changes in which they can take part. Some of these carbon analogues are closely related to those which are concerned in building up organised structures of plants and animals.

All theories of life assume that its phenomena are inseparably associated with certain complex combinations of the elements carbon, nitrogen, hydrogen and oxygen, with the occasional aid of sulphur and phosphorus. These are the elements of that protoplasm which is the physical basis of life, and by their interplay they form the unstable and complicated groupings of which that remarkable material is composed. All the phenomena we call vital are associated with the change of some protoplasm, and the oxidation of carbon and hydrogen. But it is quite open to question whether the connection of life with the elements first specified is inevitable. We can conceive the existence of similar groupings of other analogous elements forming other protoplasms capable of existing within much greater ranges of temperature than any plants or animals now known to us have to withstand. For example, we can imagine a high temperature protoplasm in which silicon takes the place of carbon, sulphur of oxygen and phosphorus of nitrogen, either wholly or in part. In fact, protoplasm so far as we know it in purest form, always contains some sulphur, and often a little phosphorus, representing a very partial substitution of the kind in question.

In view of our newer knowledge there is therefore nothing very far-fetched in supposing that under suitable conditions a plant or an animal organism, may be able to construct from silicon compounds, ultimately derived from the soil, something akin to silicon protoplasm for use in its structures.

You will now ask me whether there is any evidence that anything of this kind actually occurs in Nature. I think there is, although I admit that the evidence is not very varied as far as we know.

First as to the *Vegetable* kingdom. It is well known that many plants take up silicon in some form from the soil, and use it in ways which my botanical friends tell me they do not at present understand. Silicon is present in the straw of cereals, such as wheat, oats, etc., and in most of the *Gramineæ*. It was supposed that the stiffness of the straw was secured by a siliceous varnish, but this view is not now in favour, as it has been found possible to remove silica from the straw by careful treatment, without diminishing its rigidity. It is also present in the leaves of some palms, for my friend, Dr. Hugo Müller, in the course of his extensive researches on the sugars present in certain palm leaves, has been much troubled by the presence in the extract from the leaves of siliceous compounds of unknown nature. Again, a well-known substance called "Tabasheer," consisting largely of hydrated silica including some organic matter, is obtained at the nodes of some bamboos. What purpose silicon serves in these plants which seem to have special need for it we do not know, but the subject appears to be well worth closer examination than it has yet received at the hands of plant physiologists.

I have on the table some good specimens of Tabasheer, and can

show some portions on the screen which have been rendered nearly transparent by soaking in benzene, and under these conditions exhibit traces of structure.

Next as to the *Animal* kingdom. The most satisfactory evidence that we can at present offer as to the organ-building capacity of silicon comes, curiously enough, from some of the simpler organisms of the *Animal* kingdom, but the only group the short remaining time at my disposal permits me to notice is that of the *Sponges*.

You know that these curious forms of undoubted animal life live in sea-water and are usually anchored to rocks. The sea contains a very minute proportion of silica in solution, and the sponge has the power of appropriating very considerable quantities in the course of its life, and as a part of its normal food supply. What does it do with this silica? It appears to use it in cell production, and from the cell evolves the beautiful and minute siliceous spicules which are so abundant throughout the structure of many of the sponges.

I have here some photographs of these spicules which I have had taken, and shall throw them on the screen. Two of the best of them have been made from microscopic specimens kindly lent to me by Professor Dendy, of King's College, London, who has made a special study of these spicules and of their modes of growth. One of these slides is reproduced in the engraving. (See Fig. 1 on Plate.)

These structures do not represent mere incrustations, but rather definite growths from the cell protoplasm and are themselves in the nature of cells of characteristic forms. Professor Dendy informs me that these spicules in certain cases become surrounded by a horny substance and seem to die, as if by cutting off the supply of energy as well as growing material.

In some of the larger sponges, as in the beautiful *Euplectella aspergillum* or "Venus' Flower Basket," represented in Fig. 2, the siliceous material constitutes the greater part of the sponge, as the soft portion resembles a somewhat gelatinous coating from which the exquisite siliceous structure is developed.

To sum up, then, I have shown that silicon can easily take the place of carbon in many nitrogen compounds, as well as in others not including nitrogen. It therefore seems to me that we hazard no very violent hypothesis in supposing that the silicon which enters the sponge in its food, probably as an alkaline silicate, is in the marvellous animal laboratory made to take the place of a portion of the carbon of the protoplasm from which the spicules are ultimately developed.

The hypothesis is at any rate suggestive, and I hope enough has been said to commend it to your consideration, for there seems to be no doubt that silicon is capable of playing a larger part as an "Organic Element," than we hitherto had reason to suppose.

[J. E. R.]



Fig. 1.

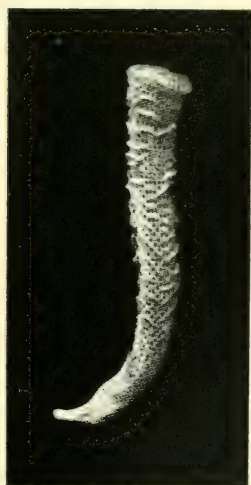


Fig. 2.

WEEKLY EVENING MEETING,

Friday, June 4, 1909.

SIR WILLIAM CROOKES, D.Sc. F.R.S., Honorary Secretary and Vice-President, in the Chair.

PROFESSOR J. A. FLEMING, M.A. D.Sc. F.R.S.

Researches in Radiotelegraphy.

RADIOTELEGRAPHY, popularly called wireless telegraphy, has outlived the tentative achievements of its precocious infancy and obtained for itself a settled but important position amongst our means of communication.

This stage, however, has only been reached after a long struggle with experimental difficulties and much labour in analysing the processes involved. As many of these matters are of general scientific interest, it is proposed, during the present hour, briefly to summarise the results of some recent research.

You are doubtless all aware that every radiotelegraphic station comprises three elements. There is first the external organ called the air-wire or antenna, by which the electromagnetic waves are radiated and absorbed. This antenna consists of one or more wires extending up into the air, either vertically or sloping, or partly vertical and partly horizontal. These wires are insulated at the upper ends and may be arranged fan fashion, or may form one or more nearly closed loops, placed in a vertical position. The antenna is, so to speak, the mouth or ear of the station, by which it speaks through the æther, or by which it hears the ætherial whispers coming to it from other stations. The æther waves are produced by very rapid electric currents moving to and fro in the antenna wires, and these, like the vibrations of a violin string, or the aerial oscillations in an organ pipe, set up a periodic disturbance in the surrounding medium, which in the electrical case consists of alternating electric and magnetic forces taking place at each point in space around the antenna.

There are, then, appliances in the station collectively called the transmitter, which have for their function to create these powerful electric oscillations in the antenna, and to control them so as to send out short or long trains of æther waves in accordance with the dot or dash signals of the Morse alphabet. Lastly, there is the receiving apparatus which, when connected to the antenna, serves to detect the presence in it of the very feeble oscillations which are being generated in the antenna by the powerful oscillations in the antenna of some far-distant sending station. It is usual to employ the same antenna

at any one station both for sending and receiving, and to switch it over from the transmitter to the receiver according as we wish to send or receive messages, although methods have been described and are being developed for using the antenna simultaneously for both purposes.

By way of preface let me illustrate by a few experiments the manner in which these electric oscillations are set up in the air-wire, and the nature of the effects produced by them in the surrounding space. We have here a very long wire which, for the purpose of keeping it within a small compass, is coiled upon an ebonite tube. Two such spirals, H_1 and H_2 , are placed side by side and connected at the bottom through two other small coils of wire S (see Fig. 1). In contiguity to these last two coils of wire are two others, P , which are in series with a condenser or battery of Leyden jars, C , and a spark gap.

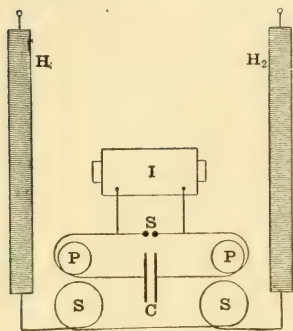


FIG. 1.

If we charge the condenser by an induction coil, I , and let it discharge across the gap, we produce rapidly succeeding trains of electric oscillations in the condenser circuit, and these induce other currents in the open or helix circuit of similar kind. The result is that electricity rushes up and down the spiral wires, which we may consider to represent two very long air wires or antennæ. We have therefore alternately free charges of electricity at the top ends of the wires and electric currents passing to and fro across the middle point. We may compare this movement of electricity in the helix to the oscillations of a liquid in a U-tube when it is disturbed. In the electrical case we have at each spark-discharge 20 or 30 electrical swings or oscillations, separated

by relatively long intervals of silence, the intervals between two swings in the train being about one four-hundred-thousandth of a second, whilst the interval between the groups or trains of swings is about one-fiftieth of a second.

Such electrical oscillations in the wire produce two effects in external space, called respectively electric and magnetic force. In the case of a simple vertical air-wire, the magnetic force is distributed along concentric circular lines embracing the wire, whilst the electric force is distributed along certain looped lines in the plane of the wire. If, however, we employ a close-wound spiral antenna as in our experiment, the positions of the electric and magnetic forces are interchanged as compared with those of the single vertical wire.

As the currents in the air-wire reverse their direction, the magnetic and electric effects in the external space also reverse, but not everywhere at the same moment. The magnetic and electric forces are affections or states of the æther, and in virtue of the inertia and

elasticity of the medium, they are propagated from point to point with a finite velocity which is the same as that of light. We can then explore the field near the antenna, and obtain an approximate idea of its nature and intensity, by the use of a Neon vacuum tube, which glows when held in the electric field with greater or less brilliancy. At certain intervals of distance in the space, the magnetic and electric forces reverse direction in the same way at the same instant, and this distance is called a wave-length.

In the case of a straight air-wire, the magnitude of the forces at considerable distances varies inversely as the distance from the antenna, and the antenna radiates equally in all directions. If, however, we employ a U-shaped antenna, as in the present experiment, the currents being in opposite directions in the two branches, then along a median line transverse to their common plane their actions will neutralise each other, and the radiation will be symmetrical only with respect to the plane of the antenna. In constructing an antenna intended to radiate in all directions, it is necessary to connect the lower end to a large plate of metal or network of wires either sunk in the earth or placed just above the surface. In the former case, this plate is called an earth-plate, and in the latter a balancing capacity. It is necessary that this balancing capacity, if insulated, should be of sufficient size to take up all the electricity which rushes out of the antenna at each oscillation without sensible rise in potential. If we are only employing an antenna of moderate capacity for short distance signalling, then an insulated balancing capacity would not be of unwieldy dimensions and may be constructed of a number of wires stretched out or laid on the ground or insulated a little way above it. When, however, we have to employ a very large antenna of great capacity for long distance work, then the provision of a suitable balancing capacity would involve constructive difficulties which are best obviated by making the earth itself the balancing capacity—in other words, by connecting the base of the antenna to an extensive network of wires or large metal plates buried in the ground. It has been asserted that the direct earth connection damps out the free oscillations in the antenna more quickly than would be the case if an insulated balancing capacity is employed. Although this may be true to a certain extent, we have to set against it the fact that the use of an insulated balancing capacity is out of the question in many cases—as on board ship, where a connection to the hull of the vessel is always made. Also for any but small antennæ the necessary insulated balancing capacity may be somewhat large, and it is in every way better to put it below ground, in other words, to employ an earth-plate and compensate for any slight earth damping by an antenna of rather larger capacity.

This matter is, however, only part of a much larger question, viz. the function of the earth in radiotelegraphy. It is well known that the nature of the earth's soil or surface between the sending and re-

ceiving stations has a great effect upon electric waves passing over it. Various imperfect explanations were given of this action in early days, but the basis for a better knowledge has been laid by the experimental researches of Admiral Sir Henry Jackson and the theoretical discussions of M. Brylinski and Dr. Zenneck. To follow their explanations it must be borne in mind that high frequency electric currents as used in radiotelegraphy are confined chiefly to the surface of conductors by means of which they are conducted. Such a current does not distribute itself uniformly over the whole cross-section of a wire carrying it, but is confined to a thin skin or surface layer. This can be proved by the following experiment. We take a copper wire spiral, or loop, and make it part of a circuit in which a high frequency current exists. If we measure in any way the current in that circuit we find it has a certain value. If we substitute for the copper wire an iron wire of the same size, we find that the current in the circuit is then much less. This can be discovered by placing near the circuit in question another testing circuit, comprising an inductance and a capacity and some means for testing the amplitude of the oscillations set up in this secondary circuit. This decrease is not due to the mere fact that the iron has a greater resistance than copper, but to the fact that the iron is magnetisable, and such magnetisation absorbs energy owing to so-called hysteresis. If, however, we dip the iron for a moment into molten zinc and deposit on it a thin surface layer of zinc, or galvanise it, we find it then becomes almost as good as a solid copper wire for conveying high frequency currents. On the other hand, if we burn off the zinc from a piece of galvanised iron wire, we render it a worse conductor for high frequency oscillations. This experiment proves that such oscillations are conveyed by a thin surface layer of the conductor. In the case of a copper wire for oscillations having a frequency of 1 million, the current penetrates about $\frac{1}{3}$ of a millimetre, and in the case of an iron wire, about $\frac{1}{40}$ mm. into the metal.

For non-magnetic substances the depth to which a current of a given frequency penetrates into a conductor is greater in proportion as the conductivity of the material is less. Hence high frequency currents penetrate further into carbon than into metal. Accordingly a much thicker layer of carbon than of zinc would be needed to shield the iron spiral in our last experiment. The same thing happens in the case of an electric wave propagated over a terrestrial surface. If the surface is a very good conductor, the wave hardly penetrates into it, but glides over the surface. If it is a poor conductor, the wave penetrates into it to a greater extent, and the worse the conductivity the deeper the penetration.

The materials of which the earth's crust is composed, with some exceptions, owe their electric conductivity chiefly to the presence of water in them. They are called electrolytic conductors. Substances like marble and slate when free from iron oxide are fairly good insu-

lators. Dry sand or hard dry rocks are poor conductors, but wet sand and moist earth are fairly good conductors. Sea water, owing to the salt in it, is a much better conductor than fresh water. The following table gives some figures, which however are only approximate, for the specific resistance of various terrestrial materials in ohms per metre cube. It will be seen that dry sand or soils are of very high specific resistance, and damp or wet sand or clay fairly low.

TABLE I.—APPROXIMATE CONDUCTIVITY AND DIELECTRIC CONSTANT OF VARIOUS TERRESTRIAL MATERIALS.

Material.	Specific Resistance in Ohms per Metre Cube.	Dielectric Constant. Air = 1.
Sea water	1	80
Fresh water	100 to 1000	80
Moist earth	10 to 1000	5 to 15
Dry earth	10,000 and upwards	2 to 6
Wet sand	1 to 1000	9
Dry river sand	very large	2 to 3
Wet clay	10 to 100	..
Dry clay. . . .	10,000 and upwards	2 to 5
Slate	10,000 to 100,000	..
Marble	5,000,000	6
Mercury	·000001	infinity

If our earth's surface had a conductivity equal say to that of copper, then the electric radiation from an antenna would glide over the surface without penetration. In the case of the actual earth there is, however, considerable penetration of the wave into the surface, and therefore absorption of energy by it.

Brylinski and also Zenneck have calculated the depth to which electric waves of such frequency as are used in radiotelegraphy penetrate into the sea or terrestrial strata of various conductivities. For mathematical reasons, it is customary to define it by stating the depth in metres or centimetres at which the wave amplitude is reduced to $1/\epsilon = 0.367$ of its amplitude at the surface. I have represented in a diagram some of Zenneck's results calculated for waves of 1000 feet in length, and for terrestrial surface materials of various kinds, conductivities and dielectric constants (see Fig. 2). You will see that in the case of sea water an electric wave travelling over it penetrates only to the depth of a metre or two, whereas in the case of very dry soil it would penetrate much deeper. Owing to the conductivity of the soil, this movement of lines of magnetic force through it, sets up currents of electricity which expend their energy in heat. This energy must come from the original store imparted to the sending antenna, and therefore the wave is robbed of its energy as it travels over the surface.

It should be clearly understood that when a wireless telegraph antenna is in operation, it sends out into the surrounding space a nearly hemispherical electric wave which spreads out in all directions. There are five causes which weaken the wave as it travels outwards:—

1. The distribution of the energy continually over a larger and larger area. The wave amplitude diminishes inversely as the distance, and the wave energy inversely as the square of the distance. This

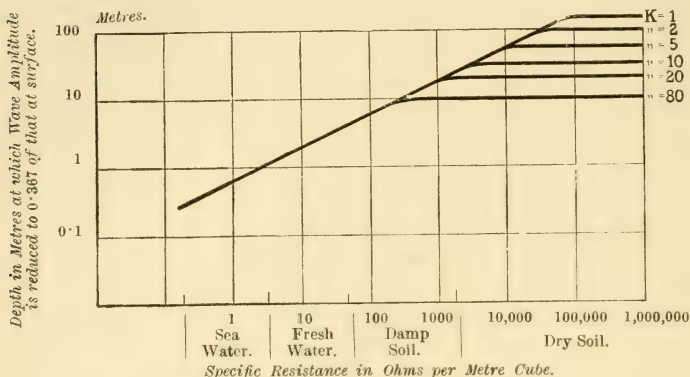


FIG. 2.—DEPTH OF PENETRATION OF WAVES 1000 FT. IN LENGTH.
(Dr. Zenneck.)

is proved theoretically from first principles by Hertz's equations, and has been confirmed experimentally by the experiments of Messrs. Duddell and Taylor, and of Prof. Tissot.

2. There is a certain absorption of energy due to the ionisation of the atmosphere by daylight and to other causes, but this is only detectable over long distances, and for the present moment we shall neglect it. We include, however, under this head, obstructions due to special atmospheric conditions, electrical or material.

3. There is a diminution due to earth curvature which is operative only over long distances.

4. There is some reduction of intensity which results from obstacles—such as hills, trees—especially from cliffs of ironstone or conductive rocks, due to distortion of the electric field.

5. Lastly, there is the weakening due to the dissipation of energy by the penetration of the waves into the surface over which they travel.

We shall consider the last-named cause alone at the present moment. Dr. Zenneck has discussed mathematically, in a very interesting paper, the effect of the conductivity and dielectric constant of the terrestrial surface, soil or sea, on the propagation of a plain electric wave over it, assuming the radiation to be from an

ordinary vertical antenna, and the electric force therefore normal to the earth, and magnetic force parallel to it. The result is to show that there are, broadly speaking, three cases to consider. First, supposing the surface material to be a good conductor, then the wave moves over the surface and penetrates a very little way into it. The electric force in the air over the surface is a purely alternating force vertical to the earth's surface, and the magnetic force is an alternating force parallel to it, and there is very little subterranean electric or magnetic force (see Fig. 3, A). This is realised approximately or most nearly in the case of radiotelegraphy over sea water. Secondly, let the earth be assumed to have a very poor conductivity and not a very large dielectric constant, then analysis shows that the electric force in the air has two components, one perpendicular to the earth's surface and one parallel to it, and the resultant is an alternating and a rotating force, the direction of its maximum value being inclined to the surface and leaning forward (see Fig. 3, B).

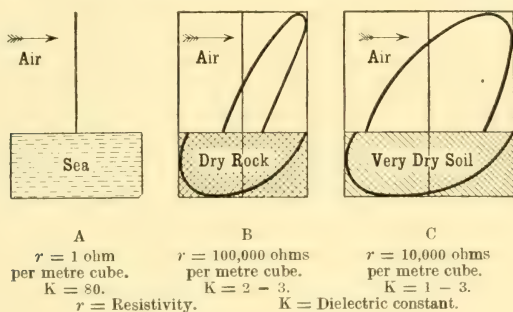


FIG. 3.

The wave-front therefore slopes forward. Also there is a subterranean electric force, showing that the wave is penetrating into the soil, and there is therefore dissipation of energy owing to the conductivity of the soil as the wave travels over the surface. This case is realised when the wave travels over land composed of dry soil having a small dielectric constant. Thirdly, let the earth be a very poor conductor, having a small dielectric constant from 2 to 3, and a specific resistance of about 10,000 ohms per metre cube. For example, very dry earth or sand. Then the investigation shows that the electric force in the air has two components, one parallel to the earth's surface and one perpendicular to it differing in phase, and the resultant is represented by the rotating radius of an ellipse, the maximum value or major axis of which is inclined forward in the direction of the wave motion (see Fig. 3, C). At the same time there is some penetration of the wave into the earth and consequent dissipation of energy.

Dr. Zenneck has considered the case of electric waves 1000 feet in wave length, and has represented the final result by some interesting curves. He defines the effect of the absorption of energy by the soil by stating the distance in kilometres at which the wave amplitude would be reduced by the effect of this absorption to $0.367 = 1/\epsilon$ of its amplitude at the sending station, altogether apart from the weakening due to the spreading of the waves out in a hemisphere, which we may call the spherical or space decrease. These curves are plotted to abscissæ representing the specific resistance of the soil (see Fig. 4). You will see from this diagram that when a plane electric wave having the above wave-length is propagated

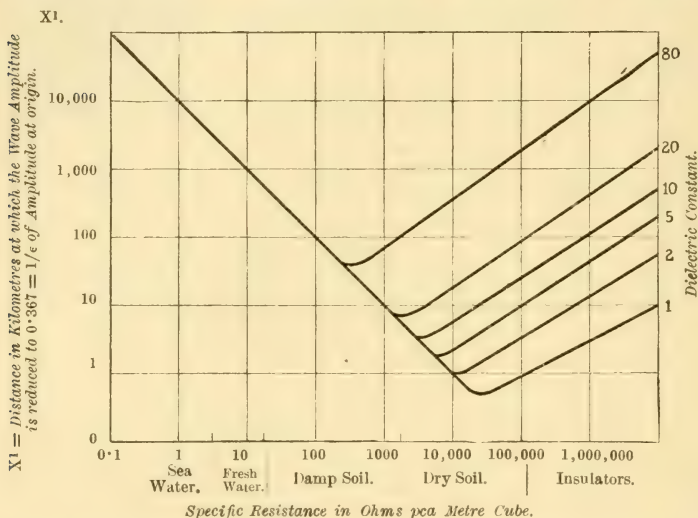


FIG. 4.—CURVES SHOWING THE DISTANCE IN WHICH ELECTRIC WAVES 1000 FT. (300 METRES) IN LENGTH HAVE AMPLITUDE REDUCED TO $1/\epsilon$ BY TRAVELLING OVER VARIOUS SURFACES. (Dr. Zenneck.)

over sea water, it would have to travel 10,000 kilometres before its amplitude would be reduced in the assigned ratio ; and over fairly dry soil, about 100 to 1000 kilometres ; but over very dry soil, having a small dielectric constant, only about 1 to 10 kilometres. Also you will notice that the curves rise up again for still higher resistivities. This, of course, is as it should be. All the practical cases lie between two ideal extremes : the case of an infinitely perfect conducting earth, in which case the waves would not penetrate into it at all ; and the other case, an infinitely perfect non-conducting earth, in which the wave would penetrate into it, but would suffer no dissipation of energy. This theory is quite in accordance with practical experience

in radiotelegraphy. Every receiving apparatus associated with an antenna of a certain height and kind must be subjected to waves of a certain minimum amplitude to give any appreciable signal. For all lower amplitudes that particular receiving arrangement is perfectly deaf. Now it is a matter of common experience that with a given radiotelegraphic apparatus and antenna, it is possible to receive signals for greater distances over sea water than over dry land, and that if the soil is very dry, the distance may be cut down very considerably indeed. This is not due merely to the difficulty of making what the telegraphists call a good earth at the sending station, it is due to the absorption of the wave by the earth for the whole distance which extends between the two stations. Hence, also, it is a common experience that when particularly dry weather is succeeded by wet weather, the radiotelegraphic communication between two stations on land is considerably improved.

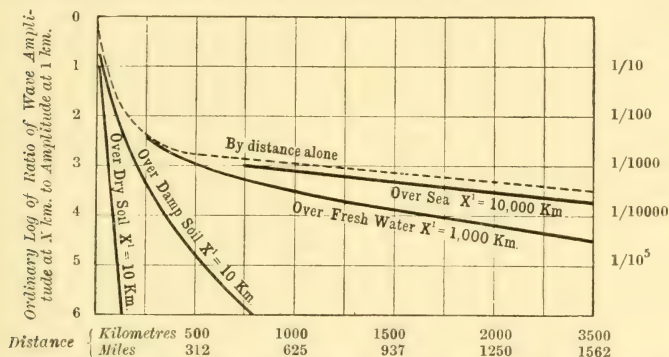


FIG. 5.—CURVES SHOWING DECREASE IN WAVE AMPLITUDE WITH DISTANCE FOR WAVES 1000 FT. IN LENGTH. (Dr. Zenneck.)

In another paper Dr. Hack has shown that even underground water is an advantage in facilitating radiotelegraphic communication. Since a shore station must always be established on shore for communication with ships, it is in consequence generally the custom to select a site for that station as near as possible to the coast, and to take pains to get a very good conducting connection between the foot of the antenna and the soil, and also if necessary between the antenna earthplate and the sea. Fessenden has suggested for this purpose the use of what he calls a wave chute, which is merely a metallic network extending some distance outwards from the antenna in cases where this antenna is established in the centre of towns or dry districts.

Dr. Zenneck has also given a series of curves which show in a remarkable manner the reduction in wave amplitude due to both distance and surface absorption, calculated for waves of 1000 feet in length, and for various coefficients of absorption (see Fig. 5). Thus,

for example, if we are propagating plane waves 1000 feet long over a surface which by itself would reduce the wave amplitude to 0·367 of its initial amplitude in 1000 kilometres, then, when we consider the decrease by distance as well, we have to take account of the fact that this last cause reduces the wave amplitude at 1000 kilometres to 0·001 of that which it is at 1 kilometre distance. I have represented in the diagram some of Dr. Zenneck's curves. The dotted line shows the decrease of amplitude by distance alone, and the firm lines that due to distance and terrestrial absorption in various cases. We are able to see from them the large effect due to travel over large distances of very dry soil. Thus, for instance, if the absorption is such as to cut down the amplitude in the ratio of 1 : 0·367 at 1000 kilometres, then at a distance of 3000 kilometres the amplitude of a wave of 1000 feet in length would be cut down in the ratio of 3000 to 1 by distance alone, but in the ratio of 60,000 to 1 by distance and terrestrial absorption combined.

An important matter is the question of the influence of wave-length on this absorption. It can be shown from theory that an increase of wave-length reduces the energy dissipation by the earth. Thus in certain cases increasing the wave-length from 1000 to 10,000 feet increases the range of effective communication 100 times. The absorption is also determined by the decrement of the wave-train being greater the larger the decrement.

One practical deduction to be made from this investigation is that the reduction in wave amplitude which takes place when the wave moves over very dry soil is as much due to small dielectric constant of the material as to high resistivity. We see also that the wave front is very far from being vertical when the waves travel overland, and hence it is an advantage in that case for the receiving antenna to slope away from the direction in which the waves are travelling or from the radiant point. Lastly, it points to the advantage of a long wave for overland working. Generally speaking, then, we find that electric wave telegraphy is conducted with much greater ease over sea than over dry land, the reason being that the dielectric constant is large and the conductivity of sea water is sufficient to prevent much penetration of the electric wave in the sea, and therefore there is not much dissipation of its energy by absorption due to the surface over which it travels. We have here an instance of economy in Nature. Over sandy deserts, where we can, if need be, put up telegraph posts and wires, radiotelegraphy has had some natural difficulties placed in its way, but on sea, where connection between moving stations is the important matter, and telegraph posts are impossible, special facilities seem to have been afforded us for conducting it.

The next point in connection with the antenna to be noticed is the means adopted of setting up the oscillations in it. The universal custom at present is to excite oscillations in a reservoir circuit consisting of a condenser and an inductance by means of the spark or

arc. If the spark method is used, then the condenser is one of relatively large capacity, and the inductance is kept small. If the capacity is measured in electrostatic units, and the inductance in electromagnetic units, the ratio of capacity to inductance may be something of the order of 5:1 or even 20:1. In this case the condenser is charged by means of an induction coil or transformer, and discharged across a spark gap, and this discharge consists of intermittent trains of electric oscillations with a periodic time equal to the free natural period of the oscillatory circuit. These discharges are made to succeed each other from 50 to 600 times a second, by using an induction coil with an appropriate interrupter, or else an alternator and a transformer. If the arc method of exciting the oscillations is employed, then the ratio of capacity to inductance must be much smaller and the oscillations are excited in this circuit by a continuous current arc worked with a voltage from 200 to 400 volts or more, the arc being traversed by a strong magnetic field and generally being placed in a chamber kept free from oxygen. The oscillations set up in the condenser circuit are then persistent or unbroken. The oscillations are excited in the antenna by coupling it inductively or directly with the condenser circuit (see Fig. 6). If the former method is employed, then an oscillation transformer is used consisting of two coils of wire, one coil being inserted in the condenser circuit and one in the antenna circuit, and according as these coils are near or far apart, they are said to be closely or loosely coupled.

These two circuits have then each their own natural period of electric vibration, like tuning forks, and they have to be adjusted to syntony. It is well known that under these conditions oscillations set up in one circuit immediately create oscillations of two frequencies in both circuits. This action can be easily illustrated by two pendulums which are of the same length and are hung side by side on a loose string distinguished by red and blue bobs. If one pendulum is set swinging, it imparts little jerks to the other and sets the latter in motion, but to do this the first must part with its own energy, and hence is gradually brought to rest. Then the operation is repeated in the reverse direction. The motion of each pendulum may then be represented by the ordinates of a curve such as those in Fig. 7. This kind of motion can, by a well-known theorem, be resolved into the sum of two oscillations of different frequencies. Hence, each pendulum may be said to possess two rates of vibration. The same thing happens in the case of two closely

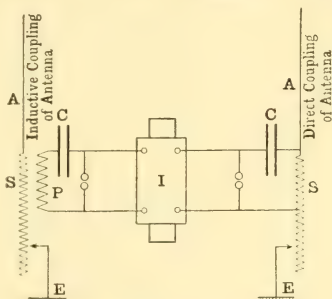
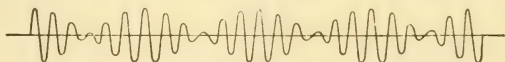
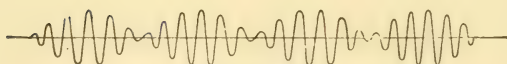


FIG. 6.

coupled syntonie electric currents. If one circuit has free oscillations set up in it, the action and reaction of the circuits generates oscillations of two frequencies. Accordingly, when an antenna circuit is coupled to a condenser circuit, we have oscillations of two frequencies set up in it, and waves of two wave-lengths radiated from the antenna. The presence of these two waves can be detected either by measurements made with the cymometer or by an oscillograph vacuum tube. In the first case all that is necessary is to place a cymometer in proximity to the antenna and vary its oscillation constant. It will be found that there are two settings of the handle for which the Neon tube glows brightly, and the scale of the instrument will indicate the wave-lengths of the two waves respectively. Some instructive measurements of this kind have been made by Professor W. G. Pierce in a recent research, and he has shown that the wave-length given by the formulæ which can be deduced from the theory of the operations are in agreement with actual measurements (see



Red Pendulum.



Blue Pendulum

FIG. 7.

Fig. 8). Another striking confirmation can be obtained by the oscillograph vacuum tube, invented by Dr. Gehrecke of the Reichsanstalt, Berlin. This consists of a glass tube having two strip electrodes in it nearly touching, which are made of nickel or aluminium. The tube is filled with pure nitrogen and exhausted to a pressure of about 10 to 20 mm. If such a tube has a high voltage applied to its terminals, a glow light extends along the electrodes, the length of which varies with the electromotive force. Hence, if the tube is connected to a circuit in which an oscillatory discharge is taking place, the glow light along the tube will rapidly extend and contract. If the electrodes are examined in a revolving mirror, making from fifty to a hundred turns a second, the images of the glowing electrodes corresponding to each oscillation will be separated out, and if the oscillations are persistent or undamped, we see a series of short bright lines alternately above and below a central line. If, however, the oscillations are damped, then we see in the mirror a train of images each decreasing in length (see Fig. 9). On apply-

ing such an oscillograph vacuum tube to the circuit of an inductively coupled antenna, and examining in a revolving mirror the image of

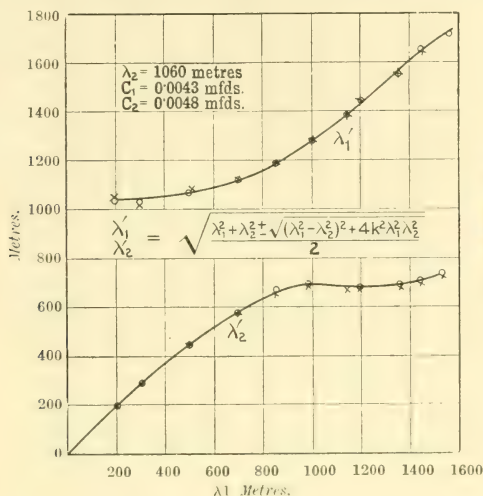


FIG. 8.—PIERCE'S EXPERIMENTS ON INDUCTIVE COUPLING.

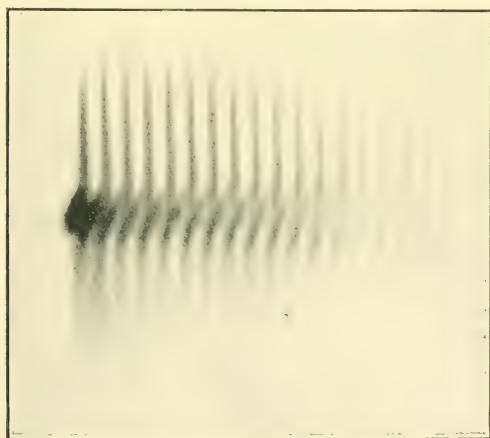


FIG. 9.—OSCILLOGRAM OF DAMPED OSCILLATION (ANTENNA NOT CONNECTED TAKEN WITH THE GEHRCKE OSCILLOGRAPH VACUUM TUBE.

the electrodes, they will be seen to present an appearance as in Fig. 10, taken from photographs kindly given me by Herr Hans Boas

of Berlin. These oscillograms indicate that there are two oscillations present of different frequency, producing an effect similar to beats in music. Owing to the difference in frequency, the oscillations alternately reinforce and extinguish each other throughout the period, and as this type of oscillogram is only obtained with an inductively coupled antenna, it is a proof that in such a case there are two oscillations present of different frequencies. A similar result has been obtained by Professor E. Taylor Jones with low-frequency oscillations in coupled inductive circuits by means of an electrostatic

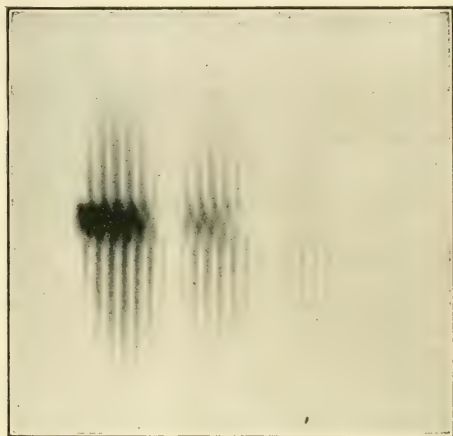


FIG. 10.—OSCILLOGRAM OF SECONDARY OSCILLATION (ANTENNA CONNECTED)
TAKEN WITH GEHRCKE VACUUM TUBE.

oscillogram of his own invention. Looking at these photographs, it will be seen that each represents a single train of damped oscillations gradually dying away, but that in each train of oscillations there is an alternate waxing and waning of the amplitude, which indicates that it may be considered to be composed of two superimposed oscillations of different frequency (Fig. 10A).

Accordingly, in the case of wireless telegraph antennæ inductively coupled, we have in general two waves radiated of different lengths, and either of these can be made to affect suitably tuned receiving circuits. These waves have different damping and different maximum amplitudes.

One of the disadvantages of close inductive coupling is, therefore, that we must divide the energy given to the antenna between two waves of different length. As the receiving antenna is generally only tuned to one of these wave lengths, we then capture and absorb only the energy conveyed by the waves of that wave length. To

meet this difficulty it has been the custom to employ a feeble coupling between the circuits of the oscillation transformer, so as to generate waves of only one wave length. The objection then arises that the energy conveyed to the antenna is much reduced. It is, however, possible, as I have shown, to duplicate the receiving circuits so as to

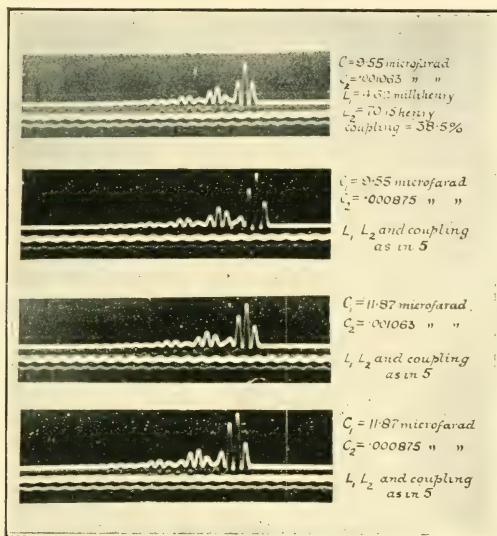


FIG. 10A.—OSCILLOGRAMS OF OSCILLATIONS IN COUPLED CIRCUITS BY PROF. E. TAYLOR-JONES.

capture the energy of both the waves even with close coupling of the transmitter transformer* (see Fig. 11).

A method of creating feebly damped oscillations has, on the other hand, recently been developed, generally known in Germany as Wien's method, or the method of quenched sparks, which is based on the fact that if we can quench or stop the spark in the condenser circuit after the first few oscillations, the oscillations of the antenna then take place freely and with a single frequency (see Fig. 11A).

The principle which underlies this method is the well-known fact, to which particular attention was called by Professor M. Wien of Danzig, in 1906, that the damping effect of very short sparks is

* Since the delivery of this lecture, my attention has been drawn by Mr. J. Hettinger to an article by himself in the "Electrical Engineer" of October 26, 1906, in which he describes an almost identical arrangement devised by him for capturing both the waves of an inductively-coupled transmitter, and refers to a prior invention for the same purpose by Dr. G. Seibt.

extremely large. Hence if we form a spark gap consisting of a large number of very small spark gaps in series, say 10 gaps each of 0.3 mm., and if we keep the spark surfaces cool, then not only can no arc form between these surfaces, but the condenser spark is immediately

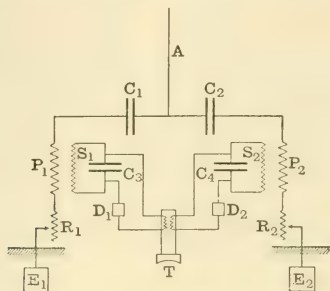


FIG. 11.—METHOD OF UTILISING WAVES OF BOTH FREQUENCIES EMITTED BY INDUCTIVELY-COUPLED TRANSMITTING ANTENNA.

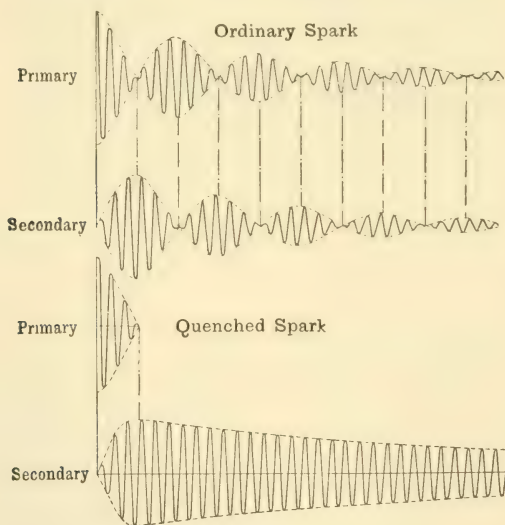


FIG. 11A.—OSCILLATIONS IN INDUCTIVELY-COUPLED CIRCUITS.

quenched. Moreover, if we supply this spark gap, either from a high frequency alternator, or from a low resistance transformer, we can produce as many as 2000 sparks per second. A form of discharger for this purpose has been devised in Germany, which consists of a

series of copper discs or copper boxes cooled with water, the flat surfaces of which are placed in contiguity, but separated by very thin rings of mica. The interspace between the boxes is not more than $\frac{1}{125}$ th part of an inch, and 10 or 12 of these discs or boxes are placed in series (see Fig. 11B). The row of boxes takes the place of the ordinary spark balls, and is connected to the secondary terminals of a transformer, fed by a high frequency alternator, and also connected to an oscillatory circuit. When the transformer is in action it produces a very large number, 1000 or more, oscillatory discharges of

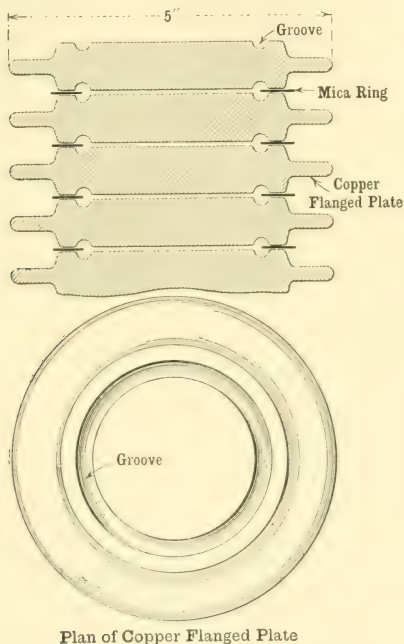


FIG. 11B.—PLAN AND SECTION SHOWING PORTION OF DISCHARGER.

the condenser per second, each of which has a large initial amplitude, but quickly dies out. The inductively or directly coupled antenna hence receives a very large number of impulses per second, each of which sets up in it free electrical oscillations of one definite period.

A discharger composed of a single pair of metal plates with interposed separating paper ring has been devised and employed by Von Lepel. In this case the plates are connected to the terminals of a high-voltage direct-current dynamo, and are shunted by a circuit having inductance and capacity, one of the plates being also connected to an antenna and the other to a balancing capacity.

These dischargers, however, have not stood the test of prolonged practical use, and we cannot say therefore that they are comparable in value for telegraphic purposes with the well-proved inventions of Mr. Marconi.

In connection with spark telegraphy it has been clearly seen, lately, that much can be done by attention to details of construction to increase the number of oscillations in each wave train in the case of spark apparatus, in other words, to lessen the damping by obviating energy losses in all parts of the apparatus. It is not a matter of indifference what kind of glass we use in Leyden jars or what form of stranded wire we employ in oscillation transformers, or type of spark discharger. By appropriate selection of apparatus, we can considerably increase the number of oscillations in damped trains of small amplitude, and therefore increase the possibilities of utilising the principle of resonance.

Before leaving the subject of the antenna we may notice some recent improvements in directive antennæ, that is, in devices for more or less confining the radiation to one direction, and for locating the position of the sending station.

In a previous discourse explanations were given of the property of a closed or partly closed antenna of radiating more in some directions than others, and the action of Marconi's bent antenna was described. Two other inventors, Messrs. Bellini and Tosi have taken advantage of this fact to construct antennæ of a very interesting character. They erect an antenna consisting of two wires, each bent into a triangular form, the top ends nearly meeting, the planes of these triangles being at right angles to one another, and both of them vertical. The nearly closed antenna circuits are then inductively coupled with a condenser circuit, which is capable of being swivelled round in various directions. If the said condenser circuit is placed in such a position as to be coupled with one of the triangular antennæ, it will cause the maximum radiation to take place in the plane of that antenna, but none at all at right angles to it. If it is coupled with the other antenna, it will cause radiation to take place to a maximum degree in the plane of that second antenna. If, however, the oscillatory circuit is placed in an intermediate position, so as to act inductively upon both the nearly closed triangular antennæ, then it can be shown both mathematically and experimentally that the radiation of the combined system is a maximum in the direction of the plane of the oscillatory circuit which is coupled with the antenna. Hence, with such a combined antenna, we have it in our power to create radiation most strongly in one direction, although not entirely suppressed in all other directions. By combining together, however, a single vertical antenna with two nearly closed circuit antennæ at right angles to one another, Messrs. Bellini and Tosi have constructed a complex antenna which has the property of producing radiation almost entirely limited to one-half of the circumjacent space (see Fig. 12). It therefore corresponds to a certain extent in effect to

the optical apparatus of a lighthouse, with catoptric or dioptric apparatus, which projects the light from the lamp largely in one direction. It is not yet possible to make with electric radiation of long wave-length that which corresponds precisely with a beam of light wholly concentrated along a certain cone or cylinder, but it is possible, by the use of a complex antenna as described, to greatly limit the diffusion of the radiation. Since radiating and absorbing power go hand in hand, it is obvious that such a directive antenna also enables the position of a sending station to be located. Messrs. Bellini and Tosi have accordingly applied their methods in the construction of a *radiogoniometer* and receiving antenna, by means of which they can locate the direction of the sending station without moving the antenna, but merely by turning round a secondary circuit into a position in which the maximum sound is heard in a telephone connected with the receiver. By the kindness of Capt. Tosi I am able to exhibit to you their ingenious apparatus (see Fig. 13).

The space occupied by such closed antennæ has hitherto prevented their employment on ships. There is still therefore an opening for the invention of apparatus capable of being used on board ship, which will enable one ship to locate, within narrow limits, the direction of another ship sending signals to it, and therefore of ascertaining immediately the direction from which some call for help is proceeding.

Closely connected with this part of the subject is the question so frequently discussed as to the isolation or secrecy of radiotelegraphic communication. Up to the present moment the only really practical method of isolating any particular receiver so as to make it sensitive only to signals coming from a certain direction, is to avail ourselves to the utmost of the principle of resonance and to tune the sending and receiving circuits to exact correspondence. The question then arises what is it which determines the effectiveness of this tuning. If waves of one particular wave-length are impinging on a receiving antenna and creating signals, by how much can the wave-length be varied or the tuning of the receiver upset or changed without preventing these signals being received? It is clear that the narrower this range, the more perfect the isolation of the receiver. It can be shown that it depends upon the form of the resonance curve of the sending and receiving circuits. If the sending station is emitting waves of a certain constant wave-length and damping or decrement, then in the receiving circuit of all other stations within range there

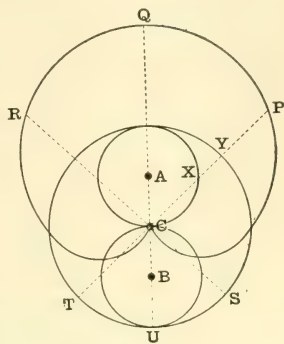


FIG. 12.

will be produced oscillations having a certain mean-square value measurable by appropriate instruments. If any receiving circuit is gradually brought by adjustment of its capacity and inductance into exact syntony or tune with the sending station, then this receiver current reaches its maximum value and there is a definite lesser value of the receiver current for every particular degree of want of tuning or dissonance between the two. The curve which by its ordinates ex-

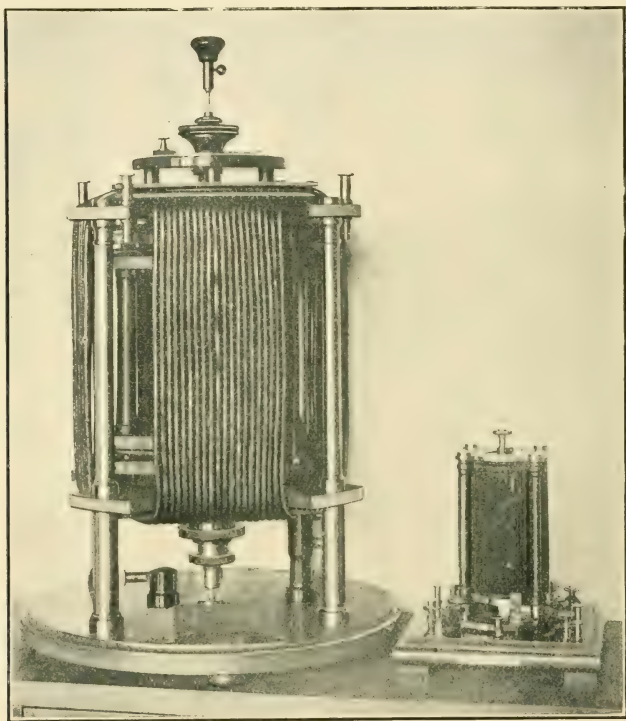
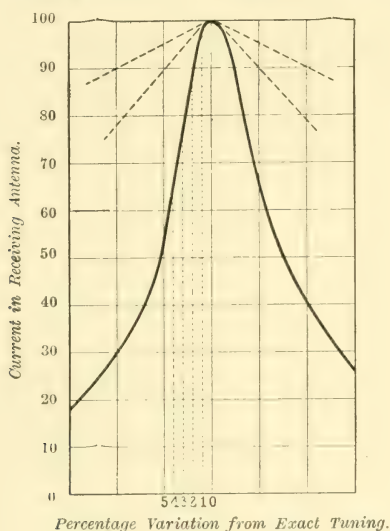


FIG. 13.—BELLINI AND TOSI'S RADIOGONIOMETERS FOR DIRECTIVE RADIOTELEGRAPHY.

presses this receiver current corresponding to each particular tuning or natural frequency of the receiving circuit, is called a resonance curve (see Fig. 14). If this curve has a very sharp peak, then it clearly indicates that a slight want of tuning or syntony between the stations will greatly reduce the receiver current. The peakiness of the curve depends upon the sum of the decrements of the sending and receiving circuits. By the term decrement of a circuit is meant the

logarithm of the ratio of the amplitudes of two successive oscillations in the train.

To obtain very sharp tuning we have therefore to employ either undamped oscillations or very feebly damped oscillations in the transmitter, and also a receiving circuit in which there is as little dissipation of energy by resistance and other causes as possible. It is then possible to cause a change of even less than one-half of one per cent. or five parts in 1000 in the wave-length of the received waves to cease to actuate the receiver. This means that we can distinguish between two waves 1000 and 1005 or 1010 feet in length respectively, and that our receiver may be tuned to respond to one and not to the other. The persistent or undamped oscillations created by



Percentage Variation from Exact Tuning.

FIG. 14.—RESONANCE CURVES.

the arc transmitters have therefore an advantage in this respect over spark transmitters, in that the damping or decrement of the transmitter is less; but it should be borne in mind that the damping of the receiver circuit has also a large influence on the form of the resonance curve, and that good isolation cannot be obtained unless the receiving circuit also has a small decrement. Under favourable conditions we can employ a sending key, which does not interrupt the production of the electric waves at the sending station, but simply alters the wave-length slightly by about $\frac{1}{4}$ per cent. If, then, the corresponding receiving station has a feebly damped receiver, this change will be sufficient to cut up the continuous record or telephone sound at that station into Morse dots and dashes and so transmit

signals. But another station not so tuned will either receive nothing at all or else a continuous unbroken line or sound not having any meaning. There are other methods by which signals not intended for a particular receiver can be rejected by it. Fessenden has described for this purpose an interference detector, in which the impulses it is not desired to receive are made to divide between two paths, the oscillations in which are then caused to neutralise each other's effect on the oscillation detector. On the other hand, the waves of the wave-length it is desired to receive do not so neutralise themselves, but produce a signal by their operation on the detector.

We must pass on to notice in the next place some improvements in oscillation detectors, and means of testing them. As already explained, the æther waves sent out by the transmitting antenna fall on the receiving antenna and create in it or some other circuit connected to it very feeble oscillations. These oscillations being very feeble alternating currents of high frequency, cannot directly affect either an ordinary telegraphic instrument or a telephone, but we have to interpose a device of some kind called an oscillation detector, which is affected by oscillations in such a manner that it undergoes some change which in turn enables it to create, increase, or diminish a local current produced by a local battery and so affect a telephone or telegraphic relay. One kind of change the oscillations can produce in certain devices is a change in their electric resistance, which in turn is caused to increase or diminish a current through a telephone or telegraphic relay generated by a local battery. To this type belong the well known coherers of Branly, Lodge and Marconi, which require tapping or rotating to bring them back continually to a condition of sensitiveness. Coherers, however, have been devised which require no tapping. Thus it has been found by Mr. L. H. Walter, that if a short length of very fine tantalum wire is dipped into mercury there is a very imperfect contact between the mercury and tantalum for low electromotive forces. This may perhaps arise from the fact that tantalum, like iron, is not wetted by mercury. If, however, feeble electric oscillations act between the mercury and tantalum, the contact is improved whilst they last. If, then, the terminals of a circuit containing a telephone in series with a shunted voltaic cell are connected to the mercury and tantalum respectively, and if damped or intermittent trains of electric waves fall on an antenna and excite oscillations which are allowed to act on the mercury tantalum junction, then at each train the resistance of the contact falls, the local cell sends current through the telephone and produces a short sound, and if the trains come frequently enough this sound is repeated and will be heard as a continuous noise in the telephone (see Fig. 15). This sound can be cut up into dot and dash signals by a key in the sending instrument. If the transmitter is sending persistent oscillations, then some form of interrupter has to be inserted in the receiving circuit to enable us to receive a continuous sound in the telephone which can be re-

second, so that, when producing discharges of a condenser, the number of sparks per second may be at least 600, and fulfil the conditions for giving maximum sound in the telephone of the receiver per microampere. Another class of oscillation detector recently discovered comprises the crystal detectors which depend on the possession by certain crystals of the curious property of acting as an electrical valve, or having greater conductivity in one direction than the other, and also on not obeying Ohm's law as conductors. It was discovered by General Dunwoody of the United States Army, in 1906, that a mass of carborundum, which is a crystalline carbide of silicon formed in electric furnaces, can act as a detector of electric oscillations if inserted in the circuit of an antenna, the crystal mass being held strongly pressed between two spring clips, which are also connected by a shunted voltaic cell in series with a telephone. When feeble oscillations are set up in the antenna, a sound is heard in the

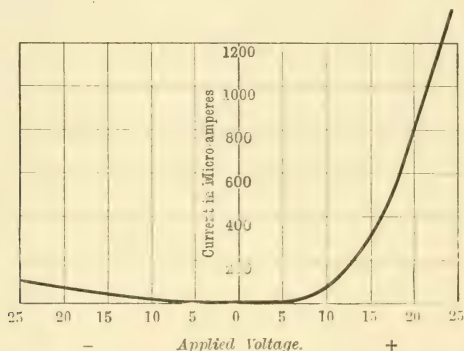


FIG. 16.—CHARACTERISTIC CURVES OF CARBORUNDUM CRYSTAL.

telephone. This property of carborundum has been carefully investigated by Professor G. W. Pierce of Harvard, and he showed that a single crystal of carborundum has remarkable unilateral conductivity for certain voltages when held with a certain contact pressure between metallic clips. Thus for a crystal held with a pressure of 1 kilogram, and subjected to an electromotive force of 30 volts, the conductivity in one direction through the crystal was 4000 greater than in the opposite direction (see Fig. 16). The result of these experiments was also to show that the current voltage curve or characteristic curve of a carborundum crystal is not linear—that is to say, the crystal as a conductor does not comply with Ohm's law, for the resistance of the crystal decreases as the current is increased. Hence the conductivity of the crystal is a function of the voltage acting on it (see Fig. 16A). Accordingly, if we pass a current from a local cell through a crystal under a voltage say of 2 volts, a telephone

being inserted in series with the cell, and if we apply an oscillatory voltage also to the crystal, which varies say between $+0.5$ and -0.5 volt, then the crystal is alternately subjected to a voltage of 2.5 and 1.5 volts, but the corresponding currents would be say 8.4 and 1.8 microamperes, as shown by an experiment with one particular crystal employed by Professor Pierce. The mean current would then be 5.1 microamperes, whereas the steady voltage of 2 volts would only pass a current of 4 microamperes. Hence, apart from the unilateral conductivity, and merely in virtue of the fact that the characteristic curve is not a straight line, we find that such a crystal or even a confused mass of crystals can act as a radiotelegraphic detector. There are, therefore, two ways in which a crystalline mass of carborundum can be used as a radiotelegraphic detector. It consists of a conglomeration of crystals arranged in a

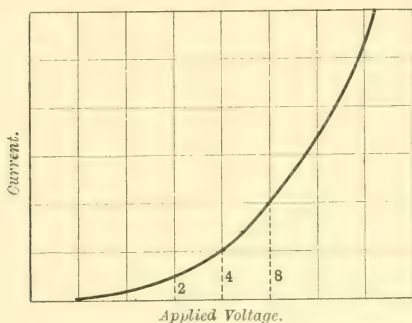


FIG. 16A.

disorderly manner, or not so symmetrically as to neutralise one another's unilateral conductivity. Hence the mass of crystals, like the single crystal, possesses unilateral conductivity, and also a conductivity which is a function of the voltage applied to it. We may then use it without a local cell, and avail ourselves of its valve property to rectify the trains of oscillations in the antenna and convert them into short unidirectional trains which can affect a galvanometer or telephone; or secondly, we may place the crystal between the ends of a circuit containing a telephone and a shunted voltaic cell, and then on passing oscillations through the crystal we hear sounds in the telephone due to the fact that the conductivity is a function of the voltage, and is therefore increased more by the addition than it is diminished by the subtraction of the electromotive force of the oscillations to or from the steady voltage of the local cell. The telephone, therefore, detects this change in the average value of the current by a sound emitted by it. Professor Pierce has discovered that several other crystals possess similar properties to

carborundum—for example, hessite, which is a native crystalline telluride of silver or gold; an anatase, which is an oxide of titanium; and molybdenite, which is a sulphide of molybdenum. As regards the origin of this curious unilateral conductivity, it seems clear that it is not thermoelectric, but at present no entirely satisfactory theory of the action has been suggested.

A number of forms of oscillation detector have recently been invented which depend on the curious fact that a slight contact between certain classes of conductors possesses a unilateral conductivity, and can therefore rectify oscillations. One such detector now much used in Germany consists of a plumbago or graphite point, pressed lightly against a surface of galena. It has been found by Otto von Bronk that a galena-tellurium contact is even more effective. To the same class belongs the silicon-steel detector of Pickard. If such a contact is inserted across the terminals of a condenser placed in the receiving circuit, and if it is also in series with a telephone, the trains of oscillations are rectified or converted into more or less prolonged gushes of electricity in one direction through the telephone. These coming at a frequency of several hundred per second, corresponding to the spark frequency, create a sound in the telephone, which can be cut up by the sending key into Morse signals. According to the researches of Professor Pierce and Mr. Austin it seems clear in many cases that this rectifying action is not thermoelectric, since the rectified current is in the opposite direction to the current obtained by heating the junction.

I may, then, bring to your notice some recent work on another form of radiotelegraphic detector, which I first described to the Royal Society about five years ago under the name of oscillation valve. It consists of an electric glow lamp, in the bulb of which is placed a cylinder of metal which surrounds the filament but does not touch it. This cylinder is connected to a wire sealed through the glass. Instead of a cylinder, one or more metal plates are sometimes used. The filament may be carbon or a metallic filament, and I found some year or more ago that tungsten in various forms has special advantages. The bulb is exhausted to a high vacuum, but of course this means it includes highly rarefied gas of some kind. When the filament is rendered incandescent it emits electrons, and these electrons or negative ions give to the residual gas a unilateral conductivity, as shown by me in a Friday evening lecture given here nineteen years ago. Moreover, the ionised gas not only possesses unilateral conductivity, but its conductivity, like that of the crystals just mentioned, is a function of the voltage applied to it. Hence, if we apply an electromotive force between the hot filament and the cool metal plate, we find that negative electricity can pass from the filament to the plate through the ionised gas, and that the relation between the current and voltage is not linear, but is represented by a characteristic curve bending upwards which has changes of curvature in it

(see Fig. 17). The sharp bend upwards at one place implies a large increase in the current corresponding to a certain voltage, which means that, corresponding to a certain potential gradient and therefore velocity of the electrons, considerable ionisation of the residual gas is beginning to take place. The current, however, would not increase indefinitely with the voltage, but would before long become constant or saturated. It will be seen, therefore, that at points on the curve where there is a bend or change of curvature, the second differential coefficient of the curve may have a large value. Hence, if we consider the current and voltage corresponding to this point, it will be seen that any small increase in the voltage increases the current more than an equal small decrease in voltage diminishes it. If then we superimpose on a steady voltage corresponding to a point of inflexion of the curve an alternating voltage, the average value of the current will be increased. This then points out two ways in which this oscillation valve or glow lamp can be used as a radiotelegraphic

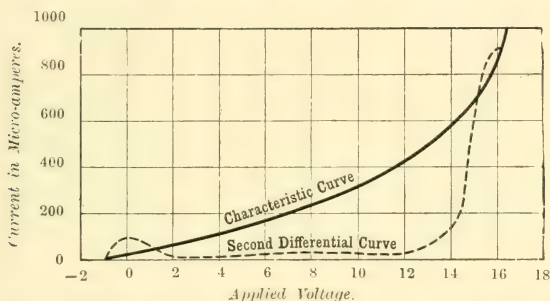


FIG. 17.—CHARACTERISTIC CURVE OF RAREFIED GAS IONISED BY HOT NEGATIVE ELECTRODE.

detector. First, we may make use of the unilateral conductivity of the ionised gas in the bulb and employ the glow lamp with cylinder around the incandescent filament, as a rectifier of trains of oscillations to make them affect a galvanometer or telephone. This method was described by me in papers and specifications in 1904 and 1905. In that case the valve is arranged in connection with a receiving antenna, as shown in Fig. 18, and used with a galvanometer or telephone. Mr. Marconi subsequently added an induction coil and condenser, and employed in 1907 the arrangements shown in Fig. 19. In this case the trains of oscillations set up in the antenna could not by themselves affect a galvanometer or a telephone, but, when rectified by the valve, they become equivalent to an intermittent unidirectional current, and can then affect the telephone or a galvanometer, or any instrument for detecting a direct current.

On the other hand, we may take advantage, as I have more re-

cently shown, of the non-linear form of the characteristic curve. In other words, of the fact that the conductivity of the ionised gas is a function of the voltage applied to it, and in this second method the valve and receiving circuits are arranged as shown in Fig. 20. In

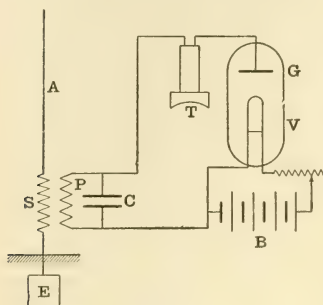


FIG. 18.

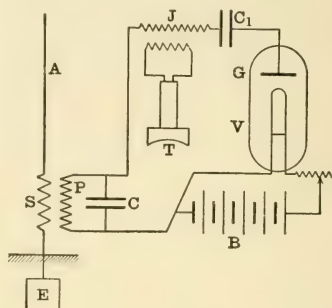


FIG. 19.

CONNECTIONS FOR OSCILLATION VALVE USED AS RADIOTELEGRAPHIC DETECTOR.

this case, we have to apply to the ionised gas a unidirectional electromotive force which corresponds to a point of inflexion on the characteristic curve, and then to add to this voltage the alternating voltage of the oscillations set up by the incident electric waves in the receiving circuit. The result is to cause a change in the average

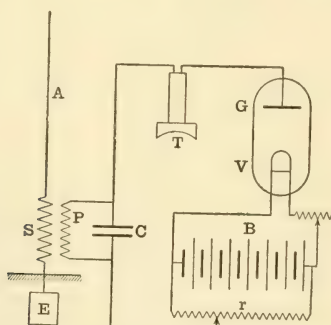


FIG. 20.—CONNECTIONS FOR OSCILLATION VALVE USED AS A RADIO-TELEGRAPHIC DETECTOR.

value of the current through the telephone, and therefore to produce a sound in it, long or short, according to the number of trains of waves falling on the antenna. This last method, then, requires the application in the telephone circuit of an accurately adjusted steady

electromotive force, not any electromotive force but just that value which corresponds to a point on the characteristic curve at which there is a sudden change of curvature.

At this point we may notice a broad generalisation which has already been made by H. Brandes, viz. that any materials such as the crystals mentioned, or ionised gases, which do not obey Ohm's law as regards the independence of conductivity on impressed voltage, can be used as radiotelegraphic receivers. It is necessary to be able to test the relative sensibility of detectors to know whether any new form is an improvement. It is not always possible for an inventor to get these tests made at real wireless telegraph stations. Moreover, it is no use to test over short distances, because then all detectors appear to be equally good. I have found, however, that we can make these comparative tests very easily within quite moderate distances by employing closed sending and receiving circuits which are poor radiators. All the devices called wave detectors are really only oscillation detectors, and we can therefore test their value simply by ascertaining how feeble an alternating current or alternating voltage they will detect. If we then set up in one place a square circuit of wire a few feet inside, and complete the circuit by a condenser and a spark gap, we can set up oscillations in it by means of an induction coil. I find that it is necessary to enclose the spark gap in a cast-iron box, and to blow upon the spark with a jet of air to secure silence, absence of emission of electromagnetic waves direct from the spark balls, and constancy in the oscillatory circuit. I then set up, a few score or few hundred feet away, a similar tuned closed oscillatory circuit, and I connect the oscillation detector to be tested either in this circuit or as a shunt across the condenser. The closed receiving circuit is so constructed that it may be rotated round either of three axes. It is then generally possible to find some position of the receiving circuit such that no sounds are heard in a telephone connected to a highly sensitive detector associated with the circuit. This position is called the zero position. If the receiving circuit is rotated round some axis, it begins at a certain displacement to receive signals, and the angle through which it has to be turned is a measure of the insensibility of the particular oscillation detector being used. I find, for instance, that it is quite easy to take one of my oscillation valves, a magnetic detector, an electrolytic detector, a crystal detector, or any other type, and arrange these in order of their sensibility by means of the device described. Sensibility is not, however, the only virtue which a wave detector should possess. It is important that it should be simple, easily adjusted, and not injured by the chance passage through it of any unusually large oscillatory currents. Another quality which is desirable is that it should be quantitative in its action, and that any change in the amplitude of the wave received should be accompanied by an equal change in the current which the detector allows to pass through the telephone. A quantitative

oscillation detector then enables not merely signals but audible speech to be transmitted. In other words, it can effect wireless telephony. The difficulties, however, in connection with the achievement of wireless telephony are not so much in the receiver as in the transmitter. We have to obtain, first, the uniform production of persistent electromagnetic waves radiated from an antenna : and, next, we have to vary the amplitude of these electric waves proportionately to, and by means of, the aerial vibrations created by the voice

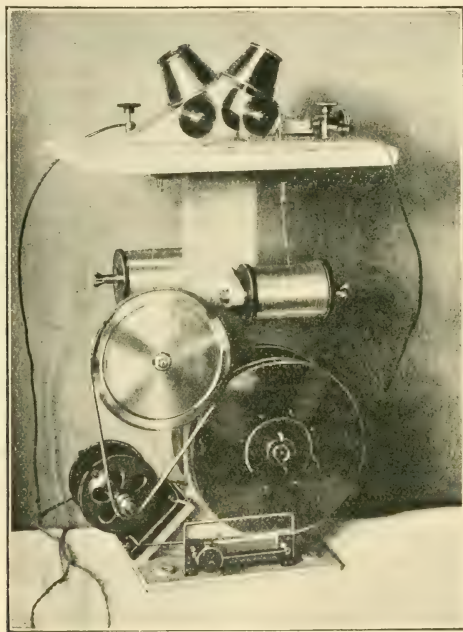


FIG. 21.—ERNST RUHMER'S HIGH-TENSION ALUMINIUM ARC FOR PRODUCING PERSISTENT OSCILLATIONS FOR RADIOTELEPHONY.

speaking to some form of microphone. We cannot employ an intermittent spark generator because each spark would give rise to a sound in the telephone, and these sounds, if occurring at regular intervals, would produce a musical note in the telephone. If, however, we make the sparks run together into what is practically a high voltage arc taking a small current, then, in an oscillatory circuit shunted across this arc, we have set up persistent high frequency oscillations, as first achieved by Mr. Duddell. We can greatly increase the energy of the oscillations by immersing the arc in a

strong transverse magnetic field and also in a hydrocarbon gas, as shown by Poulsen, or we may employ a number of arcs in series. E. Ruhmer has lately also employed a high tension arc between aluminium electrodes (see Fig. 21), shunted by a condenser and inductance as a means of generating persistent oscillations. As an alternative, it is possible to create them by a mechanical method, viz. by a high frequency alternator, subject, however, to certain limitations as to frequency. Both these types of generator have their advantages and practical objections. There is good evidence that radiotelephony has been accomplished over distances of 100 miles or more by each of these methods in the hands of experts, but what is now required is the reduction of the apparatus to such simple manageable and practical form that it can be applied in regular work. The wave-generating apparatus must be capable of producing uniform persistent oscillations of high voltage and frequency, not less than 30,000 or 40,000 per second, or at least above the limits of audition, and the amplitude of these oscillations must be capable of being varied by some form of speaking microphone placed in the oscillation circuit or in the radiating antenna, or in a secondary circuit coupled to it. No ordinary simple carbon microphone will safely pass sufficient current for this purpose. A type of multiple microphone has been used successfully and also a duplex microphone, the invention of Ernst Ruhmer.

It is not, however, possible to speak of radiotelephony at the present time as having reached the same level of practical perfection as radiotelegraphy. But the possibilities of it are of such a nature that it will continue to attract the serious attention of inventors. This is not the place to enter into a full discussion of the causes which limit submarine telephony through cables, but there are well-known reasons in the nature of submarine cables as at present made which impose very definite limits upon it, owing to what is called distortion of the wave form. Electric wave telephony is free at least from this disadvantage, and if (as has been asserted) arc generators can be made self-regulating and capable of being worked for hours automatically, or even for 10 minutes without being touched, then the remaining difficulties with the microphone are not insuperable.

Time does not permit of the discussion of the many other points in connection with radiotelegraphy and telephony which have been the subject of recent work. Much attention has been paid lately to methods of cutting out atmospheric signals due to natural electrical discharges in the atmosphere, which are troublesome disturbers of the ætherial calm necessary for radiotelegraphy. Considerable thought and expenditure have been necessary to discover means for overcoming the difficulties of long distance transmission by daylight, and also those arising from the cross talk of other stations. Much also has been done in training skilled wireless operators both in the Navy and for the mercantile marine work. Radiotelegraphy, like

aviation, is an art as well as a science, hence personal skill is a factor of importance in turning the flank of the difficulties of the moment. Nevertheless, the art and the science of radiotelegraphy are both progressing, and the splendid services already rendered by it in saving life at sea are at once a proof of present perfection and an evidence that the arduous labours of investigators and inventors have borne fruit in yet larger powers to command the great forces of Nature for the use and benefit of mankind.

[J. A. F.]

GENERAL MONTHLY MEETING,

Monday, June 7, 1909.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer
and Vice-President, in the Chair.

Athelstan E. Price, Esq.
George W. Steeves, Esq., M.D. B.A.

were elected Members of the Royal Institution.

The Managers reported, That they had received £250, being 50 per cent. of the Legacy of the late C. E. Layton, Esq., *M.R.I.*

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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WEEKLY EVENING MEETING,

Friday, June 18, 1909.

SIR FRANCIS LAKING, Bart., G.C.V.O. K.C.B. M.D. LL.D.,
Vice-President, in the Chair.

A. HENRY SAVAGE LANDOR, Esq., *M.R.I.*

The Americans and the Panama Canal.

IN the year 1876 the French directed their attention to the possibility of cutting a waterway connecting the Atlantic with the Pacific Ocean. They selected the Panama route because it was the best.

In 1878 a concession from the Columbian Government was obtained.

The Panama Canal Company, with De Lesseps as its President, was organised in 1879 and floated in 1880. The Company was to cut a sea-level canal without locks. A tidal lock near the Pacific was subsequently added, in order to control the flow due to tidal differences of the two Oceans. The tide rises 9 inches above, and falls 9 inches below, mean sea-level in the Atlantic, and 9 to 11 feet above and below mean sea-level in the Pacific.

The construction of a dam was further proposed, in order to control the troublesome waters of the Chagres River. In 1887 a change to the lock-type of canal was made.

The Company became bankrupt in 1889. The New Panama Company resumed the work in 1894.

On May 4, 1904, the United States Government took possession of all canal properties on the Isthmus. There were then some 600 West Indian labourers engaged in the Culebra Cut. A few dump trains and shovels were in use.

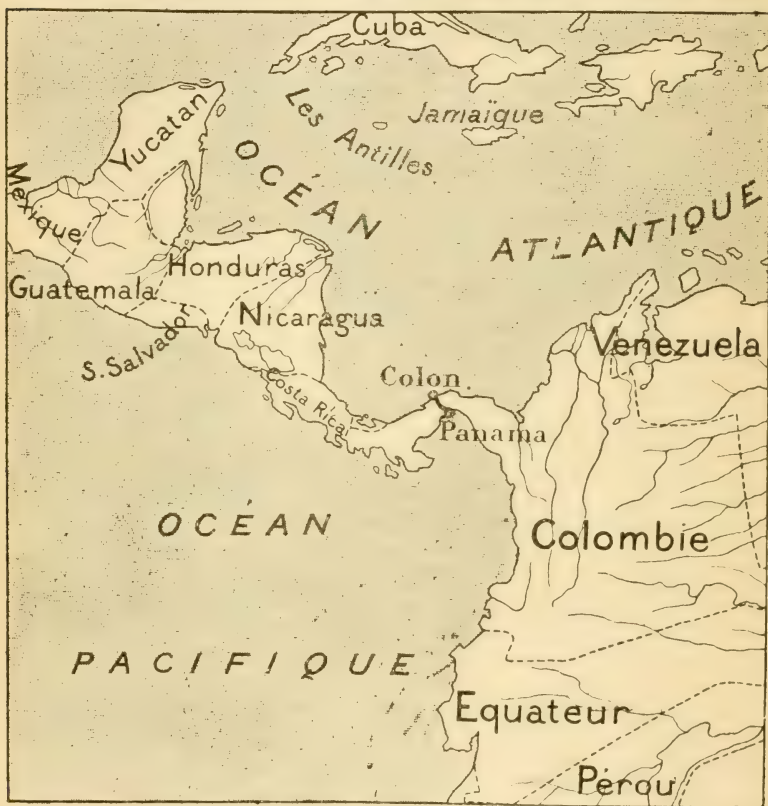
By glancing at the map, representing the Canal as it will be when completed, with its artificial lake filled and held up by the dam and locks, the undulating character of the country will at once strike the observer.

Here is what the latest plan of the Panama Canal adopted by the Americans provides.

Starting from the Atlantic, there is to be a channel 500 feet wide at sea-level, leading from N. to S., from deep water in Limon Bay to Gatun, a distance of 6.76 miles. Here the Gatun Dam,

115 feet high, will be situated, and it will be necessary for ships to be raised by a flight of three twin locks from the sea-level to the level formed by an artificial lake 85 feet above the mean sea-level and filled by the impounded waters of the Chagres River.

Having entered the elevated lake at Gatun, ships will proceed in a southerly direction until they are almost in the centre of the lake.



MAP SHOWING DIRECTION OF CANAL.

precisely above a spot where the rivers Chagres and Trinidad now meet—on what will be the future lake bottom. Here the course will be altered to S.E., a general direction which, with unavoidable detours such as the one at Bohío and Frijoles, the canal will maintain until Bas Obispo is reached, a distance of 23.59 miles from Gatun. Navigation across the lake will be in a channel from 500 to 1000 feet wide,



THE PANAMA CANAL AS IT WILL APPEAR WHEN COMPLETED WITH ARTIFICIAL LAKE FILLED.

the difference in the width being due principally to curves in the proposed route.

At Bas Obispo begins the channel through the Continental Divide. In this section through the hilly region, and for a distance of 8.11 miles, the Canal will be 300 feet wide, the surface of the water being on the same level as the lake. The Canal in this portion goes through what is called the Culebra Cut.

At Pedro Miguel, on the Pacific side of the Divide, vessels will be lowered in one flight of twin locks from the 85-foot level to a small lake 55 feet above sea-level. A channel 0.97 mile in length and 500 feet wide will convey ships to Miraflores, where twin locks of two flights, and of identical dimensions with those at Gatun and Pedro Miguel, will bring ships to sea-level in the Pacific Ocean. The Canal, 8.31 miles in length from Miraflores to deep water in Panama Bay, will have a channel 500 feet wide and 45 feet deep at mean tide.

Up to May 1, 1909, 73,124,849 cubic yards of soil had been removed. There remained 101,541,746 cubic yards to be excavated before the completion of the Canal.

Where the land will be submerged by the proposed lake, the work is simple enough, and as the line of the Canal follows in a general direction the course of the Chagres River, the work consists mainly in levelling down any irregularities of the bed that rise above the 40-foot level of this region. The Canal line is already cleared of vegetation. Preparations are made for an anchorage basin immediately south of the Gatun Dam.

Considering the gigantic nature of the undertaking, the difficulties of construction are comparatively small. No difficult problem faces the engineers, except, perhaps, the construction of the Gatun Dam. The other portion of the work—except for the quantity of it and the time and patience required to accomplish it by excavation in the dry, or by dredging in the wet—presents no obstacle. The excavation is easy, as the soil is comparatively soft. The rock when exposed to the air disintegrates quickly.

The Gatun Dam has been the source of much speculation, and is, perhaps, the chief point of interest at present in the Canal zone.

It is true enough that, if the construction of the dam were to prove defective, if the calculations of the engineers were in any way erroneous, the disaster which might result would have no parallel in history, unless, perhaps, Noah's Deluge. Should the Gatun Dam give way after navigation had been established across the Isthmus, just imagine what would happen in the artificial lake and canal. Some vessels would be swept away on to the lowlands at the foot of the dam; others would remain high and dry, resting at various angles against the walls of the Culebra Cut or on the dried lake bottom. We will not refer to the fate of the inhabitants of Colon and Cristobal towns, some of whom are already beginning to feel anxious now that they see the dam go up instead of coming down.

Personally, after a careful inspection of what is being done, I have confidence in the work of the American engineers. There have been slides, it is true, owing to subsidence of the soil under what is called the "South rock-toe." I believe that there have been five slips at that point, the accumulated rock settling down for a few feet until it reached an angle of repose on a harder stratum below. A great deal of fuss has been made by newspapers over the last mishap, which indeed is of no engineering importance. A thousand more serious slips occur every year along railway embankments and roads, and no one takes the slightest notice of them.

The dam as it is being planned by the Americans is so big that to my mind nothing will move it. If it does sink at all in some portions owing to the instability of the substrata on which it is built, it will sink before it is finished, when there will be plenty of time to repair it before the lake is filled. In its entirety I do not personally see how it could possibly give way, except, of course, under extraordinary circumstances, such as a severe volcanic commotion, a general subsidence of the entire region, or some such catastrophe.

You are told that Panama is an earthquake region. In Panama city is the old San Domingo Church destroyed by fire. In the interior of this church one can admire a curious architectural feat in the shape of a flattened arch with an elongated span of 40 feet. It could never have remained in its position for nearly 200 years if earthquakes had been violent in the Canal zone or near it.

The Gatun Dam when completed will follow an irregular line, almost like an unfinished letter "S." The spillway will occupy almost the centre of the dam, controlling and conveying in a N.W. direction the surplus water into the West diversion. The excavation of the Spillway is nearly completed. The floor of the Spillway is to be covered with concrete for its entire width of 285 feet, and for a distance of 960 feet north of the Spillway Dam. The thickness of the floor will decrease from its maximum of 4 feet at the dam to 1 foot at its northern end. Concrete is being laid at the rate of 299 cubic yards a day.

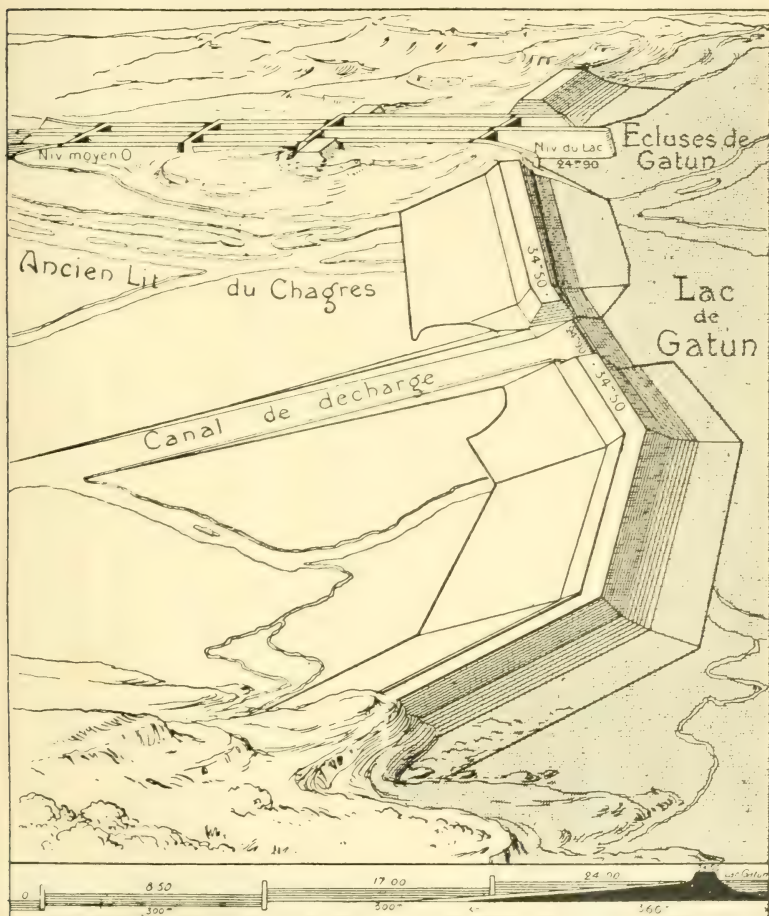
Towers for the unloading cableways were in course of construction at Gatun. One set of the duplex towers had a complete cableway and the main cable of the duplex line in place. One of the unloading buckets had been hung. Seven of the eight towers of the Gatun lock cableways had already been erected, and the eighth was nearing completion.

On the S.E. side of the dam will be the lake, at an elevation of 85 feet above mean sea-level. A set of three twin locks will raise ships from sea-level to the 85-foot level lake, or lower them from the lake to the sea-level, as the case may be. The locks are situated in the hillside, against which the eastern end of the dam is solidly resting.

In order to prepare for the two parallel sets of three locks at

TOPOGRAPHY IN THE VICINITY OF GATUN.

Gatun, an excavation of 5,139,304 cubic yards is necessary. Some 3,500,000 cubic yards have already been taken out. The laying of 2,096,000 cubic yards of concrete will commence early in August



DRAWING SHOWING GATUN DAM WITH LOCKS AND SPILLWAY AS THEY WILL APPEAR WHEN COMPLETED.

1909. The three twin locks, extending from the dam in a due northerly direction, will each be 1000 feet long, 110 feet wide, and with $41\frac{1}{2}$ feet of water over the sills.

The dam, according to the latest plans, is to be 1200 feet wide at

the base between lateral toes of piled rock. The two toes of rock have already been completed over half the width of the valley. The length of the dam will be $1\frac{1}{2}$ miles, its maximum height 115 feet.

The material of the earth's crust in the Gatun region can be roughly classified as sedimentary, metamorphic (merely limestone), and igneous rocks, the first subdivided into conglomerate (a consolidated gravel) and sandstone. The igneous subdivisions are trap (a basalt), lava flow, and rhyolite, or fragmentary tufa and volcanic ash.

So far, the great question of Panama resolves itself into whether it is preferable to build a sea-level or a lock canal.

The strongest advocate of a sea-level canal—"The Straits of Panama," as he calls it—is a most eminent French engineer, M. Bunau-Varilla. He is a man of extensive experience in the construction of dams, harbour works, and railways. His life work has been in connection with the Panama Canal. He has thrown his entire heart and soul into studying the possibilities of constructing a practical waterway across the Isthmus.

I had a long conversation with M. Bunau-Varilla in Paris, and was impressed by the genius of this man. What he says of locks and sea-level canals is true. The Americans might do worse than ponder over the statements made by M. Bunau-Varilla.

There are certainly disadvantages in a lock canal which are not to be found in a sea-level canal, such as the damage that may be done to locks by a crippled ship, by unexpected explosions, by treacherous hands, by submarine vessels, or by airships overhead: by possible, if perhaps not probable, volcanic commotions, or by the collapse of the dam. It must be borne in mind that with the increase in the dimensions of vessels, the margin which is to-day reckoned as ample, for instance, in the construction of locks, may be found insufficient in a few years to come.

One should, on the other hand, not lose sight of the immense economy of time which the construction of a lock canal will afford. If all goes well, a lock canal will, for the present, answer just as well as a sea-level transcontinental waterway. Moreover, it can at any time be transformed into a sea-level canal by dredging in the wet.

Personally, I greatly admire M. Bunau-Varilla's highly scientific prophecies, and fully recognize their wisdom. So do, I think, almost all American officers on the Canal Zone. I am not certain whether the Americans, hampered by their laws and restrictions on labour, would be able to work the dredging operations at the low figure, quoted by M. Bunau-Varilla. It must not be forgotten that it costs an American four times what it would cost a European to do anything. One could see, lying in the Canal Zone to rust and waste several hundred thousand dollars' worth of old French machinery which, because of peculiar American regulations, could neither be sold for old iron nor used. This could not happen in other countries,

I think that M. Bunau-Varilla's method of disintegrating rock under water by mechanical concussion, instead of by the old mode of dynamite mines which shattered the rock into small fragments, is most excellent, and should certainly be cheaper than excavating in the dry. Transport of materials by water is undoubtedly less expensive than over land. Everybody will, I think, fully agree with the American engineers and with M. Bunau-Varilla that it would be folly to contemplate excavation in the dry of a sea-level channel across the Isthmus of Panama. It would take from forty to sixty years at least to complete it, and it would be unwise to prophesy its eventual cost. With the powerful dredges which are to-day built, such a thing is rendered more possible. There is no reason why, as M. Bunau-Varilla says, dredges which now float in harbours and rivers should not float and operate in an artificial lake. M. Bunau-Varilla's plan, which is excellent, provides for provisional locks and a steel and concrete dam for the Chagres. Where the river would thus form falls, electricity to be used as a motive power could be generated at little cost. A lake 200 feet above sea-level would be formed, providing in itself a suitable dump for the waste. This would prove economical as well as simple.

M. Bunau-Varilla is a man whose words cannot be neglected. He has valiantly defended the certainly *ideal* form of canal, the open, wide, deep passage between the two oceans entirely freed of any artificial structures such as locks and dams—"The Straits of Panama," as he rightly calls them, in contradistinction of a tide-locked "Sea-level Canal."

Now let us see what the American engineers are doing.

Mr. C. M. Saville, the well-known American engineer, made for the Government an interesting investigation of the foundations of the dam and spillway, especially with the object of searching whether a permeable connection existed through the alluvial deposits in the relatively narrow valley across which the dam is now being constructed. The dam will rise between swamp areas to the north and the broad flats of the Gatuncillo and Chagres River to the south.

The alluvial deposits in the gorges of the Chagres River, across which the dam will be built, are composed for a depth not surpassing 80 feet almost entirely of fine sand. Under this for some 100 feet borings show a thick deposit of blue clay with sand and shells. Below this and directly overlying the rock is a conglomerate deposit of stones, angular gravels and sand, solidified and finely cemented into a solid mass with finely divided clays and silts, which are believed to be the product of decomposition of the immediate rock surface. This layer is waterproof and non-water-bearing, having been rendered so by the leaching and filling of the overlying deposits. According to excavations and borings, no continuous layer of loose sand or gravel has been found, and no deposit has been encountered that appears sufficiently extensive and permeable to

endanger the Gatun Dam when the artificial lake is filled. The composition of the upper stratum does allow water to pass through, and it will be deemed advisable, even imperative, to drive a cut-off wall of sheet piling right across the valley through this permeable material. According to American engineers this will prevent percolation and afford additional resistance.

On the sound construction of, and the eventual resistance offered by this wall will, I personally think, rest the safety of the entire dam.

Tests of permeability and frictional resistance have been made with the various rocks predominating in the vicinity of Gatun. We have there blue sandstone, volcanic ash, volcanic tufa, dark and brown conglomerate, light blue-grey conglomerate, argillaceous, grey and light brown sandstone.

These tests were made in order to determine the permeability of the rocks under various pressures and the loss by erosion when exposed to a pressure of 40 lb. (equal to a head of 92.3), the maximum head to which they will be exposed in contact with the waters of the lake. The weight and specific gravity per cubic foot when dried at a temperature of 212° F. and when saturated was accurately registered, as well as the absorption in 24 hours, and the abrasion, showing the loss in 15 minutes under a 40 lb. pressure.

Permeability and abrasion tests have also been made to determine the frictional resistance of the soil to water in the Gatun region.

The coarse-grained blue sandstone in which volcanic ash could be detected was highly porous, brittle, and crumbled easily. Water passed through freely.

The soft grey volcanic ash was somewhat firmer. It contained a considerable amount of sand and gravel. It eroded easily.

The hard compact volcanic tufa, resembling rhyolite, became soft and disintegrated when exposed to the air. It proved impervious under a pressure of 90 lb., and eroded but little in contact with water.

The dark conglomerate, resembling argillaceous sandstone with a small percentage of large gravel, was practically impervious, but eroded easily. So did the heavy brown conglomerate of gravel cemented by volcanic ash.

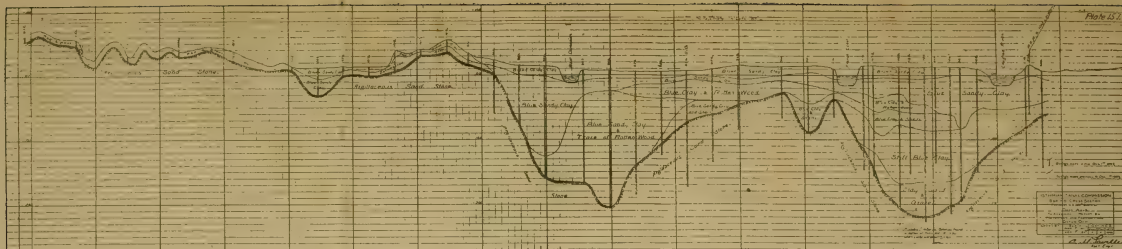
Then we find the soft light grey volcanic ash from the Gatun lock site; very fine-grained, easily crushed between one's fingers, and easily washed away when openly exposed to pressure.

The dark blue argillaceous sandstone from the Spillway test-pit was of medium hardness, but disintegrated rapidly when exposed to the air. Its large percentage of clay made it impervious, but easily eroded.

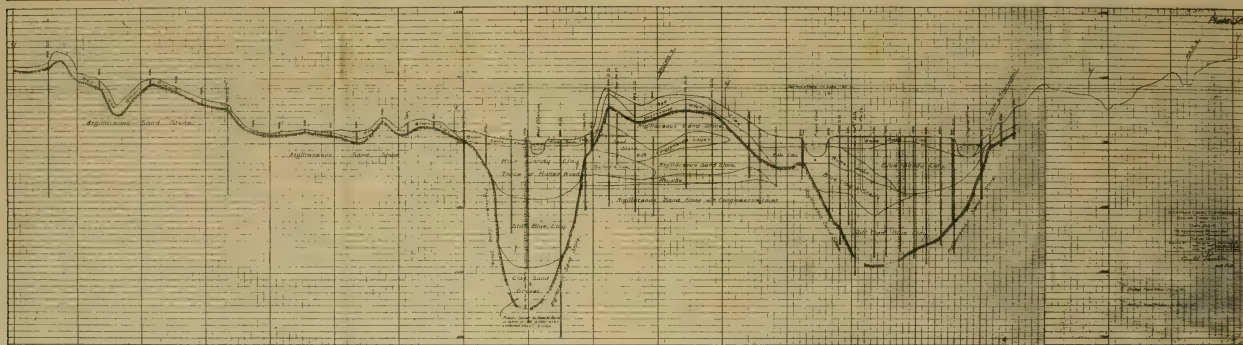
Both the light blue sandstone and the light brown sandstone from the Spillway test-pit were only moderately pervious but easily eroded.

Various interesting practical tests have been made with experi-

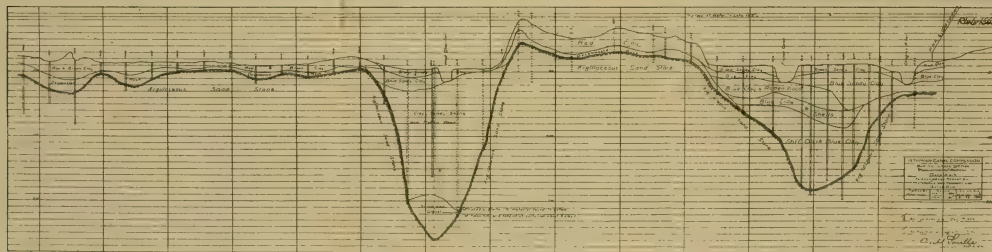
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1.—BORING CROSS SECTION PARALLEL TO AND 300 FEET SOUTH OF DAM AXIS.

2.—BORING CROSS SECTION ON DAM AXIS.

3.—BORING CROSS SECTION PARALLEL TO AND 400 FEET NORTH OF DAM AXIS.

mental dams in order to thoroughly ascertain the reliability of the available materials as well as of the soundness of the methods employed in the construction of the Gatun dam.

The site of the proposed dam at Gatun has, I think, been minutely surveyed, and complete information regarding conditions underground has been obtained by numerous test-pit borings. The surface soil is naturally weathered and more or less disintegrated by vegetation, but solid ground is found by stripping the area under the future base of the dam to a depth of not more than $1\frac{1}{2}$ to 3 ft.

Two deep test pits have been dug. The strata encountered were similar to those found in drilling and excavation both in the spillway and lock site. They were badly seamed. Quantities of water leaked through the seams. It is believed that the bulk of the water is held in the disintegrated portions of these strata and finds a passage from one plane to another along the joints.

Forty to fifty feet below sea-level a layer has been found of rhyolite apparently forming a firm and solid stratum, and it is believed that, by letting down a curtain wall as far as this layer, ample protection will be obtained against any appreciable entrance of water from the lake through the seams and cracks.

The second test-pit in the centre of the dam 80 feet below sea-level (this means 90 feet lower than the surface of the ground) showed from the surface the following successive strata: brown sandy clay, blue and yellow clay, blue sandy clay, a stratum of blue clay with decayed vegetation, and down at 60 feet below the surface a stratum of blue clay with shells. Comparatively little water was encountered. From other borings it would appear that the entire section of the Gatun Gorge upon which the dam will be built is composed of materials similar to those exposed in this pit.

Cross sections are given to illustrate this paper in order to show the geological formation under the centre of the Gatun Dam axis. One drawing shows the formation 300 feet south of the dam axis; the other diagram represents the boring cross-section 400 feet north of the dam axis.

The vertical continuous double lines show the borings made since December 1907. The vertical dotted double lines mark the borings previous to December 1907.

Drilling has been employed in order to bring geological samples to the surface, and this method, with a continuous drive sample from the surface for purposes of comparison, has given the best information regarding the materials underground. Rhyolite is perhaps the most impervious rock found at Gatun, and does not readily yield to disintegration. As a rule all the conglomerates found at Gatun weather badly and soon crumble to gravel when excavated. They are solid enough when protected from the atmosphere.

Much has been said of "artesian water," or water under pressure, occurring in the Gatun Valley. A great number of wash-drill holes

have been bored. Water has been found in some of the bore-holes ; in others it overflowed the casing. The pressure of artesian flows in the valley, which was fully expected, does not necessarily mean that the dam will be endangered by them if the necessary precautions to safeguard its stability are taken—precautions which are ordinary with the principles of earth-dam construction. According to American engineers, the geological sections at right angles to the axis of the Gatun Dam, as well as to the three sections parallel with the dam axis, conditions seem rather against a tendency to a free underground flow. It is believed that at Bohio or Gamboa there is not a sufficient elevation in the valley of the Chagres river to force an appreciable body of water through the alluvial deposits at Gatun. The inclination is to believe that there is merely a level hydraulic grade line between Gatun and Bohio—this, of course, on the supposition that there is a free underground connection between the two places. The velocity of the flow under such conditions could but be slight ; for, were it not, on reaching the narrows of Gatun, it might prove destructive to the underlying and easily dissolved strata.

It is, perhaps, on the surface of the rock that are found the most pervious conditions. The rocks are seamed and jointed, conducting the water to the bottom of the gorges, where it remains under pressure.

There seems to be no indication of a continuous underground flow parallel with the current in the Chagres Valley, or else deposits of continuous gravel should have been found in the borings. Moreover, a careful investigation has failed to establish any continuous underlying water-bearing stratum. The water found below the surface of the valley derives its pressure, it has been ascertained, from the hills surrounding the valley. The layer most to be feared, on account of its water-bearing capacity, is a stratum of sandy clay relatively near the surface.

Allowing that the wall of masonry below the foundations of the dam will effectively prevent the underground flow from passing below these foundations and eroding them in time, it still remains to be seen whether the water may not still find a passage by fissures in strata further down. No doubt American engineers have given full attention to this point.

There was one point which I personally feared more than any other. It was whether the hills surrounding the lake would actually keep the water in place, or whether they would let it escape through their fissured mass.

Colonel Goethals seems to think that the hills will not leak. He states that the question has been thoroughly studied. The reservoirs, he says, constructed in the hills of the same geological formation do not show the slightest signs of leakage. I am not prepared to judge whether that reasoning would hold good for the larger lake.

Whether the Americans build "the Straits of Panama" or a lock

canal, is really immaterial. Both ways are good. One is much more extravagant in time than the other, that is all. Taking all things into consideration, I think the Americans are right in selecting the cheaper and quicker way of opening a waterway between the two oceans, the lock canal. The Americans need a canal from the Atlantic to the Pacific now, and they cannot wait. There is no reason at all why a lock canal, however good and expensive, should not gradually be transformed, by dredging in the wet, into a sea-level canal. It would not interfere much with navigation, and it would certainly simplify matters in the end.

I am not at all anxious about the dam giving way in the immediate future, but I am curious to see whether the calculations regarding the filling of the lake come true, and whether the porosity and permeability of the lake-sides will not perhaps present unexpected surprises.

As will be seen by the diagram illustrating this paper, showing the proposed Gatun Dam in its maximum section, the central portion of the dam will consist of hydraulic fill, with a padding of cheap filling between the slope of the central hydraulic filling and the surface rock-fill. Rock-fill of a cheap description, but quite good enough for its purpose, will be used on the outer face of the dam, but not so on the water side of the dam, which will be protected by a 10-foot thickness, all along the face and summit, of selected rock from Bas Obispo. The rock piles forming each toe of the dam will be constructed of selected rock-fill.

In the company of Major Gaillard, the engineer in charge of the central division, and also alone, I visited several times the Culebra Cut.

The cut is 9 miles long across the highest part of the Continental Divide. Its width at the bottom is 300 feet. The lowest depth above mean sea-level is 40 feet. It is here that the French began their work in 1881, and continued it under the new French company until 1904, when the Americans took over and continued the work with the French equipment. The French had done a great deal of work there, They had excavated some 50,361,000 cubic yards of material.

With the arrival of powerful steam shovels and improved equipment, the progress made in that portion of the canal by the Americans has been amazing. In 1908, 13,917,433 cubic yards were excavated, and in the first four months of 1909, 5,147,944 cubic yards were removed.

This extraordinary progress is not altogether due to the improved and more plentiful machinery, but also to the nature of the rock, which is more readily extracted than the slippery clay which formed the upper strata of the Culebra Mount when the excavation first began. M. Bunau-Varilla was telling me most graphically of the anxious efforts which had been made by the French under him in endeavouring to pierce the mass of soft slippery soil until the solid core could be reached where railway tracks could be established on

firm ground. The principal trouble seems to have been that the clay moulded itself in the buckets and refused to drop off, thus requiring endless patience, time and labour.

A most perfect system of hauling materials from the cut is in operation, and the handling of the dirt on the dumps is carried on without loss of time. An excellent drainage system has been perfected so that the rainy season causes no delay in the excavation.

The greater width of the canal adopted by the Americans necessitated the establishment of diversion channels further away from the cut than those dug by the French. "The Obispo" diversion, on the East side, drains a region of over 9 square miles. On the West "the Camacho" drains $6\frac{1}{2}$ square miles. The Camacho diversion, 4 miles long, was completed in 1908. Each takes the run-off from the summit of the divide to the Chagres River.

The French diversion channel for the Camacho River (on the west side) has been utilised. A new channel riveted with stone has been cut where it runs through Whitehouse yard; the French tunnel through the hill at Bas Obispo has been cleared and a dam constructed across the Obispo River. The principal streams diverted by the Camacho Diversion were the Camacho, the San Juan, and the Mandinga.

The Chagres River has its sources in the mountains of Darien, and flows in a S.W. direction as far as Bas Obispo, where, describing a sharp turn, its waters proceed in a N.W. direction until they reach Limon Bay in the Caribbean Sea. The line of the Canal follows in a general direction, but not in its very tortuous channel, the direction of the Chagres, in that section which will eventually be the Gatun Lake, between Gatun and Bas Obispo. At Bas Obispo the hills of the Continental Divide are encountered, and advantage has been taken of the natural depression forming the Obispo River valley. The Obispo rises at Gold Hill, a high point on the Culebra elevation, and descends northwards into the Chagres river. The two rivers meet at Bas Obispo.

Where the Culebra Cut begins, exactly at Bas Obispo, low water in the River Chagres is about 43 feet above mean sea-level. At high water, or during floods, the level is 70 feet above mean sea-level.

The Canal bottom through the Cut is to be at an elevation of 40 feet above sea-level, or, in other words, 3 feet lower than the present level of the river at low water and some 30 feet lower than at high water. During construction a careful drainage of the Cut when the level of the bottom is brought below the lowest level of the river is therefore imperative.

The largest recorded flow of water in the rainy season was that of September 21, 1894, when, according to French records, the Upper Obispo discharged 36,995,000 cubic feet of water in 48 hours. The discharge at the point where the stream crosses the Culebra Cut was calculated at 6000 cubic feet per second.

The Obispo Diversion, which begins east of the Cut (near Gold Hill) and opposite Culebra settlement, runs almost parallel to and east of the Canal. At Upper Bas Obispo the channel of the stream crosses the Canal. The hills at that point are so near the Cut that the Diversion could not be constructed between them and the Canal, so that a passage had to be found in a depression further east behind these hills. In this Diversion, which is now almost completed, the waters collected in the channel on the east side of the Cut will flow into the Chagres River at a spot half a mile above Gamboa.

At the time of my visit steam shovels were at work in the final cut through a barrier separating the channel from a ravine on the opposite side, through which the stream will join the Chagres. I was told that considerable difficulty had been experienced in making this cut 250 feet wide and 97 feet deep, owing to the heavy grades encountered.

There were five steam shovels at work in the Pedro Miguel lock site. In April 1909 these five shovels took out close upon 100,000 cubic yards of rock and earth, loading rapidly on two tracks. In fact, the excavation of the lock site was so far advanced that it could be kept well ahead of the concrete-laying, which the Americans expected to begin in August (1909).

On the Pacific Slope, the Rio Grande (which was the only river crossing the line of the Canal between the summit of the Continental Divide and Pedro Miguel to the S.E. of the Culebra Cut) was entirely diverted by the French, and its waters were not allowed to flow through the bottom of the Culebra Cut. A dam had been constructed half a mile from the Cut, and the water driven into the Rio Grande Reservoir, from whence it was conveyed in pipes to the Settlements in the Canal Zone between Culebra and Ancon, as well as to the City of Panama. The reservoir was of sufficient size to hold all the water from this river in the dry season. During the rainy season the overflow found its way to the sea by the French diversion.

The excavation in the Culebra Cut was not deep enough for the water to flow by gravity into the old French diversion channel south of the Pedro Miguel lock site.

The railroad system on the Isthmus consisted of the old Panama Railroad and the railroad of the Isthmian Canal Commission. The proposed submersion, when the Gatun lake will be filled, of the present road-bed of the Panama Railroad made it imperative to relocate the line. The work of building the new railroad that would replace the old one was at the time of my visit more than half finished.

From the Atlantic terminal to Mindi, about five miles, and from Corozal to Panama and Balboa, the old line will be used; but between Mindi and Corozal the new railroad will be constructed to the East of its old location, and 10 feet above the normal surface of the artificial lake. That is to say, at 95 feet above sea level.

The relocation involved heavy embankments north of the Chagres

River, for which plenty of material was at hand from the Culebra excavations. A bridge 1320 feet long had been erected near Gamboa. A number of connecting tracks with the old line greatly facilitated the work.

The railway was well-equipped. New heavy rails had been relaid on the old line, which ran to perfection, so much so that on the main track 574 trains were operated daily, this including, of course, the heavy traffic of dirt trains as well as the passenger and freight trains. Great credit should be given to Mr. Slifer, the general manager of the railroad, and to his two engineers, Mr. Budd and Lieut. Meers, for the excellent work they have accomplished.

In due justice to the French, it must be said that they indeed worked more on the Panama Canal than the people gave them credit for. What work they did, considering the inferior machinery they used as compared with that which can be procured to-day, was certainly done well. The machinery, mind you, was inferior in design and size, but not in quality; for, indeed, the machinery imported on the Isthmus by the French was excellent of its kind, and perhaps the best that could be obtained in those days. In the Zone one hears only words of unstinted praise from Americans of the work done by the French. Many of the Belgian engines used by the French are still being used by the Americans. So are some of the dredges. Most of the French material has been discarded, because it was out of date and insufficient—not because it was bad.

French buildings, too, on the Zone are quite good. Many are occupied by Americans.

The Americans follow a great deal what the French have done on the Zone. American engineers recognise that the French surveys and maps were excellent, and with slight variations and enlargements they are to-day being used in the construction of the canal. Even the lock canal idea with a dam at Gatun is not an American one, but an old French idea, I believe, first advanced and then discarded by Godin de Lepinay in 1879.

The proverbial deadliness of the Panama Isthmus was the chief enemy of the first workers on the Canal. Until the Canal Zone could be freed from yellow fever, from bubonic plague, from cholera and small-pox; until the disastrous effects of malarial fever could be checked; until the Zone could be rendered sufficiently healthy so that the imported employees—whether American, of Latin races, or negroes—could live there in reasonable safety, and, above all, in a peaceful and restful state of mind and happiness; until the normal physical and moral strength of each unit of the labour force could be kept up without effort; until the terror that sudden death from fever could, in the ignorant workmen, be absolutely eliminated—the progress of the work could never be satisfactory. So, rightly, the Americans, upon acquiring the Canal rights, first spent two and a half years in rendering the Isthmus of Panama absolutely healthy by methodical

sanitation, such as we have few examples in colonies belonging to European powers.

They collected and organised a physically well-selected working force, and housed it in sensibly-built and kept, well-aired, well-lighted buildings. They provided all these men with first-class food of a nourishing and wholesome character ; and last, but not least, they enforced stringent laws practically suppressing intoxication. The spirits of the people on the Zone are kept up, not with whisky and absinthe, but by imported theatrical companies, by bands, lectures, dances, clubs, tournaments, etc. The result of this is that to-day the Americans have there a force of nearly 44,000 men—all in a remarkably sound physical condition.

There is a remarkable man in the Zone in Colonel Gorgas, the head of the Department of Sanitation. This department is now separated from the Government of the Canal Zone, and has been made an independent department.

The work of Colonel Gorgas can be summed up in a few words. Panama was one of the deadliest spots on earth. Colonel Gorgas has rendered it one of the healthiest. Under him are a number of practical and hard-working medical men, whose enthusiasm and devotion for their work are quite exceptional in these matter-of-fact days.

It is the general belief that yellow fever and malarial fever have been stamped out of the Isthmus by killing off all the mosquitoes. It is not by killing mosquitoes, whether they be "*stegomyia*, *anopheles*, or the humble *culex*," that yellow fever and malaria have been stamped out, but by something much greater, I think, which the public does not seem to realise. Let me explain in a few words.

Mosquitoes live in happiness, as you know, under conditions which are deadly to human beings, such as intense heat, darkness, stifling still air saturated with dampness, and within the radius of fetid emanations of rotten vegetation, putrid water, or decayed matter of any kind. Three things are deadly to mosquitoes: the powerful sunlight, pure water, and clean fresh air. It is just the other way round with human beings. Sunlight, fresh air, and pure water give life, strength and happiness. What happens, then? Change in any locality the conditions which make life impossible for mosquitoes, and you will make the inhabitants healthy—at least, as far as malaria is concerned.

My contention is—and I have had many opportunities of verifying my statement—that local conditions, and not mosquitoes, are primarily responsible for malarial attacks. I have seen cases of yellow fever and malaria where *no* *stegomyia* or mosquitoes of any kind were to be found. Mind you, this does not mean that mosquitoes cannot be infected when sucking the blood of an infected person, or even in a more direct way in the region they inhabit. There is no reason why there should not be feverish mosquitoes just as there are feverish people. When you dissect an infected *anopheles* you certainly find

malarial germs in the examination; but so you do in any infected mammal, reptile, bird or insect, to be found in that locality, and you might as well accuse them all in turn of transmitting fever to you, when you and they really got it from another and more fearsome source.

Here is what Colonel Gorgas and the Americans have done in the Canal Zone, and which is to me an infinitely greater achievement than the killing of all the mosquitoes. They have cleansed the air of all miasma, and they have purified the water in a most effective manner. First of all, they have cleared the Zone of the semi-tropical growth with its entangled vines and creepers, its putrid, stifled undergrowth and decomposing vegetable matter. They have either oiled, thus preventing exhalations, or drained or covered all stagnant waters; they have destroyed all decayed matter and allowed of no decomposing matter of any kind to contaminate the water or air of the Zone. Now that the Zone across the Isthmus is cleared of tall vegetation, the prevalent winds from Ocean to Ocean, helped by the friendly rays of a powerful sun, do the rest in the way of disinfection.

Furthermore, the Americans have hit on a practical type of buildings for the homes of their people. They have not made the common mistake of tropical architects of shutting off the light and air from the buildings. No: there is plenty of light streaming into all the rooms, plenty of air circulating everywhere. In fact, except that the people have a roof overhead, they are practically living out in the open while in their homes. The copper-wire mesh which screens the ample verandahs of every house is certainly a useful addition, because many, indeed, are the troublesome insects in tropical countries besides the annoying flies and mosquitoes. The pleasanter one can make life the better.

In several conversations with Colonel Gorgas, and by perusing statistics in reports to his Government, one learns interesting facts. For instance, the sick and death rates among the employees in the Canal Zone during the year (1908) compare favourably with those of the healthiest parts of the United States. For a proverbially deadly country this is certainly a triumph.

When the Americans first took over the Canal in 1904 we find that with a force of 6,213 employees the death-rate per thousand was 13·26. In 1906, with a force of 26,705 the death-rate per thousand reached as much as 41·37. In the year, 1908, with a working force of 43,890, the mortality per thousand was only 13·01; or, in other words, less than it was in 1904 with a force five times smaller.

Particularly interesting is to notice how the death-rate among the black labourers has fallen.

In 1905, with a force of 13,482, the death-rate per thousand was 26·25.

In 1906, with a force of 21,441, the death-rate per thousand was 47·24.

In 1908, with a force of 31,507, the death-rate per thousand was 12·76.

Another curious fact is that the black yearly death-rate has for the first time since American occupation been smaller than the white death-rate. In 1906, for instance, the death-rate among blacks was nearly three times higher than the death-rate among the whites. This satisfactory result was entirely due to the improved methods of sanitation, which have nearly reached a perfect stage, and, above all, to the better food which the blacks can obtain at low prices in the Commission Messes, instead of procuring food of inferior quality for themselves.

Taking into consideration the collective total population of the Panama Republic and the Canal Zone, the Americans have again highly satisfactory figures to show.

In 1904, with a total population of 35,000, the death-rate per thousand was 52·45.

In 1908, with a total population of 120,097, the death-rate per thousand was 24·83.

The attention given by local doctors to the diseases typical of the country has certainly given good results. Take special complaints more general on the Isthmus, such as dysentery and malaria :—

In 1906, with 26,705 employees, 69 died of dysentery and 233 from malaria.

In 1908, with 43,890 employees, there were 16 deaths from dysentery and 73 from malaria.

A similar improvement is noticeable in the percentage of typhoid fever, pneumonia, beri-beri, and other complaints common in the tropics.

No smallpox occurred during the year 1908, no cases of plague, and no cases of yellow fever. The latter seems entirely stamped out. Indeed, since December 1905, I believe not a single case of yellow fever has developed on the Isthmus. During that epidemic (1905) there were 246 cases, with 84 deaths.

Before the American occupation both Colon and Panama cities were universally renowned for the filthy condition of their streets and of the poorer homes in the old Columbian days. We will not refer to the primitive arrangements in the way of sanitation. Curiously enough, the natives themselves seemed to suffer but little from this state of affairs. The population of Panama was immune to yellow fever, and even the employees of the French Company appear to have got acclimatised to a certain extent. No death from yellow fever seems to have occurred among them since the year 1897. It was principally among the new arrivals that the disease showed itself and proved fatal.

Americans have done wonders in the way of reducing Panamanian towns from a state of inconceivable dirt and confusion to a model condition of cleanliness and order.

The sanitary work in the Canal Zone is remarkably carried out in every detail. Take, for instance, the drainage. Open ditches are used in order to concentrate surface waters where they can be kept under control. These ditches are kept oiled by means of drip vessels, and are kept free of vegetation and other obstructions that are favourable to the propagation of mosquito life. Near the permanent settlements many of the ditches have been lined and paved, and the stone surfaced with cement mortar. Seepage waters sifting through on hillsides and near the base of hills are carried off by means of tile drains. The banks of streams where anopheles are to be found are kept clear of vegetation. Oil is kept dripping into these streams so that it will collect in shallow places and prevent anopheles occurring there. During the dry season the rivers and larger streams become almost stagnant. They are then prolific sources of anopheles-breeding. Vegetable matter collecting in such places has to be constantly removed, and the stagnant waters are kept covered with oil. Narrow channels within the bed of the stream are dug in order to keep the water in motion. Where anopheles larvæ occur in moving water, they are destroyed with poisonous substances such as phinotas oil or carbolic acid. The reservoirs are kept stocked with fish. The grass around the edges of the water surfaces is cut down. Sulphate of copper and constant sweeping keep the streams and ditches clear of algæ. For larvacides, crude oil, carbolic acid, and phinotas oil are used. Fish are useful as destroyers of larvæ, providing there is absence of vegetable scum and vegetable growth.

Anopheles hide in the grass when there is a high wind. When the jungle is removed where anopheles breed they look for the nearest shelter. This accounts for the increase of mosquitoes in houses immediately after the clearing is made. Along the edge of the clearing mosquitoes are generally present, when they are not noticed in the houses near the clearing. The sharpest watch is kept in locating breeding places. The smallest pool of stagnant water is not allowed to remain. So much so that the laws of the Zone prohibit cattle and other animals being loose near settlements. They are allowed to pasture only on grounds specified as safe by the Sanitary Department, because they leave depressions with their hoofs in the soft ground, and these depressions are apt to collect stagnant water.

When labourers' cars are moved from infected districts to camps, they are fumigated before shipment in order to destroy all infected anopheles. When anopheles become numerous near or in barrack buildings fumigation is applied. Anopheles are more apt to be found in barrack buildings than in American houses, as ignorant labourers care little about mosquitoes and leave the doors wide open, where it is possible to leave them open, as the Americans have everywhere applied self-closing devices on nearly each door. Sulphur is found to be the most efficient substance used for fumigation. Where

there is danger of harming fabrics and metals, such as in the workshops, pyrethrum powder is used. Camphor phenique gives good results, because its fumes produce no appreciable stain. It leaves a faint odour of camphor, which prevents vermin from destroying fumigated fabrics.

When, after all this, surviving mosquitoes are detected anywhere, they are killed by the most delightful of all, the squashing system. A small square of metallic screening attached to a stick supplies an effective instrument of destruction with the slapping process. Where tents are used in temporary camps, a labourer is employed to go through each tent several times a day, but especially in the morning, to kill off all gorged mosquitoes.

The method of preventing increase of malaria in Zone camps is highly interesting. Each week the district physician makes a report of the number of cases of malaria occurring in his district. The report shows each section of his district and each group of houses where the cases occurred. These reports are sent direct to the Chief Sanitary Inspector, who notifies the District Inspector as to which section of his district the malarial cases are coming from. A chart is kept in each district by the District Inspector, which shows the increase and decrease of malaria cases in his district each week. The figures on this chart indicate the percentage of population that are sent to the hospital with malaria. At any section of a district where malaria increases, and where the rate remains abnormally high, the Division Inspector investigates conditions and tries to determine the cause or source of the increase of malaria. The cause of the increase of malaria may be due to labourers coming from other infected areas, exposure and other changes of surface topography which are necessary in the course of the progress of the excavation work. Areas which have been kept for eight or nine months with a relatively low malarial rate have suddenly dropped to the bottom of the list, when it has been necessary, for canal construction purposes, to block up and change the natural drainage conditions that existed previously. This makes the anti-malarial work in the Canal Zone much more difficult than it would be elsewhere. The nature of the anti-malarial work changes, and new breeding areas are continually brought into existence as the work progresses.

The last case of yellow fever occurred in the Canal Zone during December 1905. There is a larger non-immune population in the Canal Zone at the present time than there has ever been. In other words, if a case of yellow fever occurred on the Isthmus to-day, the epidemic that would follow would be exceedingly serious. In other tropical localities subject to yellow fever, the non-immune population increases and decreases from time to time. Yellow fever follows this increase or decrease. On the Isthmus, the Department of Quarantine keeps in touch with all yellow fever ports, and is constantly on the look-out for the introduction of cases of yellow fever. When such cases occur

on board ship they are transported to a screened ward where stegomyia cannot get at them. At all the hotels the law requires arriving guests to register; also the law requires a compulsory report of suspected yellow fever cases by physicians. Any physician or other person reporting a suspected case of yellow fever, that turns out to be a case of yellow fever, receives a reward of fifty dollars. The non-immune population in the Canal Zone all live in screened quarters, and special care is taken in districts where non-immunes live to get rid of stegomyia. In fact, the stegomyia destruction work is so thoroughly carried out that they are almost extinct in the Canal Zone at the present time. The greatest danger, according to local doctors, lies where light or unrecognised cases of yellow fever occur, so that the patient is not confined to his bed, but travels about and infects stegomyia in various localities. These, in turn, are supposed to infect other persons at a later period. There is an ordinance enforced in the Zone which authorises the Chief Sanitary Officer or his representative to fine persons who allow stegomyia to breed in containers on their premises. Where rainwater has to be used, or where water is to be stored, the house owners are required to keep the top of the barrel screened so that mosquitoes cannot find access, and the water must only be drawn from the barrel by means of a tap. Eave troughs are not allowed, nor is trash allowed to collect near or about houses. Vegetation near houses, that might hide water containers, is removed, and the interiors of houses are constantly inspected.

Great attention has been paid by the Commission to obtaining an excellent water supply. This, I think, more than the death of all mosquitoes, is responsible for the present healthy condition of the population. The settlements on the Canal Zone obtain their water supply from the reservoirs at Rio Grande, Camacho and Gorgona, and also from the Tabernilla and Gatuncillo rivers. The terminal cities of Panama and Colon obtain their water supply from the Rio Grande and the Mount Hope reservoirs respectively. Distribution systems from these reservoirs are so arranged as to reach all camps, and most of the native settlements. The watersheds are free from habitations, and are continually patrolled and inspected. Water obtained from the source of supply, and also from the taps and drinking-water vessels, is analysed at regular intervals. When the bacterial contents is high an immediate investigation of the cause is made. Where polluted streams occur near native settlements, proper notices are posted in conspicuous places warning people not to use such water for drinking purposes. The police authorities, as well as the Sanitary Inspectors, have authority to arrest persons polluting or trespassing on the water-shed. In case the water supply is contaminated, or even suspected to be contaminated, notices to that effect are immediately posted, and house-owners are directed to boil all water used for drinking purposes. The Commission delivers distilled water to Commission houses and to all Mess Halls. Most perfectly thought

out sanitary arrangements are to be noticed everywhere in the Zone providing for the disposal of sewage, and in order to prevent contamination of the drinking water. Distillation plants are in operation in most of the settlements.

Everything is provided in the Zone for the labourers' comfort. At the labourers' camps substantial sheds are erected with concrete floors and iron wash-tubs, supplied with running water, in order that the people may wash their clothes at times convenient to them. Bathing is encouraged, and does much towards keeping the population healthy. The labourers' camps are supplied with houses containing a series of shower baths. These baths are available for use at all times. At camps where married men's quarters are to be found, arrangements are made so that the families of the labourers may use these shower bath-houses at definite periods, while the men are away at work.

Many of the old French barrack buildings have been repaired and remodelled, and are being used in addition to the new barrack buildings constructed by the Commission. All the old and new buildings have been supplied with balconies which are screened with fine copper-wire mesh to keep out mosquitoes. The doors open outwards, and are supplied with springs closing them automatically. The space directly under the roof on all four sides of the buildings is left open and screened, for purposes of ventilation. In the old French buildings no attempt was made to ventilate the buildings except by means of windows and doors. In the American construction there is a large ventilator on the roof as well as at the sides of the buildings under the roof. This is a good thing, for good ventilation and light are the greatest enemies of disease.

The metallic screening of the labourers' barracks and the whites' quarters is inspected at regular intervals by the Sanitary Department, and necessary repairs are made.

It was found that beds and cots were unsatisfactory and collected vermin; therefore what are known as "Standee Bunks" are now used. These consist of a framework made of 2-inch galvanised iron pipe, on which canvas is stretched. These bunks are so arranged that the canvas attached to the frames is removable from the framework that supports it, and is detached at regular intervals to be dipped into vats of boiling water. The canvas is then thoroughly cleaned and put out in the sun to dry. The iron framework that remains in the buildings is previously gone over with an alcohol lamp so as to destroy by fire any vermin remaining. The barrack buildings are scrubbed out daily, and are kept neat and clean. Each afternoon the barrack buildings, labourers' kitchens, mess halls, closets, bath-houses, wash-houses, etc., are inspected.

A great institution in the Zone, and which does much towards keeping the members of the labourers' force in their normal strength, are the Mess Halls. The labourers and other employees may eat at the Commission Mess Halls or private messes, or provide their own

food if they wish. There are separate messes for the gold employees, coloured employees, and European labourers. Formerly the coloured labourers did their own cooking, but it was found that they were not eating sufficient nor properly cooked food, so the Commission now prepares food for them. The coloured labourers receive 10 cents United States currency per hour, and pay 30 cents for three meals each day. The European labourers receive 20 cents per hour, and pay 40 cents for the three meals received each day. The skilled labourers get a higher rate of pay.

Employees may obtain from the Commissary Department up to 40 per cent. of the amount they have earned. This money is deducted from their pay at the end of the month; also, meal tickets given to them are deducted from the monthly salary due to them.

At each of the camps there is a dispensary and a "sick camp." At the larger settlements there is also a hospital. These are under the supervision of the District Physicians. At Ancon and Colon are the main hospitals. On Taboga Island, 9 miles out in the Pacific, is a sanatorium for the convalescent patients. No charge is made to employees for treatment at the dispensary or the hospitals, and families of employees are treated at a nominal rate.

Hospital cars make a round trip daily on the railway, taking patients to Ancon hospital on the morning trip, and to Colon hospital on the afternoon trip. Up-to-date ambulances await the trains at the station, so that no delay occurs in conveying the patients to the hospitals. At the "sick camps" the labourers are either discharged or sent to the terminal hospitals. Both gold employees (United States citizens chiefly), and white labourers (mostly Spaniards and Italians), if unfit for work when leaving the terminal hospitals, are sent to the Taboga sanatorium. Gold employees who become ill while on duty are allowed up to thirty days' sick leave with pay during the year. In case of emergency or accident a special train is sent for the patient or patients direct to Ancon or Colon hospitals, when the patient can stand being transported.

Americans not only take the greatest care of the living, but the dead are looked after in a manner undreamed of by Europeans. All deaths occurring in the Zone are reported to the Chief Sanitary Officer. The cemeteries in the Zone are cared for, and burials are made by the Sanitary Department. A complete record of all cases of death is correctly kept. In case of the death of an American employee the body is embalmed, sealed in a metallic casket, and shipped to his relatives in the United States free of cost.

GENERAL MONTHLY MEETING,

Monday, July 5, 1909.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S.,
President, in the Chair.

Humfrey Henry Edmunds, Esq.
Colonel Sir Charles Euan-Smith, K.C.B. C.S.I. D.C.L.
The Duchess of Northumberland,
Lady Victoria A. Percy,
Alfred Rowe, Esq.

were elected Members of the Royal Institution.

The Managers reported, That they had received the sum of
£2436 18s. 10*d.*, the legacy of the late Dr. George Gore, F.R.S.

The PRESENTS received since the last Meeting were laid on the
table, and the thanks of the Members returned for the same, viz. :—

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- United States Department of Agriculture*—Experiment Station Record, Vol. XX. Nos. 7-9. 8vo. 1909.
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GENERAL MONTHLY MEETING,

Monday, November 1, 1909.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S.,
President, in the Chair.

James Douglas, Esq., LL.D.

was elected a Member of the Royal Institution.

The Honorary Secretary announced the decease of Professor Simon Newcomb on July 11, 1909, and the following Resolution of Condolence, passed by the Managers at their Meeting held this day, was read and unanimously adopted :—

Resolved, That the Managers of the Royal Institution desire to record at this, their first Meeting subsequent to his death, their sense of the loss sustained by the Institution, and by Science, in the decease of their Honorary Member, Professor Simon Newcomb, Ph.D. D.C.L. LL.D. Sc.D. Hon.F.R.S. Membre de l'Institut, Commander of the Legion of Honour, Prussian Order pour le Mérite, one of the most celebrated Astronomers of our time, Author of numerous books on Astronomy and Economics, for many years Professor of Mathematics to the Naval Observatory at Washington, and whose scientific activity has comprised Researches on the Motion of the Moon, the Stars and their Satellites, Problems of Gravitation, and other Astronomical Investigations. On the occasion of the commemoration of the Faraday Centenary, Professor Newcomb was elected an Honorary Member of the Royal Institution.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with the family in their bereavement.

The Special Thanks of the Members were returned to Sir William Farrer for his Donation of £50 to the Fund for the Promotion of Experimental Research at Low Temperatures.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

- The Secretary of State for India*—Geological Survey Records, Vol. XXXVII. Parts 3-4; Vol. XXXVIII. Part 1. 8vo. 1909.
- Kodaikanal Observatory Bulletin, Nos. 17-18. 4to. 1909.
- Linguistic Survey of India, Vol. III. Part 1. 4to. 1909.
- Agricultural Journal of India, Vol. IV. Part 3. 8vo. 1909.
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- Geography and Geology of the Himalaya Mountains. By S. G. Burrard and H. A. Hayden. Part IV. 4to. 1908.
- Aberdeen University (the Registrar)*—Roll of Graduates, 1860-1900. By Col. W. Johnston. 4to. 1906.
- Accademia dei Lincei, Reale, Roma*—Rendiconto, 1909, Vol. II. 4to.
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- Classe di Scienze Morali, Serie Quinta, Fasc. 10-12. 8vo. 1908.
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- American Geographical Society*—Bulletin, Vol. XLI. Nos. 7-9. 8vo. 1909.
- American Philosophical Society*—Proceedings, Vol. XLVIII. Nos. 191-192. 8vo. 1909.
- American Society of Biological Chemistry*—Proceedings, Vol. I. No. 4. 8vo. 1909.
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- Boston Public Library*—Bulletin, Vol. II. Nos. 2-3. 8vo. 1909.
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- Cambridge Philosophical Society*—Transactions, Vol. XXI. Nos. 8, 11. 4to. 1909.
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- Concrete for July-Nov. 1909. 8vo.
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- Western Society of Engineers—Journal, Vol. XIV. Nos. 3-4. 8vo. 1909.
- Wireless Institute, New York—Proceedings, Vol. I. No. 3. 8vo. 1909.
- Yorkshire Philosophical Society—Annual Report for 1908. 8vo. 1909.
- Zoological Society of London—Proceedings, 1909, Parts II.-III. 8vo. 1909.
- Transactions, Vol. XIX. Part 1. 4to. 1909.
- Zurich Naturforschender Gesellschaft—Vierteljahrsschrift, 1909, Heft I.-II. 8vo. 1909.

GENERAL MONTHLY MEETING,

Monday, December 6, 1909.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and
Vice-President, in the Chair.

James Henly Batty, Esq.
Thomas Gibson Bowles, Esq.
Cecil Broadbent, Esq., F.R.Met.Soc.
Captain Frank Stanley Rose,
Harold R. D. Spitta, Esq., M.D. M.R.C.S.
John B. Rankin Swan, Esq.
John Sigismund Wilson, Esq.

were elected Members of the Royal Institution.

Geheimer Regierungsrath Professor Dr. Otto N. Witt, Ph.D.
F.C.S., President of the Chemical Society of Berlin,
Professor George E. Hale, LL.D. Sc.D. Hon. F.R.S., Director
of the Mount Wilson Solar Observatory of the Carnegie
Institution of Washington,

were elected Honorary Members of the Royal Institution.

The following Lecture Arrangements were announced :—

WILLIAM DUDELL, Esq., F.R.S. M.I.E.E. *M.R.I.* Six Lectures (adapted to a Juvenile Auditory) on MODERN ELECTRICITY. On Dec. 28 (*Tuesday*), Dec. 30, 1909; Jan. 1, 4, 6, 8, 1910.

PROFESSOR W. A. HERDMAN, D.Sc. F.R.S. Three Lectures on THE CULTIVATION OF THE SEA. On *Tuesdays*, Jan. 18, 25, Feb. 1.

PROFESSOR FREDERICK W. MOTT, M.D. F.R.S. F.R.C.P., Fullerian Professor of Physiology *R.I.* Six Lectures on THE EMOTIONS AND THEIR EXPRESSION. On *Tuesdays*, Feb. 8, 15, 22, March 1, 8, 15.

THE REV. C. H. W. JOHNS, M.A. Two Lectures on ASSYRIOLOGY. On *Thursdays*, Jan. 20, 27.

MAJOR MARTIN HUME. Two Lectures on EUROPE'S DEBT TO MEDIEVAL SPAIN. On *Thursdays*, Feb. 3, 10.

PROFESSOR SILVANUS P. THOMPSON, B.A. D.Sc. F.R.S. *M.R.I.* Three Lectures on ILLUMINATION, NATURAL AND ARTIFICIAL. On *Thursdays*, Feb. 17, 24, March 3.

A. J. FINBERG, Esq. Two Lectures on TURNER. On *Thursdays*, March 10, 17.

HENRY WALFORD DAVIES, Esq., Mus.Doc. LL.D. Three Lectures on MUSIC IN RELATION TO OTHER ARTS. (*With Musical Illustrations.*) On *Saturdays*, Jan. 22, 29, Feb. 5.

PROFESSOR SIR J. J. THOMSON, M.A. LL.D. D.Sc. F.R.S. M.R.I., Professor of Natural Philosophy, R.I. Six Lectures on ELECTRIC WAVES AND THE ELECTROMAGNETIC THEORY OF LIGHT. On *Saturdays*, Feb. 12, 19, 26, March 5, 12, 19.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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American Geographical Society—Bulletin, Vol. XLI. Nos. 10–11. 8vo. 1909.

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Athenæum for Nov. 1909. 4to.

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Chemist and Druggist for Nov. 1909. 8vo.

Concrete for Dec. 1909. 8vo.

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Electrical Engineer for Nov. 1909. 4to.

Electrical Engineering for Nov. 1909. 4to.

Electrical Review for Nov. 1909. 4to.

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- Electrical Times for Nov. 1909. 4to.
 Electricity for Nov. 1909. 8vo.
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 Engineer-in-Charge for Nov. 1909. 8vo.
 Engineering for Nov. 1909. fol.
 Horological Journal for Oct.-Nov. 1909. 8vo.
 Illuminating Engineer for Dec. 1909. 8vo.
 Journal of the British Dental Association for Nov. 1909. 8vo.
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 Law Journal for Nov., 1909. 4to.
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 Mois Scientifique for Oct. 1909. 8vo.
 Motor Car Journal for Nov. 1909. 4to.
 Musical Times for Nov. 1909. 8vo.
 Nature for Nov. 1909. 4to.
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 Page's Weekly for Nov. 1909. 8vo.
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Iron and Steel Institute—Carnegie Scholarship Memoirs, Vol. I. 8vo. 1909.
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Vienna Imperial Geological Institute—Jahrbuch, 1909, Band LIX. Heft 2. 8vo.

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Wilde, H., Esq., D.Sc. D.C.L. F.R.S. M.R.I. (the Author)—A New Binary Progression of the Planetary Distances. 8vo. 1909.

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WEEKLY EVENING MEETING,

Friday, June 11, 1909.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L.
LL.D. F.R.S., President, in the Chair.

PROFESSOR SIR JAMES DEWAR, M.A. LL.D. D.Sc. F.R.S. *M.R.I.*,
Fullerian Professor of Chemistry, Royal Institution.

Problems of Helium and Radium.

[ABSTRACT.]

METALLIC VACUUM FLASKS AND SYPHONS.

PROGRESS in low temperature investigation is greatly aided by careful attention to questions of method. This observation is specially applicable to vacuum-jacketed arrangements for heat isolation. Silvered glass vessels are most useful, and give high isolation, but are subject to deterioration and collapse, all the more when their form is complicated. Thus the question of the production of good metallic vacuum-jacketed apparatus becomes highly important. If influx of heat by radiation and gas convection is diminished as much as possible, metallic vacuum-jacketed vessels give no outward sign of the low temperature of their contents. I have here a lead pipe nearly 100 ft. long (Fig. 1). Liquid air is flowing from the glass vessel A to the lower one B, which is connected to a suction pump at C. The pipe shows no sign of frosting or condensation of moisture, except at the two ends which dip into the liquid air. The outside tube is the jacket to a smaller inner tube (which is externally covered with a layer of flannel) in which the liquid air is flowing, the annular space between the two tubes being well exhausted. The vacuum is maintained high by some charcoal placed in a small enlargement D at the end of the tube dipping into the liquid air reservoir A.

I have here a double jacketed vacuum vessel of 3 litres capacity, made of nickel, full of liquid air. A little charcoal placed—as described in my Friday Evening Address of 1906—in the lower part of the flask, where it is cooled to the temperature of the liquid air, keeps the vacuum between the walls up to the necessary perfection by absorbing adventitious gases. The neck of the flask is made of thin German silver, a badly conducting alloy, and is covered with a

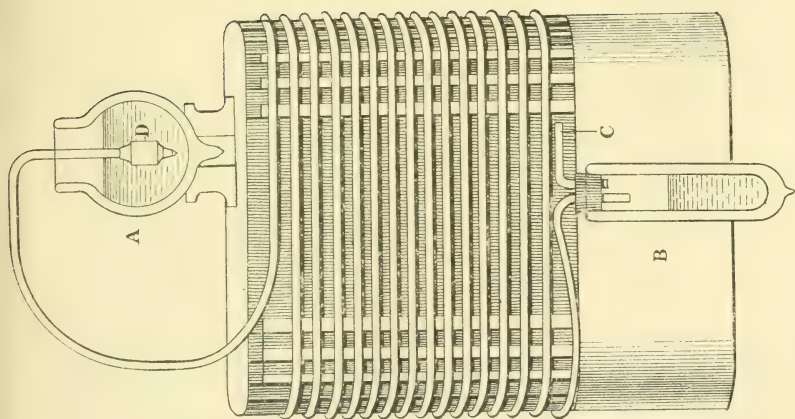


FIG. 1.

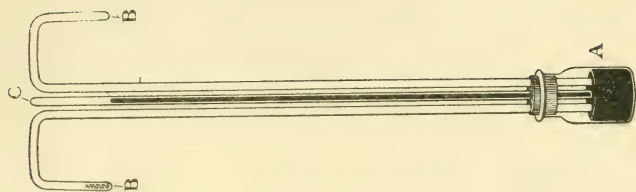


FIG. 2.

spiral of worsted cord. This enables a silvered glass vacuum vessel to fit tightly over it and form a kind of open regenerator covering, through which the cold air coming from the evaporating liquid is directed spirally round the neck of the flask, thereby greatly retarding the heat conduction into the inner vessel, which chiefly arises from the outer metallic tube.

COMPARATIVE CONDENSABILITY OF GASES BY CHARCOAL AT LIQUID AIR AND LIQUID HYDROGEN TEMPERATURES.

The relative condensability by charcoal of the gases, air, hydrogen, and helium, can be shown by filling three similar sets of double tubes, shown in Fig. 2, respectively with samples of these gases. The two tubes of each set are filled with the particular gas at atmospheric pressure and the ordinary temperature, and dip into a little bottle of mercury, A. The tubes are bent twice at right angles and sealed up, as shown in the figure. Thus the closed end of either tube of each set can be cooled by immersion in a vacuum vessel containing liquid air or hydrogen. A gramme of charcoal is placed in the sealed end of one tube in each pair. For convenience of pressure observations, an ordinary barometric tube C accompanies each pair of tubes. When the closed end of the air tube is cooled in liquid air, only a small contraction is observed, as shown by a slight rise of the mercury in the tube. If, however, the air tube which contains the charcoal is cooled all the air is thereby condensed, and the mercury quickly rises to the barometer height in the tube. Now take the pair of hydrogen tubes. As before, on cooling the tube without charcoal, only a slight contraction is shown, due to the cooling, but when the charcoal tube is immersed in the liquid air, a quantity of the hydrogen is condensed in the charcoal, and the mercury rises in the tube. The height it attains, however, is noticeably less than in the air-charcoal tube, showing the smaller condensability of hydrogen in the charcoal at liquid air temperature. On cooling either of the helium tubes with liquid air, practically no condensation is shown. Now instead of liquid air, let us use liquid hydrogen, and cool first the air tube. Immediately the mercury rushes up to the barometric height, all the air being condensed into a solid of inappreciable tension of vapour, and the charcoal tube behaves in the same way. Now pass to the hydrogen tubes. When the plain gas tube is cooled, quite a noticeable contraction is visible, because the temperature of liquid hydrogen is so low compared to the hydrogen gas inside at the room temperature. On now cooling the charcoal-hydrogen tube, complete absorption is produced, and the mercury rises to the barometric height. The hydrogen, at the temperature of its own boiling point, is completely absorbed in the charcoal. Now compare this with the set containing helium. On cooling the helium tube it behaves similarly to the plain hydrogen tube, but on cooling the charcoal tube, quite a

large diminution of pressure is produced, showing that even helium is condensed to a considerable extent by charcoal at 20° absolute. As the charcoals warm up, the condensed gases are again expelled; the helium very rapidly, the hydrogen somewhat more slowly, and the air after some few minutes.

[Brief explanation of low temperature researches with helium, illustrated by slides of the Royal Institution and Onnes Helium Plants; Reference Diagrams.]

COMPOSITION OF BATH GAS.

					Hydrogen	Nitrogen	Inert Gas
Liquid Bath Gas (most volatile part)					15	30	55
					Helium		Neon
Inert Gas (Bath)	88	17
„ (Air)	16	84

VAPOUR PRESSURES OF LIQUID GASES.

					A	B
1. Oxygen	log. P	= $7.012 -$	$378.3/T$
2. Hydrogen	„	= $5.981 -$	$63.6/T$
3. Gas same volatility as 1 to 2	„	= $5.182 -$	$10/T$
4. Helium deduced from charcoal tensions	„	= $5.324 -$	$11/T$
5. Helium (Onnes)	„	= $6.496 -$	$16.3/T$

Critical constants and boiling points used in calculating 1, 2, 3, 5.

T is the Absolute Temperature, and P is Pressure in mm. of Mercury.

The B constant is proportional to the Molecular Latent Heat.

The boiling point of hydrogen is about 20° abs., its critical temperature 32° abs., and the critical pressure 13 atm. The corresponding figures for helium are approximately: boiling point $4\frac{1}{2}^{\circ}$, critical temperature $5\frac{1}{2}^{\circ}$, critical pressure about 3 atm. Onnes has, by evaporating liquid helium under reduced pressure, reached a temperature of 3° abs. With the aid of some hypothetical element related in volatility to helium, as helium is to hydrogen, we should be able to reach 1° or possibly even $\frac{1}{2}^{\circ}$ abs., but not the absolute zero. The experimental approach to the absolute zero has practically been made during the last thirty years. Far greater advances have been made during this period than in the previous 300 years, yet all the new knowledge acquired only shows the need for further research.

Phosphorescence of Gases.—Twenty years ago, I endeavoured in a Friday Evening Discourse to demonstrate the phosphorescence of ozone and oxygen compounds. The effect of the impurities in the air was not then fully recognized. Geissler was the first to discover that phosphorescence may be produced in vacuum tubes. Becquerel considered that oxygen was essential to the production of such phenomena. The new apparatus may be understood by referring to Fig. 3. On the brass cover, A, ground to fit air-tight to a glass

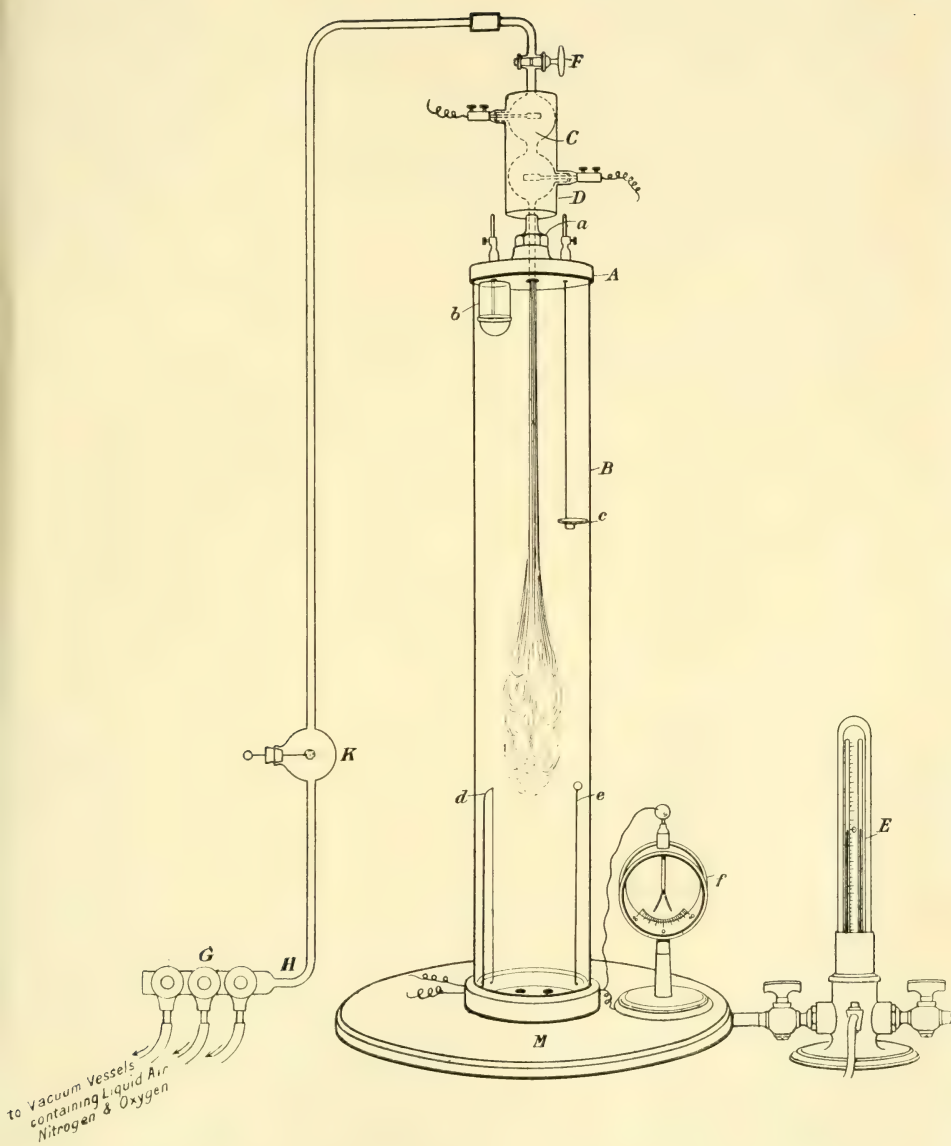


FIG. 3.

cylinder, B, more than 3 ft. in height and 8 in. diameter, is mounted an electric discharge-tube, C, stimulated from a transformer and provided with steel, platinum, aluminium, or other electrodes, about 1 cm. diameter and 4 cm. long, the whole discharge tube being surrounded by a water-jacket, D. An exhaust-pump, capable of dealing with a large leak of gas, while still maintaining a pressure of from 1 to 5 mm. of mercury, is connected to the metal base, M, on which the cylinder is mounted vacuum tight. This base is fitted with stop-cocks, and connected to a mercury manometer, E, for controlling the exhaust. The gases are admitted at the top, where they can be regulated by the cock, F.

For obtaining pure air, oxygen and nitrogen, these gases were vaporized directly as required from the respective liquefied gases through narrow lead pipes soldered to the top of brass tubes about 1 ft. long and 1 in. wide, covered over the ends with flannel to act as a filter when dipped into the respective liquids contained in vacuum vessels. The narrow lead pipes are connected to the inlet screw-down regulating valves, G, fixed to the table on which the apparatus is set up, the common outlet tube, H, being connected by another narrow lead pipe to the stop-cock F at the top of the glass cylinder, passing through a small tubulated spherical bulb, K, into which organic and other vapours are introduced by saturating a little cotton wool rolled on the end of a wire with the respective organic liquids. This combination affords a very effective and ready means of obtaining large currents of gas in a state of purity, necessary to ensure good phosphorescence in large scale experiments required for lecture demonstration.

The gas stream is thus seen to form a phosphorescent axial core. The discharge-tube is mounted on a ball and socket union, *a*, so that the gas stream can be deflected in the cylinder. A glass cup, *b*, mounted immediately under the top plate of the cylinder, can be turned under the orifice of the discharge-tube so that the entering gas stream impinges into it. The gas stream is thus broken up into a luminous cloud overflowing the cup and filling the upper part of the glass cylinder. A horizontal metal plate, *c*, can similarly be moved into the stream; this causes a general scattering of the beam. When the air current is started, it rushes down the tube at a velocity of about 1000 ft. a second, and a steady brilliant phosphorescent stream appears down the whole length of the cylinder. On increasing the amount of the entering air, the stream is shortened and assumes a brush-shaped formation. A thermo-junction, *d*, connected to a reflecting galvanometer, indicates change of temperature when the phosphorescent stream passes over it. Similarly the beam can be deflected on to a small insulated metal sphere, *e*, connected to a charged electrometer, *f*. When this is done, rapid discharge of the electrometer shows the ionization of the gas particle forming the phosphorescent stream; even should phosphorescence be

feeble or absent, as with some gases, the ionization can be demonstrated. A long thin glass tube sealed at the end can be inserted through a hole in the top plate of the cylinder and filled with liquid air when necessary. Nitrous acid and nitric peroxide are condensed upon it when placed in the phosphorescent stream. This is proved by the well-known Starch Iodide of Potassium and Griess Reactions. It is extremely difficult when using this large apparatus to prevent the formation of some nitrous acid even if liquid oxygen is evaporated; and thus the proof that the phosphorescence is really due to ozone alone and not to nitrous compounds is not perfectly conclusive. If a current of carbon dioxide is substituted for air, the phosphorescence is marked but much more feeble. Hydrogen gas alone gives no luminosity, and if a trace of hydrogen is added to the phosphorescent stream obtained by the evaporation of liquid oxygen or liquid air the phosphorescence at once disappears. Thus 5 per cent. of hydrogen by volume stops the glow in very pure oxygen, while 10 per cent. is required to arrest the light-stream in air. The rate at which the gas passes through the discharge-tube makes a difference in the intensity of the glow, but the presence of more or less moisture has little effect. A little ether vapour or benzol behaves like hydrogen, arresting all phosphorescence, and the luminosity does not reappear until all the vapour has been carried away by the continually renewed pure gas stream. All volatile organic bodies containing hydrogen stop or diminish the phosphorescence. On the other hand, volatile organic substances containing no hydrogen, such as carbon bisulphide, still give phosphorescence, but pure cyanogen or tetrachloride of carbon vapour give no luminosity. Pure carbon bisulphide and sulphur dioxide vapour alone give good phosphorescent streams at suitable tensions. The glow with very pure oxygen is short but distinctly more brilliant than that given by air. Nitrogen obtained from the liquefied gas gives only a feeble and very diffuse glow. This gas contains a few per cent. of dissolved oxygen, the presence of which causes the glow, as pure nitrogen gives no visible luminosity with this kind of electric discharge.

RADIUM AND ITS EMANATION.

What has all this to do with Radium? It is now known that bodies like ozone and nitric-oxide are produced from dissociated molecules at very high temperatures under considerable absorption of heat, and that such bodies are unstable. That may give us a clue to the formation of radio-active bodies. In my Friday Evening Address for the year 1888, I remarked: "Ozone is formed by the action of a high temperature owing to the dissociation of the oxygen molecules and their partial recombination into the more complex molecules of ozone. We may conceive it not improbable that some of the elementary bodies might be formed somewhat like the ozone,

but at very high temperatures, by the collocation of certain dissociated constituents and with the simultaneous absorption of heat." This suggestion of endothermic elements was made fifteen years before the isolation of radium, and the proof of its continual thermal emission by the Curies. Rutherford has shown that the de-electrified particle of the α -rays of radium turns into ordinary helium, and radium itself has been traced to uranium as its parent. As far as our knowledge of the emanations of radio-active bodies goes, it would seem that they are substances a little more volatile than carbon dioxide. The annexed tables show the relative atomic weights and volatilities of the Series of Rare gases.

NEW CONSTITUENTS OF THE ATMOSPHERE.

				Atomic Weight		Boiling Point abs.
Helium	4	..	4°
Neon (new)	20	..	32
Argon (inactive)	40	..	87
Crypton (hidden)	82	..	121
Xenon (stranger)	128	..	164
x_1	172	..	
x_2 Radium emanation	222	..	211

RADIUM EMANATION.

Vapour Pressure.

	A.		B.	
Log P.	= 7.626	—	1020/T	$\left(\begin{smallmatrix} -78^\circ \\ -101^\circ \end{smallmatrix} \right)$ Rutherford
„	= 7.332	—	941/T	$\left(\begin{smallmatrix} -55.8^\circ \\ -60.6^\circ \end{smallmatrix} \right)$ Ramsay
„	= 6.950	—	859/T	$\left(\begin{smallmatrix} \text{critical} \\ \text{boiling point} \end{smallmatrix} \right)$ Ramsay
$\left(\frac{\text{Log P.}}{\text{Xenon}} \right)$	= 6.963	—	669/T	„ „

The volatilities of the rare gases and that of the emanation of radium are here expressed by the well-known Rankin equation—

$$\text{Log } P = A - \frac{B}{T}$$

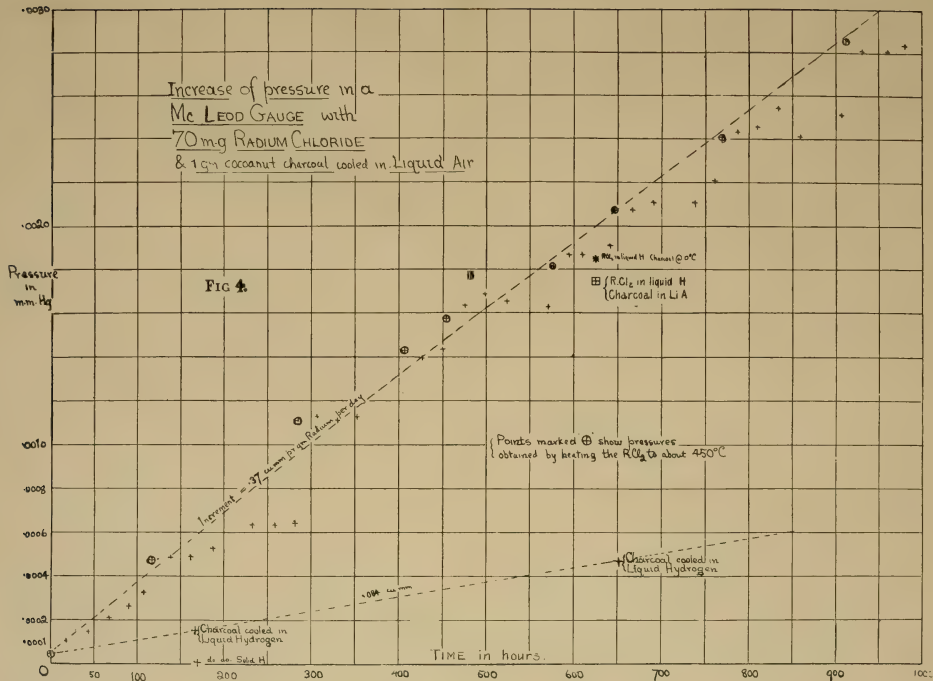
where P and T are the pressure in mm. of Hg and the absolute temperature respectively, and A and B are constants; B is here proportional to the molecular latent heat. The tables thus show that not only does the emanation fit into a series with the rare gases when classified chemically, but that also in its chief physical properties it allows of such a grouping. It seems probable, therefore, that x_1 and x_2 , of which the latter is the radium emanation, together with that from thorium and actinium, would suitably find a place in this series of gases. It may be added that the B constant for liquid carbonic acid is 869; that for sulphuretted hydrogen is a little lower. The radium emanation, therefore, is of the same order of volatility as these substances.

RATE OF FORMATION OF HELIUM FROM RADIUM.

I determined the rate of production of helium from radium in an apparatus consisting of a McLeod gauge, in the construction of which no indiarubber joints were used, the mercury reservoir being connected to an exhaust pump, while the elevation and lowering of the mercury was carried out by admitting and exhausting air in the reservoir. The air coming in contact with the mercury, was purified by passage over stick-potash and phosphoric anhydride. Sealed on to the gauge was a long U tube containing $\frac{1}{4}$ gram. of cocoanut charcoal placed in a small enlargement at the bend, the whole being arranged for liquid air or other cooling for any desired length of time. The object of this cooled charcoal is to take up and condense all adventitious gases, other than hydrogen or helium, which might arise from minute leakage, or otherwise be generated in the apparatus. The radium chloride was contained in a small bottle standing in a cylindrical glass bulb, connected by a T joint to the U tube. To the other arm of the T was sealed a bulb containing about 15 gram. of cocoanut charcoal for producing a high exhaustion in the apparatus when cooled to -190° C. Fig. 5 shows the arrangement of the apparatus, except that the special joint B there shown was employed subsequently, as described below. The whole apparatus was well exhausted by mechanical means, all the glass tubes being heated as well as the charcoal receptacles and the radium chloride. On immersing the receptacle containing the 15 gram. charcoal in liquid air for some hours, while the $\frac{1}{4}$ gram. charcoal and the radium chloride were kept hot, an exhaust of 0.00015 mm. was obtained. This charcoal receptacle was now sealed off, and the small $\frac{1}{4}$ gram. charcoal tube cooled in liquid air. In two hours an exhaust of 0.000054 mm. was reached.

The volume of the gauge and apparatus being approximately 200 c.c., the pressure in the apparatus, gives by a simple calculation the actual volume of gas produced measured at atmospheric pressure and the temperature of the laboratory, and thus the rate of production of helium is obtained. This, referred to the weight of radium present, gives the increment in terms of cubic millimetres of gas per gramme of radium per day. During the first three days the growth of pressure was very small, corresponding to about 0.3 c.mm. per gram. of radium per day. The cooling of the small charcoal was, however, interrupted owing to the holidays. The emanation consequently was not now completely condensed, but diffused into the McLeod gauge, where it had the opportunity of coming in contact with large surfaces of glass which no doubt held traces of organic matter and water. The result would be that hydrogen would be produced, and errors in the determination would accrue. This was practically confirmed in that the growth of pressure observed was very irregular, and a further experiment was carried

FIG 4.



out to which these objections did not apply. In this experiment the gauge as well as the connecting tubes were well cleaned out with nitric acid, and all thoroughly dried. The radium, after the 1100 hours in which it was under high exhaustion with frequent heating, was certainly in a more satisfactory condition. Further, to prevent the unchecked action of the emanation throughout the apparatus, the little charcoal condenser was maintained at a degree or two below that of the boiling point of oxygen by the use of old liquid air for a period of about six weeks. A larger quantity of charcoal was used, viz., 1 grm., the more effectively to condense out extraneous gases while leaving any helium substantially unaffected. This charcoal had been treated with chlorine at a red heat, and subsequently with hydrogen.

Beyond this the conduct of the experiment followed the lines of the former one. The mercury pump exhaust was continued for several hours—the charcoal being well heated meanwhile—and was carried to 0.002 mm. The large charcoal bulb was then cooled for several hours in liquid air while continuing the heating of the 1 grm. of charcoal and the radium salt. A pressure as low as 0.00005 mm. was thus obtained when the charcoal was sealed off. On now placing the U tube containing the 1 grm. of charcoal in liquid air the pressure registered was 0.000044 mm.

These conditions were maintained for five days, during which a steady growth of pressure was observed corresponding to an increment of approximately 0.3 c.mm. per grm. of radium per day. The radium was then heated with a small Bunsen flame as before to below a red heat, when the pressure was increased by about 40 per cent. This increase showed no sign of disappearing, but during the next week a decided but somewhat irregular growth of pressure was recorded. The radium was again heated, when a further increase of pressure was observed. In the succeeding five days it remained steady, only to be again increased on heating the radium. This treatment was repeated in all ten times at varying intervals during 1100 hours, and in each case the pressure rose on heating and remained fairly steady on standing. All the observations of the second set of experiments are graphically represented in Fig. 4. A mean line is drawn through the observations taken with the radium heated, giving a steadily maintained helium increment of approximately 0.37 c.mm. per grm. of radium per day.

In order to ascertain if any helium was occluded in the cooled charcoal and the surrounding glass, the latter was raised to near a low red heat while the tube containing the radium chloride was temporarily cooled in liquid air, with the object of condensing out and localising the emanation coming from the heated charcoal and preventing its access to the gauge. The temperature was maintained for an hour, and the charcoal was then allowed to cool, and finally replaced in the liquid air. The radium chloride was then allowed to warm up, and

was heated to near a low red heat for a short time. After these alterations no increase in pressure was observed, from which it may be inferred that the occlusion of the helium takes place mainly in that part of the apparatus where the radium chloride is situated.

On two occasions the charcoal was cooled in liquid hydrogen, viz., after 165 hours, and again after 650 hours. The proportionate reduction of pressure was the same in both cases, tending to show that the composition or nature of the gas remaining uncondensed by the charcoal in liquid air remained the same throughout, although steadily increasing in quantity.

In reference to this last point a separate experiment was made in which pure helium produced by heating 0.5 grm. uranite and passing the gas produced over 1 grm. charcoal cooled in liquid air, was subjected, at a small tension measured on a McCleod gauge, to the action of $\frac{1}{4}$ grm. of clean exhausted charcoal at the temperatures of liquid air and liquid hydrogen respectively. The ratio of the two pressures so obtained was in close agreement with that observed in the radium experiment.

A further test of the purity of the gas producing the permanent pressure observed in the radium experiment with the charcoal cooled in liquid air was made by simply cooling the bulb containing the radium in liquid hydrogen, allowing the charcoal meanwhile to warm up to 0° C. If any hydrogen had been present in the gas it is certain that there would have been an increase of pressure recorded, since although hydrogen is partially absorbed by charcoal in liquid air, yet it would not be materially reduced in pressure by cooling in liquid hydrogen. On allowing the charcoal therefore to warm up, any hydrogen thus expelled would remain and cause an increased pressure. Inasmuch as an increase was not recorded, it can be safely assumed that no hydrogen was present, and thus the gas pressure measured consisted entirely of helium.

A confirmation of this was obtained spectroscopically as follows: Two tin foil electrodes were placed round the narrow capillary measuring-tube of the gauge, near the closed end. These were about 3 cm. long and about $1\frac{1}{2}$ cm. apart, and were wired on with thin copper wire. The gas was compressed into this capillary space, as in taking an ordinary measure, to any pressure of the order of 2 or 3 mm., while an induction discharge passed in the gas. The spectroscopic examination of this discharge revealed only the six principal helium lines, mercury, and a trace of the carbonic oxide spectrum. I have shown that the carbonic oxide spectrum always occurs in electrode-less tubes.*

The curve showing the rate of production of helium is clearly linear within experimental errors, as shown in Fig. 4. The volume of the gauge given was unfortunately erroneously estimated, and the

* Proc. Roy. Soc., lxiv., 237.

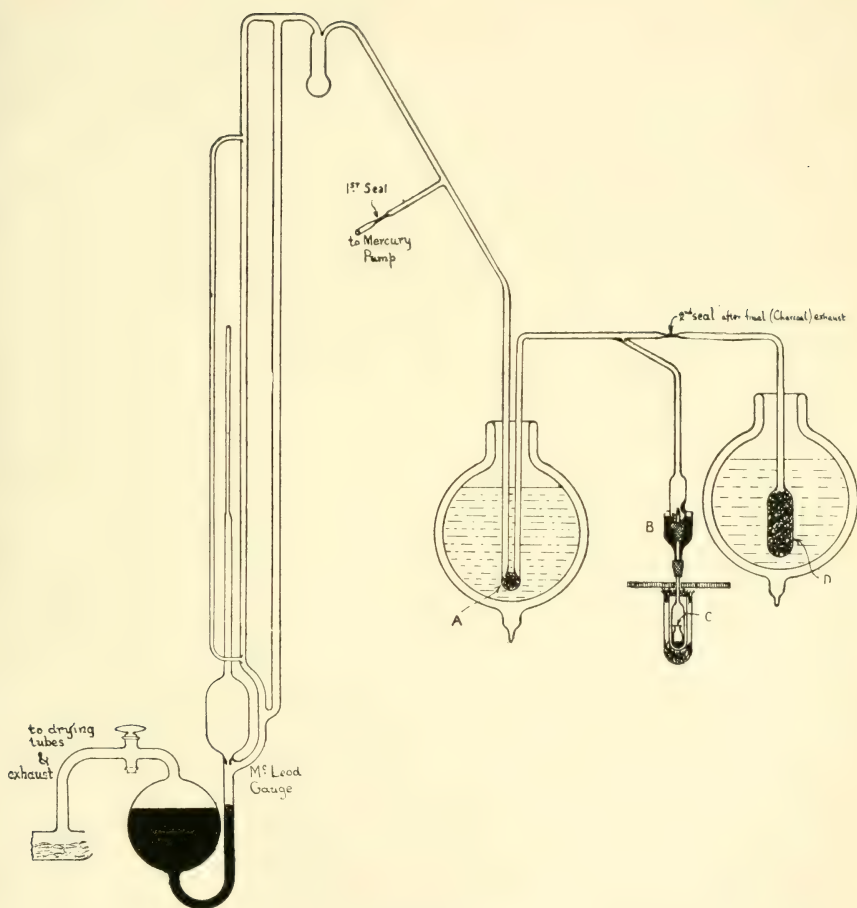


FIG. 5.

correct value was found subsequently to be 270 c.cm. This would give an increment of 0.499 c.mm., which must be taken as the true value obtained from this experiment.

The determination of the rate of production of helium made after a long period of storage was carried out as a confirmatory experiment. Fig. 5 shows the general arrangements of the apparatus used for this determination. A is the 1-grm. charcoal U-tube, D the 15-grm. exhausting charcoal bulb; the Ra_2 bottle is shown at C. This is similar to that employed in the shorter period determination, with the exception of a special vacuum-tight joint at B. This joint was so constructed that after thoroughly exhausting the gauge, etc., the drawn-out neck of the radium bulb could be broken off, thus allowing the pressure of the accumulated helium from the radium in the bulb to be rapidly determined. Just previous to such breaking the 1-grm. charcoal U-tube was placed in liquid air, in which it remained during all the subsequent operations. The amount of helium produced from 70 m.grm. of radium chloride during nine months was thus measured, and gave a pressure of 0.0163 mm., which is equivalent to a uniform rate of production of 0.463 c.mm. of helium per gramme of radium per day. The experiment was now continued for some five weeks on the lines of the former determinations, observations of pressure being made daily to observe the rate of production of helium from the radium while thus connected with the gauge and cooled charcoal.

At weekly intervals the radium was heated, and a rise of pressure was recorded. Between the times of heating, the pressure fluctuated somewhat, but in general only gave evidence of rising. Through the "heated" observations, which were six in number, the nearest straight line was drawn, as in the earlier experiments. The calculated increment so obtained, however, had a value as high as 1.26 c.mm. of helium per gramme of radium per day. This high value is untenable, and is explained by the action of the radium emanation on the surface of the vaselined rubber joint producing hydrogen, which would remain uncondensed by the cooled charcoal.

Thus we have 0.499 c.mm. as the value of the helium produced from the short-period experiments, and from the long period determinations the value 0.463 is obtained. The actual value is most probably between these two figures.

I am not aware of any previous direct measurements of the rate of production of helium from radium, but in a paper on "Some Properties of Radium Emanation," by A. J. Cameron and Sir William Ramsay,* the ratio of the amount of helium produced to that of the emanation was found to be 3.18, and as the amount of the emanation found by them was about 1 c.mm. per gramme of radium per day, the resulting helium, according to this experiment, ought to reach about 3 c.mm.,

* Journ. Chem. Soc., 1907, 1274.

or at least six times the rate of production found in the above experiments. I am at a loss to explain the origin of such grave discrepancies in the measured amount of the helium produced by radium. On the other hand, Professor Rutherford, in his work entitled "Radio-active Transformations," 1906, page 186, on the theoretical assumption that the particle is an atom of helium carrying twice the ionic charge, deduced from electrical measurements that the number of particles expelled per year per gramme of radium would reach 4×10^{18} , and as 1 c.cm. of a gas at standard temperature and pressure contains 3.6×10^{19} molecules, the volume of helium produced per year would amount to 0.11 c.cm., which is equivalent to about 0.3 c.mm. per day.

It is interesting to note that a more recent calculation made by Rutherford from theoretical considerations of radio-active data, gives a result of 0.45 c.mm. of helium per gramme of radium per day, as compared with the earlier calculation of about 0.3. The agreement between experiment and the theoretical prophecy of Rutherford is remarkable considering the difficulties to be overcome in the observations; substantiating as it does the accuracy of the theory of radio-active changes which he has done so much to develop.

If the helium in the atmosphere was produced chiefly from the radium present in sea-water, Strutt's experiments demand 100 million years for its accumulation. Joly's examination of samples of the ocean beds from the 'Challenger' Expedition revealed the presence of considerable amounts of diffused radium, which would shorten this period. Such conclusions are based on the estimation of very minute quantities of radium, and it is just as likely that 50 million years might suffice to account for all the helium present in the atmosphere as the 100 million year estimate given above.

[J. D.]

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 28, 1910.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

THE REV. CANON BEECHING, M.A. D.Litt.

The Spiritual Teaching of Shakespeare.

[The Lecture is printed in full in
"The Nineteenth Century and After" for April 1910.]

WEEKLY EVENING MEETING,

Friday, February 4, 1910.

THE RIGHT HON. SIR JOHN FLETCHER MOULTON, P.C. M.A.
F.R.S., Vice-President, in the Chair.

PROFESSOR WILLIAM BATESON, M.A. F.R.S.

The Heredity of Sex.

[NO ABSTRACT.]

GENERAL MONTHLY MEETING.

Monday, February 7, 1910.

SIR JAMES CRICHTON-BROWNE, M.A. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Henry Hermann Gruning, Esq., M.Sc. F.R.A.S.,
Kenneth Robert Hay, Esq., M.A. M.B.,
Henry Keatley Moore, Esq., J.P. B.A.,
Mrs. Sington,

were elected Members of the Royal Institution.

The Honorary Secretary announced the decease of Dr. Ludwig Mond on December 11, 1909, of Dr. Shelford Bidwell on December 18, 1909, and of Professor Friedrich Kohlrausch on January 17, 1910, and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted :—

Resolved, That the Managers desire to record their sense of the loss sustained by the Royal Institution, and by Science, in the decease of Dr. Ludwig Mond, Ph.D. (Padua and Heidelberg), D.Sc. (Oxon and Victoria), F.R.S. F.C.S. F.I.C. Grand Cordon of the Crown of Italy, Member of the Accademia dei Lincei, Rome, one of the most distinguished Industrial Chemists, President of the Society of Chemical Industry (1899) and of the Chemical Section of the British Association (1896).

Dr. Mond was distinguished for his discovery of the novel and remarkable class of Volatile Compounds of Carbonic Oxide with the Metals, and as the author of technical improvements in the manufacture of Alkali by the Ammonia Soda Process, and of processes of Production of Pure Nickel from its Ores.

Dr. Mond became a Member of the Royal Institution in 1883, a Visitor in 1889, a Manager in 1891, and Vice-President in 1894. He delivered a Friday Evening Discourse on June 3, 1892, on "Metallic Carbonyls."

Dr. Mond transferred to the Members of the Royal Institution, under a Deed of Trust in 1896, the Freehold of the house, No. 20 Albemarle Street, together with the furniture, apparatus, and appliances therein, and founded the Davy Faraday Research Laboratory of the Royal Institution, for the purpose of promoting, by original research, the development and extension of chemical and physical science, making a generous endowment to carry on the work of the Laboratory, and also providing increased accommodation for the Royal Institution. He always showed his keen interest in the work carried out in the Royal Institution by liberally contributing towards the promotion of experimental research at low temperatures, and also to the general fund of the Institution.

The Managers desire to offer, on behalf of the Members of the Royal Institution, their expression of the most sincere sympathy with Mrs. Mond and the family in their bereavement.

Resolved, That the Managers of the Royal Institution desire to record at this, their first Meeting subsequent to his death, their sense of the loss

sustained by the Institution, and by Science, in the decease of Dr. Shelford Bidwell, M.A. Sc.D. (Cambridge), F.R.S. Author of "Curiosities of Light and Sight," and late Manager of the Royal Institution. Dr. Shelford Bidwell was elected a Member of the Royal Institution in 1880, became a Visitor in 1886, and a Manager in 1892. He always took an interest in Experimental Scientific Research, but especially in relation to Electricity, Magnetism, and Optics. Dr. Shelford Bidwell delivered Friday Evening Discourses on the subjects of "Selenium and its Applications to the Photophone and Telephotography" (1881), "Magnetic Phenomena" (1890), "Fogs, Clouds, and Lightning" (1893), and "Some Curiosities of Vision" (1897).

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with Mrs. Bidwell and the family in their bereavement.

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution, and by Science, in the decease of their Honorary Member, Professor Friedrich Kohlrausch, Member of the Academy of Sciences of Berlin, Honorary Fellow of the Royal Society, President of the Physikalisch Technische, Reichsanstalt in Charlottenburg (from 1895 to 1900), Honorary Professor of Physics in the University of Berlin, and the author of numerous works giving the results of experimental investigations in many branches of Physics, chiefly in connection with the Theory of Electrolysis, and Physical Measurement.

Professor Kohlrausch was elected an Honorary Member of the Royal Institution in 1904.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with the family in their bereavement

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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- Cambridge Philosophical Society*—Transactions, Vol. XXI. No. 10. 4to. 1909. Proceedings, Vol. XV. Part 4. 8vo. 1910.
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- Civil Engineers, Institution of*—Proceedings, Vol. CLXXVIII. 8vo. 1909.
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- Dax Société de Borda*—Bulletin, 1908, Part 4; 1909, Part 1. 8vo. 1908-9.
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- Author* for Jan.-Feb. 1910. 8vo.
- British Homeopathic Review* for Jan.-Feb. 1910. 8vo.
- Chemical News* for Dec.-Jan. 1909-10. 4to.
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- Dyer and Calico Printer* for Dec.-Jan. 1909-10. 4to.
- Electrical Contractor* for Dec.-Jan. 1909-10. 8vo.
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- Engineer-in-Charge* for Dec.-Jan. 1909-10. 8vo.
- Engineering* for Dec.-Jan. 1909-10. fol.

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 Science Abstracts for Dec.-Jan. 1909-10. 8vo.
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- Warsaw, Society of Sciences*—Comptes Rendus, Vol. II. 1909, Nos. 7-8. 8vo.
- Washington Academy of Sciences*—Proceedings, Vol. XI. 8vo. 1909.
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- Monthly Statistical Abstract for Sept. 1909.* 4to.
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WEEKLY EVENING MEETING,

Friday, February 11, 1910.

GEORGE MATTHEY, Esq., F.R.S., Vice-President, in the Chair.

CHARLES E. S. PHILLIPS, Esq., F.R.S.E. *M.R.I.**Electrical and other Properties of Sand.*

THIS material, which flows so freely through my fingers and which may be poured in the manner of a liquid from one vessel to another, is common sand.

Specimens from various parts of the world are exhibited upon the table. There are sands from the Sahara Desert, from New Zealand, France, Scotland, and several parts of England. I am indebted to Mr. Harrison Glew and Mr. George Draper for many of these.

There are also bottles of the coloured sands from Alum Bay in the Isle of Wight and Redhill. It may be pointed out at once, that this colouration is merely due to the presence of an adherent layer of oxides or hydroxide of iron, for even varieties which appear under the microscope to contain little or no coloured particles, generally have a trace of iron clinging to the grains.

For instance, a small quantity of white sand from Charlton, having been wetted with strong sulphuric acid before the lecture, will yield on the addition of water a solution containing iron. A few drops of ferrocyanide of potassium give a strong blue characteristic precipitate.

Further, the so-called black iron sand from New Zealand (for a specimen of which I have to thank Mr. Morison) consists almost entirely of magnetite. If some of it is poured out upon a sheet of paper and brought near to a powerful magnet, you see that the grains fly eagerly to the poles and form large clusters there. This powder, on account of the regularity of its grains, their highly magnetic character and freedom from dust, is particularly useful in the laboratory for tracing lines of magnetic force. It is interesting to compare this with the black oolitic sand from Compton Bay in the Isle of Wight, for that is a silicate of iron and therefore non-magnetic. I am indebted to Mr. Colenutt, of Ryde, for the specimen upon the table.

I wish now to direct your attention to some of the phenomena connected with sand in large quantities, such as are met with upon wide stretches or drifts.

Blown sand, having been stopped by hedges and grass, gradually accumulates to a mound (Fig. 1)—in some cases with serious consequences. Dr. Vaughan Cornish, who has made a special study of



FIG. 1.



FIG. 2.



FIG. 3.



FIG. 4.



FIG. 5.



FIG. 6.

this subject, has clearly proved, however, that the formation of a sand dune is very frequently due to wind eddies. The second photograph upon the screen was, in fact, taken by him in Egypt, and depicts the steady irresistible march of millions of tons of sand, encroaching upon and slowly burying casuarina trees. (Fig. 2.)

To come nearer home, the seriousness of problems arising out of this state of things may be illustrated by two photographs obtained recently at Southport in Lancashire.

In the first one (Fig. 3), the back garden of a newly built house is nearly buried beneath the enormous hill which will probably soon cover the whole property. The second slide (Fig. 4) shows that the familiar appearance of a sandy beach at low water, with regular lines of ripples, may be produced by the direct action of the wind, and, incidentally, the utter futility of constructing an esplanade in such a neighbourhood. All these phenomena depend, in some measure, upon the size, weight and shape of the sand grains themselves.

Silica, a substance which occurs in numerous impure forms and constitutes a large portion of the rock masses known to geologists, is also to be found in a pure state as crystalline quartz. Here is an actual specimen about 18 inches long, which together with the beautiful group of quartz crystals by its side, known as amethysts (and tinted probably by a trace of organic matter), are the property of this Institution. Sand, therefore, being the result of rock disintegration assisted by the grinding action due to the motion of wind or water, varies in composition in different localities.

The next slides are micro-photographs taken with a low-power objective.

They represent some grains of sand found at Charlton and the Isle of Eigg respectively. (Figs. 5 and 6.)

The former are seen to consist of minute silica particles of very irregular form, whereas the larger grains of the Eigg sand are remarkable for their smoothness. It is owing to this fact that the latter possess a peculiar property to be referred to later.

Owing to the Sahara Desert having once formed the bed of a vast sea, it is of course found to be rich in marine deposit.

The damage which sand is capable of doing has been already referred to. It must not be forgotten, however, that its utility in the arts and crafts is of the utmost importance.

The Egyptians are reputed to have been the first to find a wide use for it. They were probably the earliest glass-workers in the world.

By the time glass making was begun in England, viz. about 1611, the Romans and Venetians had so far mastered the art of blending sand with other substances, that almost all the technical difficulties had already been overcome.

Now the melting point of silica being about 3000° C., it cannot

be worked in an ordinary furnace. In glass making, the sand is therefore heated with a salt of one or more of the alkaline group of metals, preferably with sodium carbonate. At a moderate temperature sodium silicate is formed, and if this be subsequently heated in the presence of either lead oxide or borax, the melting-point of the mass is still further reduced.

Here is a white-hot crucible containing sand so treated and melted. You see the glass pours out like treacle and sets rapidly into a transparent slab upon a hot brass plate.

Many useful applications, besides providing us with windows and glass-ware, have been found for sand, such as the decorating of hard surfaces by means of an impinging stream of its particles, scouring and cleaning, preventing slip on the roads, and so on. By no means the least important of these is its employment in war as a protection against bullets: a thickness of 20 inches of dry sand is proof against the modern rifle.

Now a mass of sand grains moving down a slope, by a motion consisting of rolling and sliding, meets with great opposition due to friction. The grains thus come into close contact with the surface, and a considerable charge of electricity may readily be obtained by the simple device of allowing them to impinge upon a suitable substance.

A stream of sand flowing from the base of this reservoir B (Fig. 7) strikes upon an oblique sheet of tin T, which is attached to an insulating pillar N. An electrostatic voltmeter connected with the metal plate serves to measure the electrical potential. You see that in a moment the tin becomes charged to 3000 volts. The needle, however, soon falls back. Something has changed. The plate has, in fact, become dulled and pitted where the sand struck it. A fresh part reproduces the high potential. Filter paper is far more serviceable and so is a wooden surface. One may rapidly obtain a potential of 6000 volts if the sand fall upon paper or wood, and this can be maintained for a considerable time. If the reading of the voltmeter diminishes, a fresh portion of the surface offered to the sand stream immediately brings it to its original value as before. The greater efficiency of paper (preferably filter paper), as compared with a metal sheet, in producing the electrification, appears to arise in the following way. A fine layer of dust soon becomes firmly imbedded in the metallic surface, so that further sand falling does not come into contact with the metal itself. On the other hand it is probable that these particles cut through the fibres of the paper and thus free themselves. I need hardly point out that the filter papers used should not be specially dried. Pieces which have been left about in a room for a few hours absorb sufficient moisture to ensure the right degree of conductivity.

The sign of the charge is always positive, in spite of the fact that a rod of silica rubbed upon the paper electrifies it negatively.

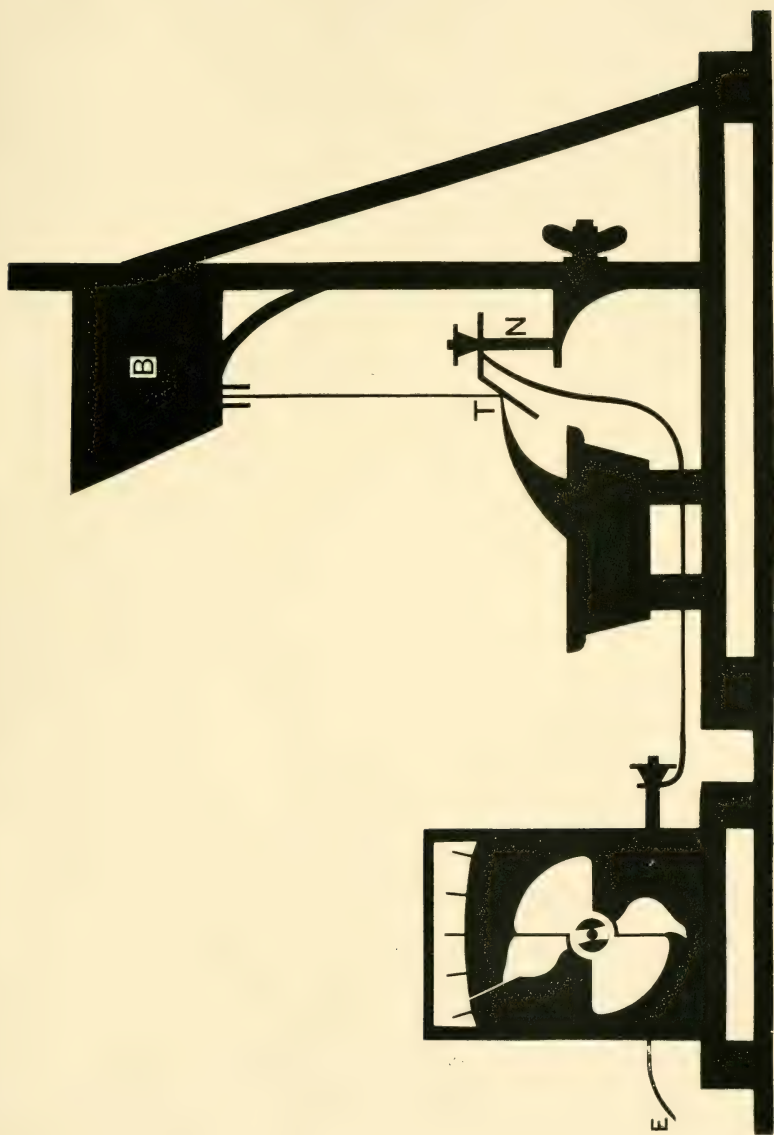


FIG. 7.

In 1843 Faraday had noticed this curious reversal, and briefly refers to it in his *Experimental Researches*. Even if the actual silica rod be broken up into pieces, say as large as an orange pip, and allowed to fall upon the paper held obliquely, the sign of the electrification is still positive. Further experiments have shown, however, that the sign of the electricity caused by friction against glass or silica depends upon the form of the rubbed surface. For instance, a strip of paper stroked by the smooth side of a tube of either substance becomes negatively electrified, whereas if the sharp edges of the end graze the paper the sign of the electrification of the latter is positive. Now sand consists of sharply angular particles of silica, and even the comparatively large pieces obtained by crushing the tube, as previously described, have razor-like jagged edges. We should therefore expect, from the result of the experiments just mentioned, that when either sand grains or even large silica chips fall upon paper they will electrify it positively—and this is what actually occurs. Why an edge of glass should give an opposite charge to that produced by a flat surface, when rubbed say with paper, is a question of great interest and difficulty. But that this is the explanation of the strange electrical behaviour of practically all powders appears certain.

The sand grains themselves become of course negatively electrified after striking the paper, so that this is often a convenient method of obtaining a high potential of either sign. Further, a stream of sand falling upon a metal plate will give a comparatively low potential, say 600 volts for an indefinite period, in spite of pitting, and a tolerably steady value may be obtained by catching the grains upon a second disc (previously dulled by a sand blast) connected with the apparatus required to be constantly electrified. As the charge increases upon this, a point is reached when some of the impinging sand particles become deviated by repulsion, so as to completely miss it. If the potential falls below the critical value a reverse action takes place, and the plate rapidly charges up.

Turning for a moment to the question of the electrification produced in sand by the friction between the grains, experiments upon this point may be conveniently made by catching the particles, which roll down the surface of a sand cone, upon a small wet insulated table. Any electrification of the latter may then be detected in the usual manner. If the grains are all of the same nature we should not expect to find other than slight irregular charges. The friction between particles differing in composition would give more definite results. Thus white sand racing over iron sand might be expected to show a charge. But experiment gave only a feeble electrification. I mention this because it is of interest in connection with the atmospheric electrical phenomena which often accompany sand storms in hot climates. Even if the wind electrified the surface of the sand over which it blows, the charge would probably leak

instantly to earth, for in common with all powders it readily absorbs moisture into the interstices between the grains. When making electrical experiments with this material it is therefore essential to have it well warmed.

There is still much useful work to be done in studying the electrical conditions in the neighbourhood of wide stretches of warm sand swept by dry wind. Owing to lack of data, it is difficult to form an opinion as to the part which this substance plays in the remarkable electrical phenomena sometimes witnessed during a storm.

I spoke of allowing sand to run down itself. Here is a cell made by separating two glass plates, 14 inches square, by strips of wood along the bottom and top edges. The sides are open. Through a hole in the upper distance strip sand pours from a funnel and builds itself into a beautifully symmetrical conic section. Presently the base will so far widen that any further increase shoots the sand off through the open ends of the cell. When this point is reached the cone can no longer grow. A supply of white sand is then poured in and seen to run down the sloping sides without carrying any of the coloured particles with it. The base has spread out proportionately as the cone increased in height, so that the angle which the sides make with the horizontal shall be 35° . If the sand be wet or damp this law no longer holds. The addition of sufficient water materially diminishes the friction between the grains.

It is often observed when walking along the sea-shore, upon sand left wet by the receding tide, that for a moment the foot on touching the ground is surrounded by a comparatively dry area. This appearance is quickly followed, however, by one which indicates that the sand has gathered moisture, for on lifting the foot—which has by now probably sunk a little below the surface—the excess of water is particularly noticeable. In order to explain this we must have recourse to some ingenious experiments made a few years ago by Professor Osborne Reynolds. He pointed out that a number of particles, whether spheres or irregular grains, may fit together in such a way that the size of the spaces enclosed by them is either a maximum or minimum. Figs. 8 and 9 show a sectional view of a collection of spheres, arranged in what Professor Reynolds calls abnormal and normal piling respectively. It is evident that the spaces between the spheres are far less in the second than in the first case. Now here is an elastic bag tied upon one end of a glass tube. The arrangement is partly filled with sand and coloured water—the latter standing 2 inches in the tube, so as to serve as an index. If the bag is now tapped, all the particles in it become normally piled. We have seen that any departure from this arrangement will enlarge the spaces between them. It is no longer surprising to notice, therefore, when the bag is pinched and the grains are thus made to ride up on one another, that the liquid in the tube instead of rising, actually sinks.

Returning to the effect observed upon the sea-shore, we see that

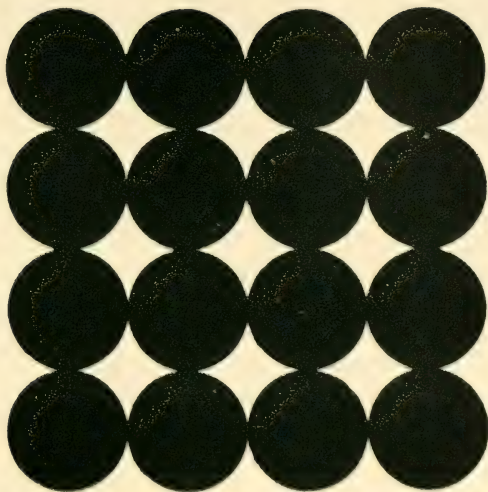


FIG. 8.—ABNORMAL PILING.

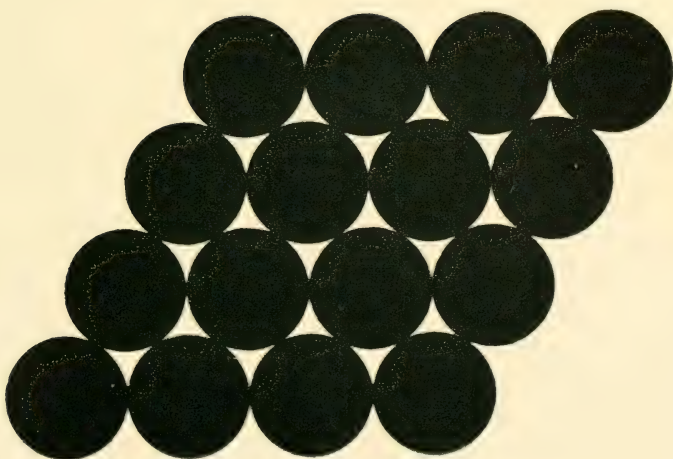


FIG. 9.—NORMAL PILING.

the pressure of the foot disturbs the arrangement of the sand-particles from one of normal piling to one in which the interstices between the grains become larger. Since these spaces were originally full of water (held up by capillarity) they are now no longer filled, and we obtain a comparatively dry area. Water is rapidly drawn in from all sides, however, by the partial vacuum formed in the interstices, and the internal friction diminishes. The sand feels insecure. On withdrawing the foot normal piling is resumed, the excess of water producing a puddle, until it slowly percolates away whence it came.

This brings me to the subject of quicksands.

A certain amount of unnecessary mystery seems to surround this matter. I hasten to point out that the grains of quicksands appear to be in no way extraordinary. Nevertheless the fact remains, that sand in certain localities upon the coast readily gives way under a load. Instances are recorded where a cart driven over a wet shore has rapidly disappeared below the surface. The general opinion seems to be that this is due to a soft underlying layer of clay or mud, which no doubt in some instances is the true explanation. Mr. Carus-Wilson, who is an expert in these matters, pointed out to me recently, however, that another factor may be the imprisoning of gas between the grains, due to decomposition of organic matter. Experiment certainly supports this view, for you see that one of these beakers of wet sand easily sustains a weight which sinks down in the other. Yet both appear similar. The sand in the second beaker, however, was mixed when dry with a powder capable of effervescing if wetted. In the neighbourhood of dangerous bogs, in Ireland especially, it is evident that a quantity of gas is imprisoned in the mud.

It must also be borne in mind, that any surface in so good a contact with wet sand that the air is excluded will be held fast by atmospheric pressure. And further, that an object so situated and tilted this way and that will rapidly become embedded and swallowed up. It is by this simple process that the celebrated Goodwin Sands have claimed so many victims. A large percentage of the vessels stranded upon them, however, float safely off on the rising tide, but now and then one is caught and doomed. In the past they have been responsible for many a shipping tragedy, and there is a pathetic interest attaching to the fact that ribs and other remains of ships, long lost and forgotten, sometimes re-appear for a time above the surface.

Since the advent of steam it is happily a rare occurrence for a vessel to be lost upon a sandbank.

In 1849 boring operations were carried out on the Goodwins by the engineering staff of Trinity House.

The Deputy Master and Brethren, whose generous offer of assistance on all matters relating to this subject I gratefully acknowledge, have kindly lent a model made at the time, which shows the nature of the sand found at increasing depths. Solid chalk was reached at 80 feet below the surface.

Let us now turn to some experiments upon the flow of sand through a tube.

This long glass barrel is filled and ready. I free the nozzle and collect the powder which flows out during 10 seconds. The quantity so obtained is placed in one pan of a balance. When the height of sand in the tube has fallen to only a few inches above the outlet, I repeat the operation, placing the second amount collected in the opposite one. You see that the pans again stand level. It is therefore clear that the sand pours out at the same rate irrespective of its height in the tube.

The question now is, how has the "head" been so completely destroyed? This may be answered by a further experiment.

A glass cell 2 feet high, 14 inches wide and $\frac{1}{2}$ inch deep, is closed in at the sides only (Fig. 10). A movable section of a cone O, made of wood and imitating one of sand, is pushed up through the lower opening. Resting upon this and fitting its sloping sides is a strip of felt D. If the wood section be lowered (as shown in the figure), the felt, resembling an inverted V, remains wedged between the glass back and front of the cell. A very small force, however, will dislodge it.

Suppose we replace the wood model and hold it in position by a strut S. Regarding this as a section of a sand cone, we see that its entire weight would be carried upon the base of the cell. Sand is now poured in from the centre of the top opening and rests upon the sloping felt. The point to notice is that it supports its own weight. When the particles are interlocked it resembles the span of an arch, for if I now remove the wood section the sand remains in position. When more is added and the cell is nearly filled the net weight is considerable, yet the felt bridge is not deformed in the least. Further, a wooden plunger P, fitting the top opening, and carrying heavy weights, may be inserted without increasing the pressure upon the felt.

Since the angle which the slope of a dry sand cone makes with the horizontal is 35° , the height, h , to which the particles will build in a tube of radius, r , so that the base of the cone corresponds to the diameter of the tube, is $h = r \tan 35^\circ$. If we consider an element of the section just referred to, it is evident that a vertical downward force applied to the top of the sand becomes resolved in two directions, making an angle of 55° with the vertical. Now applying the well-known formula for a symmetrical triangular frame loaded at its apex, we have

$$H = \frac{W}{4h} \quad . \quad . \quad . \quad . \quad (1)$$

where H is the horizontal thrust, W the load, l the span, and h the height.

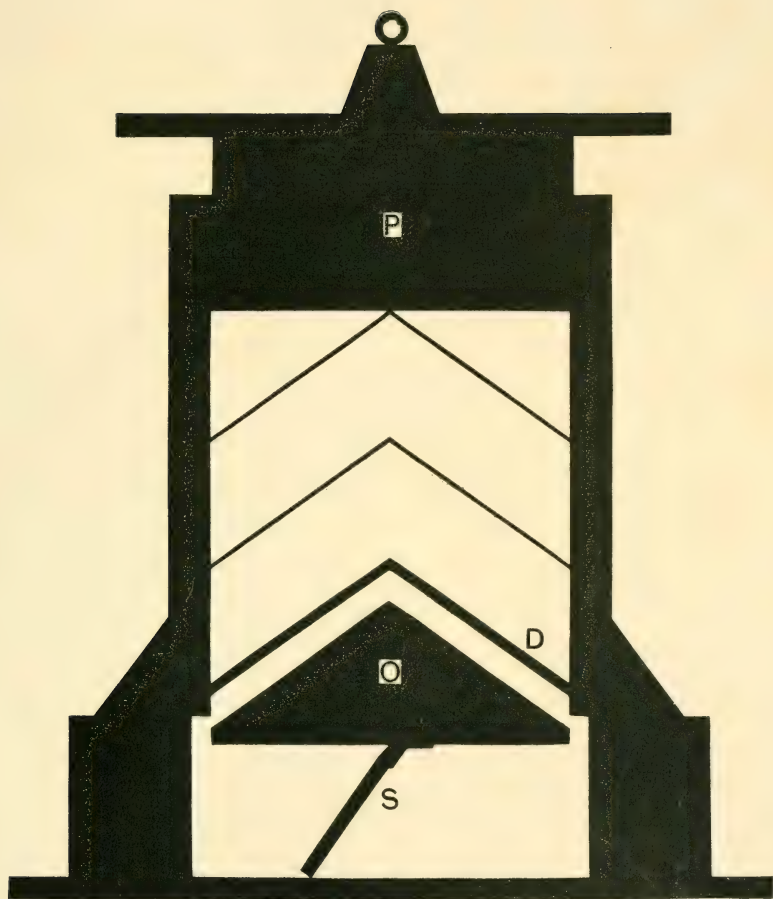


FIG. 10.

Regarding the cell as the section of a tube, $l = 2r$ and $h = r \tan 35^\circ$. Therefore, substituting these values in (1), we have

$$H = \frac{W}{2 \tan 35^\circ} = \frac{W}{1.4}.$$

The ratio of the force applied vertically, to that of the lateral thrust, is thus equal to twice the tangent of the angle which the slope of a cone makes with the horizontal, viz. 1.4.

For instance, if the vertical force due to a weight placed on the sand is 100 lb., the lateral pressure will amount to about 71 lb. A piston resting upon a column of sand only a few diameters high, contained in a strong tube closed at its lower end by merely a thin membrane, is capable therefore of sustaining very heavy loads.

In order to demonstrate this on a moderate scale I have arranged a sort of gallows, through the projecting arm of which a flanged brass tube is inserted vertically. This tube is 0.5 inch in diameter, and closed at its lower extremity with a piece of cigarette paper held in position by an indiarubber band. A small quantity of sand is tipped into the tube from above—enough to fill it to a height of 3 inches. The column within will therefore measure 6 diameters. The tube is then well tapped to ensure normal piling of the grains, and a loosely fitting iron plunger is inserted so as to rest upon the sand. Attached to the plunger is a cross-piece carrying a ring at each end which may be grasped with the hands. My assistant (who weighs about 11 stone) thus suspends himself safely, his weight being supported by the small sand column. If the piece of cigarette paper is now removed he is let down with an unpleasant jerk.

Some idea of the close arrangement of the particles may be gathered by noticing that a long column of sand, moving downward within such a tube, will produce a vacuum above it sufficient to lift water to a height of about 6 feet. (Experimentally shown.)

These experiments, upon loaded sand columns, clearly prove therefore how it is that the "head" is destroyed, and explain why the powder issues from an orifice at a uniform rate.

Lord Rayleigh applied this principle to a very interesting device which he used here some years ago for the purpose of slowly rotating a smoked disc. A weight stood upon a sand column contained in a glass tube. Its downward motion as the column lowered, due to escape of powder from a nozzle at the end, served to operate a train of wheels. The question arises, however, as to whether such a motion is quite uniform. In other words, does the sand move regularly in the tube? Experiments indicate that it is very difficult to obtain an absolutely uniform motion by this means. Friction appears to be the controlling factor. A tube oiled upon its inner surface is now

filled. On freeing the nozzle you see that the sand moves out by slow regular jerks. Certain curious rattling sounds, emitted occasionally by the column descending in a glass tube, also drew attention to the intermittent motion of the grains.

It seemed reasonable to hope therefore that this might be made sufficiently rapid and regular to give rise to a musical note.

Now many strange noises have been heard in the neighbourhood of large sand masses, when surface layers have been disturbed by someone walking over them. And there are curious shrieking sands—rarely met with upon the coast.

Thanks to the great kindness of Mr. Carus-Wilson, whose work in this direction is so well known, I am able to exhibit a remarkable specimen of sand from the Isle of Eigg in the Hebrides. When a plunger strikes down upon the grains contained in a suitable cup, you hear a piercing musical sound. Mr. Carus-Wilson attributes this to the friction between the particles, the effect being produced in much the same manner as that which results from gently rubbing an agate style upon glass. He has discovered musical sand in Poole Harbour as well as at other places.

The essential conditions for the production of this sound are :—

1. That the grains be nearly of the same size and rounded.
2. That they be clean and free from adherent fine dust.
3. That the vessel in which they are struck have sloping sides and be made of a suitable material.

But to return to the question of obtaining musical sounds from ordinary sand.

There stands, fixed to the wall, a large glass-fronted section of a tube. It is filled with alternate bands of white and black sand, the latter being about one-sixth as deep as the former. An outlet is provided at the bottom. This arrangement enables the motion of the different portions of the sand column to be observed while the powder issues from the orifice.

On freeing the nozzle we see that the centre of the lowest black band immediately falls, and that as the sand continues to escape, successive bands become similarly deformed. It is clear that the grains from the central part of the column are moving rapidly downward, and since no eddies can form in the remainder, the whole becomes divided into a core of moving particles and a large surrounding mass of dead sand. (Fig. 11.)

The diminished density of the axial region releases the lateral pressure upon the sides of the tube, and the upper part of the column suddenly slips until the grains again pack and seize as before.

Now if sand of a suitable fineness be slowly passed in this manner through a glass tube of correct dimensions, a musical note may be produced.

The tube should be about 1 inch in diameter, and filled with sand

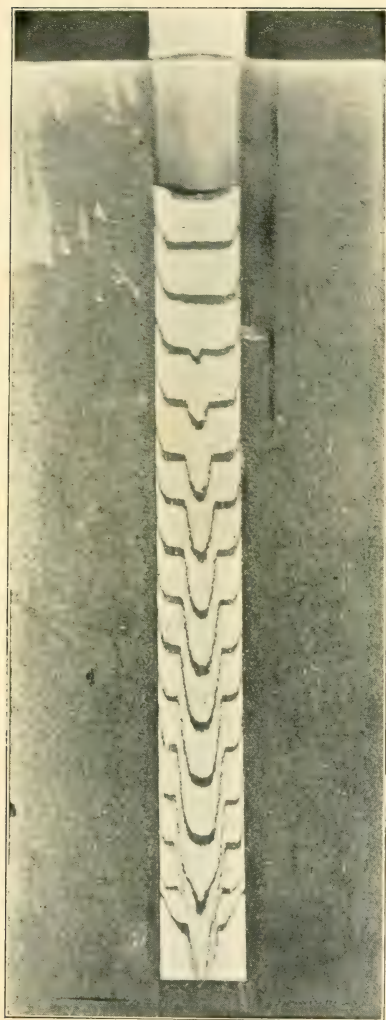


FIG. 11.

resembling that found in the Charlton pits. The length of the one now ready is 3 feet. When the flow begins, a curious rattling sound is heard which finally changes to a distinct musical note. It may be varied slightly, say to the extent of a whole tone or so, by gripping a part of the tube while the sand pours out. The two upper dark bands (Fig. 11) have not become deformed, except slightly at their ends owing to friction between the sand and tube. It is essential for the production of musical sounds, that the ratio of the length of a column to its diameter be such that the upper portion moves downward without central deformation. In order to explain the cause of the sound we must therefore consider the motion of this more or less compact body of particles.

Now, if the lower half of the tube be filled with mercury, and the rest with well-packed sand, the regular lowering of the liquid causes the granular piston to apparently stretch until its extension is about 2 per cent. of its original length. It is not until that point is reached that the upper layers begin to move downward. The particles, however, are no longer normally piled. A further slight movement of the lower layers causes the upper ones to follow and to over-run a little (owing to their momentum). Therefore, even if the mercury is adjusted to pour out uniformly from the orifice, the upper part of the sand column moves downward with an intermittent motion, analogous, in fact, to that of a weight drawn over a rough surface by an elastic string. It is also clear that within wide limits the motion of the upper layers may be independent of, or completely out of phase with, that of the lower ones and still produce a musical note.

The glass wall of the tube is thrown into violent vibration by the intermittent rise and fall of the lateral pressure upon it, so that damping the barrel raises the pitch of the note. The greater part of the sound is due, however, to the direct action of the sand column upon the air above it, for even a tight wrapping of tape but slightly affects its quality. Where the tube is filled entirely with sand the pitch of the note emitted rises as the column diminishes, owing to a proportional decrease of inertia.

In order to see in what way varying the friction between the grains would influence the result, we may fill the tube with magnetic sand and magnetise the column longitudinally. This can be conveniently done by winding a current-carrying wire round the tube. With such an arrangement, the sound produced by the descending column, though feeble at first, is strongly increased on magnetising the grains. Each time the circuit is "made," the sound, almost inaudible before, is plainly heard. In all cases the closeness of the grains, i.e. the proportion of normally piled particles, largely determines the pitch of the note. Other factors are the state of the glass surface, the size and roughness of the grains, as well as the rate at which they issue from the nozzle. By suitably adjusting all these conditions, a limited

number of notes may be obtained. So far I have succeeded in producing only five with any degree of certainty—one note being, in fact, obtained by damping the vibrations of the largest tube. The sound is hardly pleasant, but nevertheless I venture to play, if I can, a simple tune upon what may perhaps be called the sand-organ.

[C. E. S. P.]

WEEKLY EVENING MEETING,

Friday, February 18, 1910.

SIR FRANCIS LAKING, Bart. G.C.V.O. M.D. LL.D.,
Vice-President, in the chair.

HERBERT HALL TURNER, Esq. D.Sc. D.C.L. F.R.S., Savilian
Professor of Astronomy in the University of Oxford.

Halley's Comet.

THE published records of the Royal Institution mention no lecture on *comets* previous to 1882. In 1835, when Halley's comet was last with us, the publication of the records was suspended : but with the kind help of the Assistant Secretary, Mr. Young, references were found in the "Records of General Science" to two lectures given here on April 10 and May 1, on Halley's comet, by Dr. Lardner.

He gave an account of the calculations which had been made by Lalande and Clairaut, with the help of Madame Lepaute, for the return of 1759 which Halley had predicted ; mentioning how these three earnest workers had toiled incessantly day and night, not excepting meal times, for six months ; how they were handicapped by their ignorance of the existence of the planet "Herschel," of which the mass, Dr. Lardner remarked, was still an uncertain quantity (and Neptune was of course then unsuspected) ; how Voltaire had said that astronomers never went to bed in 1758 for fear of missing the comet ; and how it was at last discovered on Christmas day by a farmer near Dresden. He estimated that there were 7 million comets in our system, and considered that the mass of a comet could not be as great as one five-thousandth part of that of our earth. Some of the phraseology reads a little strangely to us nowadays. "All the planets," he said, "are collected in the Zodiac, from what cause we are not aware." Comets, on the other hand, are not so collected, and the ellipticity of their orbits "does not depend upon physical laws, but upon the will of the Creator."

The publication of the Proceedings of this Institution was resumed in 1851, but the first nine volumes contain no reference to comets in the index,* although the period to which they refer includes the conspicuous comets of Donati in 1858 and Tebbutt in 1861.

* Since these words were in type, Mr. Young found that Professor Robert Grant delivered a course of seven lectures at the Royal Institution in 1870 on "the Astronomy of Comets," and that a report appeared in the "Illustrated London News" of that year.

the law of gravitation, and realised, from the form of Kepler's third law, that there might exist a force, varying inversely as the square of the distance, which would approximately explain the movements of the planets round the sun and of the moon round the earth; possibly also the rate at which bodies fell towards the earth. But nothing practical came of his speculations, for several reasons: he was shy and reserved as to his discoveries, and he realised a grave difficulty which no one else seems to have suspected—viz. that though the huge sun and planets might be considered to attract one another as mere particles when separated by the planetary distances, the same ideas were not applicable to bodies attracted close to the earth's surface. His reserve was overcome by Halley, who visited Cambridge in August, 1684, with the express purpose of finding out how far Newton had got in applying the idea of gravity. Halley was overjoyed to find that Newton had already proved the proposition that bodies attracted gravitationally would describe ellipses; he insisted on this "and much more" being published, and paid for the publication himself, and his generous insistence was the means of Newton discovering the wonderful proposition that spheres attract as particles at all (external) distances however small:—the theory of gravity was complete! The *Principia* appeared in 1687.

For nearly twenty years Halley had no opportunity of following up this great success. We may here glance at a few leading events in his life, and it may help us in remembering dates to remark that he was a contemporary of the famous Vicar of Bray. Born under Cromwell on November 8, 1656, educated at St. Paul's School and at Queen's College, Oxford, "in good King Charles's golden days," he undertook a voyage to St. Helena to catalogue the stars of the Southern Hemisphere, and showed his gratitude to the King, who had aided his expedition, by re-naming one of the Southern Constellations the "Royal Oak"—a name which has not survived. From his study of star positions, Halley was led to remark that some of them must have moved, and he thus laid the foundation of our modern knowledge of "proper motions."

It was in the reign of "Royal James" that Halley published Newton's *Principia*. William of Orange commissioned Halley a captain in the Royal Navy, and gave him command of H.M. *Pink*, the *Paramour*, for "an expedition to improve the knowledge of the longitude and variations of the compasse," an expedition which he carried out with great scientific success and without losing a man from sickness, though he had to return from his first start in consequence of mutiny.

"When gracious Anne became our Queen," Halley was appointed Savilian Professor of Geometry at Oxford, and it was then that he made his famous discovery about the return of comets. George the First made Halley Astronomer Royal, and he died on January 14, 1742, in the reign of George the Second, at the ripe old age of 85.

It was probably lack of leisure, due to his many absorbing interests, which prevented his earlier following up the possibilities of work opened out by the great discovery of the law of gravitation. But on being appointed Savilian Professor of Geometry at Oxford in 1704, he set to work to calculate the orbits of as many comets as he could find records of, according to the principles and methods furnished by Newton, and after "incredible labour" published the elements of 24 comets. In three cases, the elements bore a close resemblance to one another, as shown in this table :—

Date of Comet.	Interval.		Longitudes of		Inclination.	Distance.
			Node.	Perihellion.		
	Y.	M.	Degrees.	Degrees.	Degrees.	
1531, Aug. 24	76	2	49	301	18	0·57
1607, Oct. 16			50	302	17	0·59
1682, Sept. 4	74	11	51	302	18	0·58

It will be seen that the last four columns have nearly the same figures, and the quadruple coincidence suggested to Halley that the same comet had appeared three times, travelling in an elliptic orbit. One circumstance, however, was puzzling : the interval between the first two appearances was 76 years 2 months, between the second and third 74 years 11 months. But Halley was ready with an explanation which we now know to be correct. He had noticed that the planets Jupiter and Saturn disturbed each other (as they should according to the great law of gravitation). He argued that they might also disturb a comet and the effect would be much greater : for a not very large disturbance was capable of sending a comet away for ever—outside the sun's sphere of influence : hence a smaller disturbance could lengthen its journey appreciably.

In fact he saw no difficulty which could not be explained away in concluding that the three sets of elements in Table I. referred really to the same comet ; and he predicted that it would again return in another seventy-five or seventy-six years, say in 1758 or thereabouts. This return he could himself hope to witness (he died in 1742 at the ripe age of eighty-five), but he trusted posterity, when the comet did reappear, to credit an Englishman with the prediction. '*Quocirca si secundum predicta nostra redierit iterum circa annum 1758, hoc primum ab homine Anglo inventum fuisse non inficiabitur aequa posteritas.*'*

Before the reappearance of the comet was due there was ample

* These words are not in the original paper, but were added in a later edition, which, however, was not published until after Halley's death, in 1749.

time to make calculations of the effects which Jupiter and Saturn would actually produce, though the planets Uranus and Neptune, which also contributed something to the perturbation, were as yet undiscovered. These calculations foreshadowed a greater delay than had been anticipated, and the comet did not return to perihelion until 1759. But the delay, the causes of which Halley had so expressly recognized, really added fresh laurels to his success in prediction. The comet went round once again, and reappeared in 1835: once again, and has come back to us once more. It has been photographed and seen in telescopes of moderate power. In May, we hope it will be easily seen with the naked eye. Until recently the calculations of the circumstances of return had been chiefly made by foreign astronomers, but for the present return, Messrs Cowell and Crommelin of the Royal Observatory at Greenwich have outdistanced all competitors and been awarded the prize of the *Astronomische Gesellschaft* for their most successful prediction. They used special methods for the work, such as had been devised shortly before by Mr. Cowell for dealing with the exceptional case of the tiny eighth satellite of Jupiter. To understand the difficulties and the method of meeting them, let us recur for a moment to the statement of the law of gravitation, and consider what it means in detail. Suppose we are at a distance of 16 feet* from an attracting centre and find the pull is 1 lb. If we halve the distance, *i.e.* go 8 feet nearer, then the pull is 4 lb.; if we go 4 feet nearer (halving the distance again), the pull is 16 lb.; 2 feet nearer and it is 64 lb.; 1 foot nearer and it is 256 lb. Notice particularly that as we approach the centre the force is not only itself greater, but *increases* more rapidly. At 16 feet distance, an error of 1 foot would not much matter: at 2 feet distance it makes the enormous difference between 64 and 256 lb. pull.

From the great increase in the force itself in the neighbourhood of the attracting body, it is readily understood that when a body, such as the moon, is very close to the earth, and remote from the sun, the attraction of the earth, in spite of its much smaller mass, affects the motion more than that of the sun; and the motion of the moon is thus mainly controlled by the earth. Although we cannot solve the problem of the movements of these three bodies (or any other three) in finite terms, we can solve it by a series of approximations. First we may suppose the sun non-existent, when the moon will describe an ellipse round the earth in a focus. Next we may consider how the distant sun disturbs this ellipse; but then we immediately find troubles arising from the second feature of the law of gravitation above noticed—the fact that a slight change of position of an attracted body, when near a centre of attraction, makes a large difference in the attraction. The disturbance of the moon from its ellipse by the sun may be slight, but it alters considerably

* These figures were illustrated by a practical experiment.

the attraction of the earth which we formerly assumed, and hence our first calculations must be sensibly altered. Without following the difficulties into detail, we can see how they arise, and how they will continue to arise if we look at the problem in this way. We might call the first orbit O_1 , calculated with the earth alone; in the second, O_2 , we should consider the sun's attraction; but now we must go back and correct the earth's attraction, which is seriously modified even by the slight change from O_1 to O_2 , and we get a new orbit O_3 ; then we must correct the sun's attraction, and so on. This is not the actual way in which the calculations are made, but it suffices to illustrate some prominent features of the work, and especially that it is conducted by continued approximations. It will suggest also, that if, instead of being always chiefly under the attraction of one of the bodies (the earth) and only slightly disturbed by the other—if instead of this the control were more equally divided, the difficulties would be increased. Recently a satellite of Jupiter (the eighth) was discovered, which is so far from Jupiter, that his control is not much more decisive than that of the sun, and the difficulties were so enormously increased that special methods of calculation had to be devised. The notion of determining a complete orbit round Jupiter first of all had to be given up entirely; it was necessary to follow the satellite step by step, assessing at each point the exact attraction of the sun and Jupiter for a little distance ahead, verifying that the satellite would then move in the estimated way, correcting the assumption for any discrepancy found until forecast agreed with result, and so making each point in the path secure before venturing to the next. The process required great labour, but the labour was not grudged in the hope of success, and the hopes were justified. After an interval of some months, during which it was lost in the glare of the sun's rays, the tiny satellite was found again close to the place which these laborious calculations had found for it; and it is, perhaps, not too much to say, that in no other way at present known could this satisfactory result have been attained.

The problem of Halley's comet is somewhat different. Its movements are chiefly determined by the sun's attraction alone, but there are occasions when it passes near Jupiter or one of the other large planets, when for a brief period the disturbance of the planet is of enhanced importance, and when the methods just described are suitable. Indeed they are suitable generally except for the drawback of the labour involved, and we have learnt by long experience in astronomy, that when there are two alternative procedures, one of which involves more labour but seems to promise rather greater accuracy, it is often a real saving of time in the end to face the labour at once. This at any rate was the decision at which Messrs. Cowell and Crommelin arrived; they determined to apply to Halley's comet the procedure found successful in the case of Jupiter's eighth satellite, and after an herculean piece of computation, obtained such

accurate results that Dr. Max Wolf of Heidelberg was enabled to pick up the faint comet on one of his photographs on September 11, 1909, close to the place predicted.

It may not be quite clear wherein lies the chief difficulty of the work, or the chief merit of the accuracy attained. Is not the orbit of the comet well known after so many revolutions have been observed? Let us suppose for a moment that this were so; that there were rails laid on the track round the sun on which the comet must travel like a train, without deviating so much as a hair's breadth. There would still remain one element of uncertainty, viz., the time at which it would arrive. The disturbances of the planets would now be limited to delays and accelerations, such as attend the passage of a train through wayside stations. We must add together all the delays and subtract any time saved. What accuracy may we expect in the final result? Remember that the whole journey takes 75 years, and that there may be unknown disturbing causes. We shall be fortunate if the time is predicted within a week; the prediction of a famous French astronomer differed by a whole month from that of Messrs. Cowell and Crommelin, and that of another unknown calculator by two months. It was therefore gratifying to them to be correct within three days.

It may be remarked here that this difference, small though it is, nevertheless considerably alters the *apparent path of the comet as seen from the earth*. We have supposed the actual path round the sun to be prescribed; but the apparent path as seen by us who are moving also is a different matter. The paths of the planets round the sun are nearly circles; but as seen from the earth, their motions are very different—there are times when they seem to stand still and then move backwards. Hence we must be prepared to find the *apparent* path of the comet, as seen by us, modified considerably by a delay in its orbit, even though that orbit may be accurately prescribed; and in searching for the comet we depend, of course, on the *apparent* path, and not the real. To see the kind of difference that may result, let us compare the two ephemerides given by Messrs. Cowell and Crommelin, one in March 1908, when they had made approximate calculations only, and the other now that they have made their laborious and accurate calculations, and corrected them finally by the latest news brought by the comet itself.

The change is for our advantage in the northern hemisphere, but the possibility that the comet might be visible during the total eclipse of the sun on May 8 in Tasmania has now disappeared. The comet will have set before the eclipse begins. One peculiarity of the present return, to which Mr. Crommelin has drawn attention, is that the interval since the last appearance is the shortest on record by some five and a half months. It is also noteworthy that this quick return is the paradoxical result of special *delays* by Jupiter. The orbits of the comet and of Jupiter cross (approximately) at two

Right Ascension. Prediction of					Declination. Prediction of				
March, 1908.			January, 1910.		March, 1908.		January, 1910.		
	H.	M.	H.	M.	Deg.	Min.	Deg.	Min.	
Jan. 1,	1	46	2	10	+ 9	38	+ 11	30	
Feb. 1,	0	32	1	3	+ 6	15	+ 8	20	
Mar. 1,	0	2	0	34	+ 5	32	+ 7	55	
April 1,	23	29	0	10	+ 4	23	+ 8	3	
May 2,	23	36	23	55	+ 2	29	+ 8	18	
„ 6,	0	6	0	3	+ 2	37	+ 9	5	
„ 10,	2	3	0	22	+ 3	3	+ 10	30	
„ 14,	7	22	1	6	+ 0	34	+ 13	27	
„ 18,	9	10	3	7	- 0	41	+ 18	51	
„ 22,	9	44	7	3	- 1	9	+ 15	14	
„ 26,	10	0	8	59	- 1	21	+ 6	59	
„ 30,	10	10	9	42	- 1	29	+ 3	15	

points, and the time occupied by the planet in journeying from one to the other in its orbit is nearly equal to that occupied by the comet in *its* orbit: hence if both are near one crossing point together, they will also be near the other point together. Jupiter got behind the comet on both these occasions during the last round and pulled it back, the direct effect of which is, of course, delay; but an indirect effect overpowers this; the comet does not go so far afield when checked at the outset, and so comes home quicker. When the juggler throws up his potatoes very vigorously they take a long time to come down on to his forehead: but if something were to check his throw, the interval would be smaller.

As regards the limit to which the comet attains, it journeys beyond the orbit of Neptune, some thirty times the distance of earth from sun, say 3000 million miles. It does not go far beyond this orbit, but by the peculiarities of elliptic motion under gravity it spends half its time doing the small arc which lies beyond Neptune's orbit. Attention was called earlier in the lecture to the rapidity with which gravity increases near the attracting centre; as a consequence the comet's movements are greatly accelerated, and it describes in a few weeks an arc equal to that over which it spends as many years at the other extreme of its orbit. This lingering in the part of the journey remote from the sun becomes more and more pronounced as the time of revolution increases. We know that there are comets which take thousands of years to return to the sun, instead of only 75 to 80 like Halley's comet: the greater part of this time they spend at a great distance, travelling so slowly as to be almost stationary. Suppose there were 1000 comets, each taking 1000 years to revolve round the sun, of which about a year was spent in its neighbourhood (and within our field of observation). This would provide us with about one comet a year. If the period were 10,000 years we should require

10,000 comets under the same circumstances to provide us with one a year. Now, we scarcely know enough about comets in general to know what the average period is, but it is probably something in the thousands of years, and there must therefore be thousands of comets, which spend most of their time at a distance from the sun, hanging between successive journeys to him, while there *may* be millions—our knowledge is too imperfect to guide us. We can, however, assign a superior limit in some such way as this: if a comet travelled more than half way to the nearest star (supposed equal to the sun) it would be liable to desert the sun for the star. We may therefore take something like 100,000 times the earth's distance from the sun as a limit for the average major axis of a comet's orbit, giving a period of about 10 million years. Since we see about 3 comets of long period per year and we may miss several, there may be (say) 50 million comets, but there are not likely to be more, assuming them permanent members of the solar system.* In forming a mental picture of the universe we must not forget to include this possible envelope of comets surrounding each star.

Messrs. Cowell and Crommelin have not limited their work to the present return of the comet, but have carried its history back through the ages to 240 B.C. Nearly a century after Halley's death a fine piece of work in continuation of his great discovery was accomplished by Mr. J. R. Hind, who, by examining old records and especially the Chinese Annals, was able to indicate with fair probability the following previous appearances of Halley's comet:—

PROBABLE EARLY RETURNS OF HALLEY'S COMET (Hind)

A.D. (1682)	1223	760	295
(1607)	1145	684	218
(1531)	1066	608	141
1456	989	530	66 A.D.
1378	912	451	12 B.C.
1301	837	373	

But a more complete discussion was needed: several of the identifications were uncertain, and one or two of them turn out to be wrong. Messrs. Cowell and Crommelin, with the assistance of three volunteers, Dr. Smart, Mr. F. R. Cripps, and Mr. Thomas Wright, have proceeded backwards step by step, making each return sure before proceeding to the next. B.C. 87 and (probably) B.C. 240 have been added to the list of returns. Hind was wrong in 608 (a year and a half too late) and in 912 (four months too early) and in 1223 (ten months too late). The comet of 1222 was a bright one, seen both in Europe and China, but its identity with Halley's was not suspected until this careful investigation was made. The date 1066 will be noticed as that of the Norman Conquest of

* Dr. Lardner in a different manner estimated 7 millions.

England. Taking the cue from Halley's pride in an English achievement, we may note that 1531 was the year in which King Henry VIII. was declared Head of the English Church; that 1607 saw the foundation of Jamestown, with which the history of our lost colony the United States may be said to commence; that 1758 saw the birth of Nelson and 1759 the Battle of Quiberon Bay. Mr. Crommelin has called attention to the curious parallel between the general elections in England in 1835 and 1910. The numbers of the parties at the previous elections and after the election in the comet year are curiously parallel :—

	1835	1910
Liberals in previous	514	513
Liberals after election	385	397
Opposition in previous	144	157
Opposition after election	273	273

The comet of 66 was perhaps the sword mentioned by Josephus as hanging over Jerusalem for a whole year together, which he took to be a warning of its impending destruction.

The return of 1456 originated a false story (which grew with age and will be hard to eradicate from the various literary channels into which it has found its way) that Pope Calixtus III. had cursed the comet. The true facts have been clearly stated several times, and it has been shown that the legend has no foundation. A very complete discussion of the matter by Father Stein of the Vatican Observatory has just been published as No. II. (1909) of the publications of the Specola Vaticana. With the original story must go also the following "elaborate witticism" quoted by Newcomb (in his "Reminiscences of an Astronomer," page 282) as due to Professor J. C. Adams: "In view of the fact that the only human being ever known to have been killed by a meteorite was a monk, we may concede that after four hundred years the Pope's bull against the comet has been justified by the discovery that comets are made up of meteorites."

As regards the present return, special efforts were made to detect the comet as early as possible. Many photographic plates were exposed in powerful telescopes during the winter 1908-9, but without success. The first to announce an image of the comet was Dr. Max Wolf, of Heidelberg, who found it on one of his plates taken on September 11, 1909, close to the place predicted by Messrs. Cowell and Crommelin. This practical proof of the correctness of their work led almost immediately to the award to them of the prize offered by the Astronomische Gesellschaft.

Guided by the information afforded by the Heidelberg photograph, a new search was made on plates previously taken at the Royal Observatory, Greenwich, and the tiny faint image of the comet was then found on plates taken on September 9. Dr. Wolf found images on his plates of August 28, and ultimately the comet's

image was detected on a plate taken at Helwan in Egypt on August 24, with the Reynolds reflector, the mirror of which was made by the late Dr. Common, F.R.S.

What of future returns? Can we expect reappearances of the comet to continue indefinitely? Our knowledge of the nature and history of comets, though it has advanced rapidly in the last few decades, is still scarcely sufficient to enable us to answer with confidence; but the indications are that comets are continuously being disintegrated and are ultimately broken up, perhaps into a swarm of meteors. The tail of a comet probably represents its losses at the moment. The tail or train does not, as might be supposed, follow behind the head in the same path, as the smoke follows an engine: it is as often in front of the head as behind it. The tail always, in fact, points away from the sun, as though a strong current of air were blowing in all directions outwards from the sun, determining the direction of the tail as the wind determines that of a streamer. And there is actually this in common between the cause of the tail and a current of air—that both have a tendency to drive away lighter particles from heavier. We blow away chaff from grain: and the fierce *light-pressure of the sun* (to which many astronomers now attribute the formation of cometary tails) in the same way separates the lighter constituents of the comet and drives them outward into space. Possibly we are wrong in assigning such large powers to light pressure—the older view that the repulsive action is electrical may turn out to be more correct—but that will not alter the nature of the separating action, which depends on the fact that the repulsion varies as the surface of a particle, and therefore as the square only of its linear dimensions, while its mass varies as the cube. By reducing the dimensions we thus give the repulsion greater relative importance; halve the size of a particle and it is twice as easy to blow away; halve it again, and the facility is again doubled, and so on; and this is true whether we are concerned with light-pressure or electrical action, or the blowing of dust.

Comets thus tend to grow smaller. The losses represented by the tail are difficult to estimate quantitatively: but from recent photographs, especially those of Prof. E. E. Barnard, and those taken at the Royal Observatory, Greenwich, it has been conclusively proved that matter is travelling outwards from the head of the comet, and some progress has been made with quantitative estimations. Mr. A. S. Eddington recently gave an interesting account of such work in this Institution (Friday, March 26, 1909).

The hypothesis is not by any means new. “On this hypothesis, said Dr. Huggins in 1882, “a comet would of course suffer a large waste of material at each return to perihelion, as the nucleus would be unable to gather up again to itself the scattered matter of the tail: and this view is in accordance with the fact that no comet of short period has a tail of any considerable magnitude.” But in recent years

the evidence for the hypothesis has been much strengthened by the photographs above mentioned.

The occasion for Dr. Huggins's lecture was the acquisition of new spectroscopic evidence about the chemical composition of comets. It had been found that the spectrum was continuous, and contained Fraunhofer lines, so that the light was reflected solar light in part : but that it was crossed by some *bright* lines, indicating the presence of carbon, hydrogen and nitrogen, and probably oxygen. In most comets observed since then the bands of carbon have extended into the tail of the comet : but in 1907 Deslandres announced some new bands in the tail, and these new bands were a prominent feature, also in the tail of Morehouse's comet (c. 1908). Prof. Fowler, of the Imperial College of Science and Technology, has had the good fortune to identify the new spectrum with one observed in his laboratory, but though he can reproduce the laboratory spectrum at will, he is not yet fully clear as to its origin. His conclusion at present is "that the spectra of the tails of Daniel's and Morehouse's comets were chiefly derived from some compound of carbon, under conditions which have only been reproduced in the terrestrial gases when at pressures of about one-hundredth of a millimetre or less."* The lowness of the pressure is in accord with what has been hitherto inferred as to the physical conditions of the tail : and may help to allay the anxieties of those who dread our passage through the tail of Halley's comet on May 18. Will the spectrum of the moon on that night, viewed through a quarter of a million miles of the comet's tail, show Prof. Fowler's new spectrum ?

So far as we know, then, Halley's comet is being gradually disintegrated. At each return the sun exacts a considerable fine, and since we know of no compensating replenishment of the patrimony, it must be dwindling, and will ultimately disappear. Other evidence tells us that there is a close connexion between comets and meteors, and hence, in ages to come, the flashing of a few meteors across the sky may be all that remains to tell us of the comet which was a terror to past ages. But that end is not yet. We may confidently expect many more returns of his comet to bear witness to Halley's fame : and when we see it in May next, let us remember Halley—Halley the astronomer, who first remarked that the stars moved ; Halley the navigator, who hoped to render navigation less difficult for others ; Halley the friend and stimulator of Newton, whose labours, undertaken merely to elucidate his friend's great law of gravitation, brought unexpected fame for himself : Halley, in fine, as he would himself have us remember him, Halley "the Englishman."

[H. H. T.]

* Monthly Notices of the R.A.S., lxx., p. 182.

WEEKLY EVENING MEETING,

Friday, February 25, 1910.

SIR WILLIAM CROOKES, LL.D. D.Sc. F.R.S., Honorary Secretary
and Vice-President, in the Chair.

THE RIGHT HON. LORD RAYLEIGH, O.M. D.C.L. LL.D. D.Sc.
F.R.S. *M.R.I.*
Honorary Professor of Natural Philosophy, Royal Institution.

Colours of Sea and Sky.

A RECENT voyage round Africa recalled my attention to interesting problems connected with the colour of the sea. They are not always easy of solution in consequence of the circumstance that there are several possible sources of colour whose action would be much in the same direction. We must bear in mind that the absorption, or proper, colour of water cannot manifest itself unless the light traverse a sufficient thickness before reaching the eye. In the ocean the depth is of course adequate to develop the colour, but if the water is clear there is often nothing to send the light back to the observer. Under these circumstances the proper colour cannot be seen. The much admired dark blue of the deep sea has nothing to do with the colour of water, but is simply the blue of the sky seen by reflection. When the heavens are overcast the water looks grey and leaden: and even when the clouding is partial, the sea appears grey under the clouds, though elsewhere it may show colour. It is remarkable that a fact so easy of observation is unknown to many even of those who have written from a scientific point of view. One circumstance which may raise doubts is that the blue of the deep sea often looks purer and fuller than that of the sky. I think the explanation is that we are apt to make comparison with that part of the sky which lies near the horizon, whereas the best blue comes from near the zenith. In fact, when the water is smooth and the angle of observation such as to reflect the low sky, the apparent blue of the water is much deteriorated. Under these circumstances a rippling due to wind greatly enhances the colour by reflecting light from higher up. Seen from the deck of a steamer, those parts of the waves which slope towards the observer show the best colour for a like reason.

The real colour of ocean water may often be seen when there are breakers. Light, perhaps directly from the sun, may then traverse the crest of the waves and afterwards reach the observer. In my experience such light shows decidedly green. Again, over the screw

of the ship a good deal of air is entangled and carried down, thus providing the necessary reflection from under the surface. Here also the colour is green.

The only places where I have seen the sea look blue in a manner not explicable by reflection of the sky were Aden and Suez. Although the sky was not absolutely overcast, it seemed that part at any rate of the copious, if not very deep, blue was to be attributed to the water. This requires not only that the proper colour of the water should here be blue, but also the presence of suspended matter capable of returning the light, unless indeed the sea bottom itself could serve the purpose.

The famous grotto at Capri gives an unusually good opportunity of seeing the true colour of the water. Doubtless a great part of the effect is due to the eye being shielded from external glare and so better capable of appreciating the comparatively feeble light which has traversed considerable thicknesses of water. The question was successfully discussed many years ago by Melloni, who remarks that the beauty of the colour varies a good deal with the weather. The light which can penetrate comes from the sky and not directly from the sun. When the day is clear, the blueness of the sky co-operates with the blueness of the water.

That light reflected from the surface of a liquid does not exhibit the absorption colour is exemplified by brown peaty water such as is often met with in Scotland. The sky seen by reflection is as blue as if the water were pure. But an attempt to illustrate this fact by experiment upon quite a small scale was not at first successful. A large white photographic dish containing dark brown oxidized "pyro" was exposed upon the lawn during a fine day. Although the reflected light certainly came from the clear sky, the colour did not appear pronounced, partly in consequence of the glare of the sunshine from the edges of the dish. The substitution of a dish of glass effected an improvement. But it was only when the eye was protected from extraneous light by the hands, or more perfectly by the interposition of a pasteboard tube held close up, that the blue of the reflected light manifested its proper purity. It would seem that the explanation is to be sought in diffusion of light within the lens of the eye, in consequence of which, especially in elderly persons, the whole field is liable to be suffused with any strong light finding access.

As regards the proper colour of pure water, an early opinion is that of Davy, who, in his "*Salmonia*," pronounces in favour of blue, basing his conclusion upon observations of snow and glacier streams. The latter, indeed, are often turbid, but deposit the ground-up rock which they contain when opportunity offers, as in the lake of Geneva. A like conclusion was later put forward by Bunsen on the basis of laboratory observations. The most elaborate experiments are those of Spring, who, in a series of papers published during many years,

discusses the difficult questions involved. He tried columns of great length—up to 26 metres ; but even when the distance traversed was only 4 or 5 metres, he finds the colour a fine blue only to be compared with the purest sky-blue as seen from a great elevation. But when the tubes contain ordinary water, even ordinary distilled water, the colour is green, or yellow-green, and not blue.

The conversion of the original blue into green is, of course, explicable if there be the slightest contamination with colouring matter of a yellow character—i.e. strongly absorbent of blue light. Spring shows that this is the effect of minute traces—down to one ten-millionth part—of iron in the ferric state, or of humus. The greenness of many natural waters is thus easily understood. Another question examined by Spring is not without bearing upon our present subject—viz. the presence of suspended matter. I am the better able to appreciate the work of Spring, that many years ago I tried a variety of methods, including distillation *in vacuo*, in order to obtain water in the condition which Tyndall described as “optically empty,” but I met with no success. Spring has shown that the desired result may be obtained by the formation within the body of the liquid of a gelatinous precipitate of alumina or oxide of iron, by which the fine particles of suspended matter are ultimately carried down.

Perhaps the most telling observations upon the colour of water are those of Count Aufsess, who measured the actual transmission of light belonging to various parts of the spectrum. The principal absorption is in the red and yellow. In the case of the purest water, there was practically no absorption above the line F, and a high degree of transparency in this region was attained even by some natural waters. That these waters should show blue, *when in sufficient thickness*, is a necessary consequence.

In my own experiments, made before I was acquainted with the work of Aufsess, the light traversed two glass tubes of an aggregate length of about 4 metres (12 feet). On occasion the light was reflected back so as to traverse this length twice over. I must confess that I have never seen a blue answering to Spring's description, when the original light was white. For final tests I was always careful to employ the light of a completely overcast day, which was reflected into the tubes by a small mirror. The colour, after transmission, showed itself very sensitive to the character of the original source. The palest clear sky of an English winter's day gave a greatly enhanced blue, while, on the other hand, isolated clouds are usually yellowish, and influence the result in the opposite direction. I should myself describe the best colour of the transmitted light on standard days as a greenish blue, but there is some variation in the use of words, and, perhaps, in vision. Some of my friends, but not the majority, spoke of blue simply, but all were agreed that the blueness of a good sky was not approached. The waters tried have been

very various. Sea-water from outside the grotto of Capri, from Suez, and from near the Seven Stones Lightship off the Cornish coast, I owe to the kindness of friends. Of these the two former showed a greenish blue, the latter a full, or, perhaps, rather yellow-green, and these colours were not appreciably modified after the water had stood in the tubes for weeks. It is important to remember that the hue may, to some extent, depend upon thickness. It is quite probable that in a greatly increased thickness the Capri and Suez waters would assume a more decided blue colour. But I do not think the Seven Stones water could so behave, the colour, with 12 feet, seeming to involve the absorption of blue light.

Further observations on greater depths of sea-water would be desirable. A naval son informs me that off the coast of Greece a plate lying in 6 fathoms of water looked decidedly blue, although the sky was a dirty grey. I have doubts whether this would be generally the case in the Mediterranean; the green due to moderate thicknesses seems too decided.

Of natural fresh waters that I have tried, none were better than that from a spring in my own garden. This water is hard, but bright and clear, and it shows a greenish-blue, barely distinguishable from that of the Capri and Suez water. Distillation does not improve the blue. Neither did other treatments do any good, such, for example, as partial precipitation of the lime with alkali, or passage of ozone with the idea of oxidising humus. Wishing to try water of high chemical purity, I obtained—through the kind offices of Sir J. Dewar—water twice distilled from alkaline permanganate, and condensed in contact with silver, but the colour was no bluer. In the light of this evidence, I can hardly avoid the conclusion that the blueness of water in lengths of 4 metres has been exaggerated, especially by Spring, although I have no reason to doubt that a fully developed blue may be obtained at much greater thicknesses. I should suppose that sufficient care has not been taken to start with white light. It may be recalled that overcast days are not so common in some parts of the world as in England.

A third possible cause of apparent blueness of the sea must also be mentioned. If a liquid is not absolutely clear, but contains in suspension very minute particles, it will disperse light of a blue character. Although, undoubtedly, this cause must operate to some extent, I have seen no reason to think that it is important. But the existence of three possible causes of blueness complicates the interpretation of the phenomena. Hitherto observers have not been sufficiently upon their guard to distinguish blueness having its origin in the sky from blueness fairly attributable to the water itself.

As regards the light from the sky, the theory which attributes it to dispersal from small particles, many of which are smaller than the wave-length of light, is now pretty generally accepted. To a first

approximation at any rate, both the polarization and the colour of the light are easily explained. According to the simplest theory, the polarization should be absolute and a maximum at 90° from the sun, and the colour should be modified from that of the sun according to the factor λ^{-1} . But it is easy to see that there must be complications, even if all the particles are small and spherical. The light illuminating them is not merely the direct light of the sun, but also light diffused from the sky and from the earth's surface. On these grounds alone the polarization must be expected to be incomplete even at 90° , and the certain presence of particles not small in comparison with the wave-length is another cause operating in the same direction. It is rather remarkable that, as I noticed in 1871, the two polarised components show much the same colour. The observation is best made with a double-image prism mounted near one end of a pasteboard tube, through which a suitable rectangular aperture at the other end is seen double, but with the two images in close juxtaposition. When this is directed to a part of the sky 90° from the sun, and the tube turned until one image is at its darkest, the two polarised components are exhibited side by side in a manner favourable for comparison of colours. The addition at the eye end of a nicol capable of rotation independently of the tube, gives the means of equalising the brightnesses without altering the colours. This observation, made independently by Spring, is regarded by him as an objection to the theory, and as showing that the cause of the blueness and of the polarization are not the same. The argument would have more weight if the colours of the two components were exactly the same and under all circumstances, but I do not think that this is the case. Observations on the purer sky, to be seen from great elevations, would be of interest. The question is to what causes the second component is principally due. So far as it depends upon sky illumination, it would be bluer than the first component. Any "residual blue" of the kind described by Tyndall, and due to particles somewhat too big for the simple theory, would make a contribution in the same direction. On the other hand, large particles under the direct light of the sun, and perhaps small ones, so far as illuminated by light from the earth, would contribute a whiter light. In this way an approximate compensation may occur, but the matter is certainly worthy of further attention.

In this connection it should be noticed that, according to the now generally received electro-magnetic theory, complete polarization at 90° requires that the dispersing particles should behave as if spherical, even although infinitely small. If the shape be elongated, there would be incomplete polarization combined with similarity of colour even under the simplest conditions.

When the particles are no longer very small in comparison with the wave-length, the direction of maximum polarization was found

by Tyndall to become oblique, and the deviation is in the opposite direction to that which would have been anticipated from the Brewsterian law for the reflection of light from surfaces of finite area. As I showed in 1881, the gradual precipitation of sulphur from a very weak and acid solution of "hypo" exhibits the phenomena remarkably well. At a certain stage, depending on the colour of the light, the direction of maximum polarization becomes oblique. Even when the obliquity is well established for blue light, red light still continues to follow the simpler law, and the comparison gives curious information concerning the rate of growth of the particles.

The preferential scattering of light of short wave-length involves of course a gradual yellowing and ultimate reddening of the light transmitted. The formation in this way of sunset colours is well illustrated by the acid hypo.

That Spring rejects this theory in favour of one which would attribute sky-blue to absorption by oxygen or ozone, has been already alluded to. Although one must not conclude too hastily from the behaviour of these bodies when liquefied, it is, of course, possible that their absorbing qualities may influence atmospheric phenomena in some degree. But to attribute the blue of the sky to them seems out of the question. It is sufficient to remark that the setting sun turns red and not blue.

An interesting question remains behind. To what kind of small particles—dispersing short waves in preference—is the heavenly azure due? That small particles of saline or other solid matter, including organic germs, play a part, cannot be doubted, and to them may be attributed much of the bluish haze by which the moderately distant landscape is often suffused. But it seems certain that the very molecules of air themselves are competent to scatter a blue light not very greatly inferior to that which we actually receive. Theory allows a connection to be established between the transparency of air for light of various wave-lengths, and its known refractivity in combination with Avagadro's constant, expressing the number of molecules per cubic centimetre in gas under standard atmospheric conditions. The first estimate of transparency was founded upon Maxwell's value of this constant, viz. 1.9×10^{19} . Recent researches have shown that this number must be raised to 2.76×10^{19} , and that the result is probably accurate to within a few per cent.* It has been pointed out by Dr. Schuster that the introduction of the raised number into the formula almost exactly accounts for the degree of atmospheric transparency observed at high elevations in the United States, apparently justifying to the full the inference that the normal blue of the sky is due to molecular scattering. But, although there is no

* It is a curious instance of divergence in scientific opinion that while some still deny the existence of molecules, others have successfully counted them.

reason to anticipate that this general conclusion will be upset, it should not be overlooked that a molecule, especially a diatomic molecule, can hardly be supposed to behave as if it were the dielectric sphere of theory. Questions are here suggested for whose decision the time is perhaps not yet ripe.

[R.]

P.S.—The question of the colour of the Mediterranean and other waters was long ago discussed by Mr. J. Aitken—an excellent observer—in *Proc. Roy. Soc. Edin.* 1881–82. His principal conclusions are very similar to my own. Mr. Aitken rightly insists upon the influence of the colour of the suspended matter to which the return of the light is due. Only when this is white, has the proper colour of the water a full chance of manifesting itself. From the heights of Capri, I noticed that the shallow water near the shore showed decidedly green, an effect attributed to the yellowness of the underlying sand.

WEEKLY EVENING MEETING,

Friday, March 4, 1910.

SIR JAMES CRICHTON BROWNE, M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

CHARLES CHREE, Esq., M.A. Sc.D. LL.D. F.R.S., Superintendent
Observatory Department, National Physical Laboratory.

Magnetic Storms.

§ 1. IN the case of terrestrial magnetism, the ordinary scientific procedure of passing from the more known to the less known has been by no means universally followed. I shall, however, observe it to-night, and shall preface my description of the complicated phenomena exhibited in magnetic storms by a brief account of the comparatively regular phenomena which are of daily occurrence.

The magnetic needle has been described with poetic licence as "true to the pole," and few, I suspect, are aware how little it deserves this reputation. The earliest known information on this point in England dates from 1580, when Boroughs, observing at Limehouse, found the needle to point $11\frac{1}{4}^{\circ}$ to the east of geographical north. During the next $2\frac{1}{2}$ centuries it kept moving to the west, reaching its extreme position of $24\frac{1}{2}^{\circ}$ to west of north in 1818. It has since retraced its path, and now at Kew points only a little more than 16° to west of north. The declination, or angle made with the true north, is at present diminishing about one degree in 10 years, so the poet's words may be true of England before the year 2100.

§ 2. Besides this slow secular change, there are daily changes, which are continuously recorded at a number of observatories. The method of obtaining a continuous record of the direction of the compass needle is very simple. A magnet suspended by a fine fibre carries a mirror, which reflects a beam of light. As the magnet turns the mirror moves with it, and the reflected light, by a well-known optical principle, moves through twice the angle described by the mirror. The reflected light falls on photographic paper at a fixed distance, and the movement of the spot of light on the paper is proportional to the angle through which the magnet and mirror have moved. From a fixed mirror, adjacent to the movable one, light is also reflected on to the paper, which is wound on a drum turned by clockwork at a uniform rate. When the paper is unwound and developed, it shows a straight line trace due to the light from the

fixed mirror, and a curved line, the distance of any point of which from the straight line represents the corresponding angle between the two mirrors. The light from the fixed mirror is interrupted for a few minutes every hour or second hour, and the gaps thus produced in the base line serve to mark the time.

When a magnet, instead of having its motion limited like the compass needle to the horizontal plane, is so supported at its centre as to be perfectly free to turn, it dips, making with the horizon an angle which varies from 0° in the magnetic equator to 90° at the magnetic poles. In London at present the dip is about 67°. The dipping needle must coincide in direction with the resultant magnetic force on either pole. This force has thus in general both a horizontal and a vertical component, and the intensity of each is continually varying. At a complete magnetic observatory there are three magnetographs in constant action, recording respectively declination, horizontal force, and vertical force. In the Kew pattern instrument each magnetograph has a separate drum and a separate sheet of paper, but the three drums are driven by a single clock. To economise paper, two days' traces are usually taken on the same sheet, the position of the light being shifted at the beginning of the second day to avoid superposition of the traces. At Kew, the hour of changing papers is shortly after 10 a.m. [Slide.]

In some foreign types of magnetograph, e.g. the Eschenhagen, which was used in the National Antarctic Expedition of 1901-4, the three elements are recorded on one sheet of paper, but only one day's record is taken on each sheet. [Slide.]

§ 3. In my subsequent remarks I am obliged to employ a term which has unfortunately more than one meaning. It will be simplest to explain these meanings by reference to the daily variations of temperature which are familiar to everyone. Suppose that in the present month of March we record the temperature at every hour and take a mean value for each hour of the 24 from all days of the month. We shall then find a regular progressive rise from a minimum, probably at 6 a.m., to a maximum probably at 3 p.m., and then a gradual fall to the minimum. The difference between this maximum and minimum is known as the *range* of the regular diurnal inequality for the month. On individual days, however, the hours at which the highest and lowest temperatures occur will vary, and if we take the mean of the differences between the highest and lowest temperatures of each individual day, irrespective of the hours at which they occur, we get a totally distinct range which I shall call the mean *absolute* range.

It is obvious on reflection that the absolute range cannot be less and must usually be considerably greater than the range of the regular diurnal inequality. At Kew, for instance, taking the four years 1892-5, the mean absolute range of temperature for the year as a whole was 13°·9 F., while the range of the regular diurnal

inequality was only $10^{\circ} \cdot 0$ F. Similarly, the mean absolute daily range of declination at Kew derived from the 11 years 1890 to 1900 was $13' \cdot 6$, while the corresponding range of the regular diurnal inequality was only $8' \cdot 0$. Analogous results hold for the other magnetic elements.

§ 4. The range of the regular diurnal inequality varies with the season of the year. Table I. shows its amplitude in the case of the declination at Kew ($51^{\circ} 28'$ North lat.), Batavia ($6^{\circ} 11'$ S.) and the 'Discovery's' winter quarters ($77^{\circ} 51'$ S.).

TABLE I.—RANGE OF REGULAR DIURNAL INEQUALITY (DECLINATION).

Station.	Latitude.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
Kew	$51^{\circ} 28' \text{ N}$	4'·9	6'·1	9'·1	10'·9	10'·7	10'·9	10'·6	11'·0	9'·5	7'·7	5'·4	4'·5	8'·0
Batavia.	$6 \ 11 \text{ S}$	4'·2	4'·6	3'·6	2'·9	2'·4	2'·0	2'·3	3'·2	3'·8	4'·5	4'·5	4'·2	3'·0
Antarctic	$77 \ 51 \text{ S}$	58'·6	63'·6	47'·4	35'·4	27'·3	28'·1	29'·2	35'·8	52'·6	45'·5	60'·1	85'·2	45'·5

Remembering that in the Southern hemisphere June represents midwinter, it will be seen that the range is in all cases larger in summer than in winter. So far as is known, this is true all over the earth.

It will be noticed that the range in the Antarctic is much larger than that at Kew, which in its turn much exceeds that at Batavia. Allowance must, however, be made for the fact that the disturbing force required to displace the needle $1'$ out of the magnetic meridian is proportional to the intensity of the horizontal component H of the local magnetic force. Now the values of H in C.G.S. measure, at the epochs to which the data refer, were $0 \cdot 183$ at Kew, $0 \cdot 367$ at Batavia, and only $0 \cdot 065$ at the Antarctic station. Thus the disturbing force required to produce a range of $1'$ at Batavia would produce a range of $2'$ at Kew and of nearly $6'$ at the 'Discovery's' winter quarters. But even allowing for this, the Antarctic range is much the largest of the three. The large size of the range is of course not peculiar to the 'Discovery's' winter quarters. The same phenomenon has been observed at all Arctic stations.

§ 5. The great increase apparent in the amplitude of the regular diurnal changes as we pass from temperate to Arctic or Antarctic latitudes is even more conspicuous in the case of the irregular movements, which when sufficiently pronounced are known as magnetic storms. This is illustrated by Table II.

TABLE II.—ABSOLUTE RANGES OF DECLINATION.

At Kew from 11 years.				Antarctic (77° 51' S.) from 12 years.			
Percentage of Days when Range				Percentage of Days when Range			
0'-10'	10'-30'	20'-40'	over 40'	0'-30'	30'-60'	60'-120'	over 120'
31	57	11	1	7	22	32	39

As already explained, the forces required to displace the needle 1' out of the magnetic meridian at Kew and 3' out of the magnetic meridian at the Antarctic station are approximately equal. If then the disturbing forces at the two places were of similar magnitude, we should expect ranges under 30' in the Antarctic to be as common as ranges under 10' at Kew, and on the other hand ranges over 40' at Kew to be as common as ranges over 120' in the Antarctic. This, it will be seen, is exceedingly wide of the mark, ranges over 120' being 39 times as common in the Antarctic as ranges over 40' at Kew. A single year's records in such latitudes at the 'Discovery's' winter quarters is likely to supply as many large disturbances as the records of a generation in the south of England. This is one reason why so much importance attaches to trustworthy observations with self-recording magnetographs during scientific expeditions to high latitudes.

§ 6. The daily amplitude of irregular magnetic changes, like that of the regular diurnal inequality, is variable throughout the year, but the seasonal variation is usually different in the two cases. This is shown by Table III.

TABLE III.—ANNUAL VARIATION IN INEQUALITY AND ABSOLUTE DECLINATION RANGES AT KEW, OMITTING HIGHLY DISTURBED DAYS (1890-1900).

	Winter.	Equinox.	Summer.
Inequality range	5'·25	9'·30	10'·80
Absolute range	10'·35	13'·81	13'·56
(Absolute range ÷ Inequality range)	1·94	1·49	1·25

Each of the three seasons contains 4 months, March, April, September and October being included under "Equinox."

The diurnal inequality range, which practically depends only on the regular changes, is distinctly largest in summer. But the absolute range, which depends greatly on the irregular changes, is largest in the equinox, and its winter value is both relatively and absolutely very

much larger than the winter value of the inequality range. If the days of large disturbance, averaging 19 a year, had been included in Table III., the pre-eminence of the equinoctial value of the absolute range would have been greater. In the present case, it should be added, Kew is fairly representative of all stations in temperate latitudes.

§ 7. When we pass to days of large disturbance, the prominence of the equinoctial season in temperate latitudes becomes accentuated. This is shown by Table IV., which gives the seasonal distribution of the 721 magnetic storms recorded at Greenwich from 1848 to 1903, as calculated from the lists drawn up by Mr. W. Ellis and Mr. E. W. Maunder, with corresponding results for Batavia from 1883 to 1899, obtained by Dr. Van Bemmelen.

TABLE IV.—SEASONAL DISTRIBUTION OF MAGNETIC STORMS.

Place.	Epoch.	Percentage of all Recorded.		
		Winter.	Equinox.	Summer.
Greenwich . .	1848-1903	32	42	26
Batavia . .	1883-1899	33	35	32

Out of every 100 storms recorded at Greenwich, 42 occurred in the 4 equinoctial months: the number in summer was less than the number in winter.

The seasonal variation seems to diminish as we approach the magnetic equator, and but little remains of it at Batavia.

§ 8. When we pass to high latitudes the pre-eminence of the equinox as a season for magnetic storms seems to disappear entirely. This is shown by Table V., which compares declination results at Kew and at the 'Discovery's' winter quarters.

TABLE V.—PERCENTAGE OF DAYS HAVING RANGE OVER 20' AT KEW, AND OVER 120' AT THE 'DISCOVERY'S' WINTER QUARTERS (77° 51' S.).

Station.	Mid-Winter.	Equinox.	Midsummer.
Kew	12	16	9
77° 51' S	24	31	81

At Kew, out of every 100 days at midsummer (May to July), only 9 had an absolute range over 20', the corresponding figure for

the four equinoctial months being 16, or nearly double. But in the Antarctic 81 out of every 100 days at midsummer had a range exceeding 120', while the corresponding figure for the equinoctial months was only 31.

§ 9. The phenomena of magnetic storms appear, at least at some stations, to be largely influenced by the hour of the day. Table VI. gives some figures for Greenwich derived from the hours of beginning and ending in Mr. Maunder's lists for the years 1848 to 1903, as well as some figures which Dr. Van Bemmelen has given for Batavia.

TABLE VI.—DIURNAL VARIATION IN MAGNETIC STORMS AT GREENWICH (MR. E. W. MAUNDER) AND BATAVIA (DR. W. VAN BEMMELEN).

Station.	Local Time.	Percentage of Total Occurrences.		
		1-8 p.m.	9 p.m.-4 a.m.	5 a.m.-noon.
Greenwich . . .	Beginning . . .	60	22	18
	End . . .	9	45	46
Batavia . . .	Beginning . . .	30	25	45
	End . . .	18	55	27
	Maximum intensity	33	43	24

At Greenwich, no less than 60 per cent. of the storms commenced during the eight hours 1 to 8 p.m., while only 9 per cent. then ended.

§ 10. There is yet another influence on magnetic changes, which requires to be considered. Referring some little time ago to Prof. Hale's discovery of the Zeeman effect in light from sun-spot areas, an eminent physicist is reported to have said: "Until we understand better than we do these solar processes . . . we may do well to cultivate a humbler frame of mind than that indulged in by some of our colleagues." If the scientific man associates a partiality for sun-spots with some exaltation of mind, the unscientific man for his part is apt to regard it as qualifying for admission to a lunatic asylum. As one who has devoted considerable time and attention to the subject, it is thus incumbent on me to walk warily. With this end in view, I think I cannot do better than call attention to the figures in Table VII., in the preparation of which bias is quite impossible. Maxima are in heavy type.

Prof. Wolfer, of Zurich, and his predecessor, Prof. Wolf, have made a special study of sun-spots, and their tables of sun-spot frequencies, going back to 1749, command world-wide respect. While Wolfer's figures are given in Table VII. as a measure of average sun-spot activity for each year from 1890 to 1900, it may be added that closely parallel results would be derived from the Astronomer

Royal's figures for sun-spot areas. Wolfer's figures show a well marked maximum in 1893. The preceding and succeeding minima were in 1890 and 1901 respectively. The remarkable parallelism between the progressive changes from year to year in sun-spot frequency and in the diurnal inequality ranges at Kew and Pavlovsk—representing respectively Western and Eastern Europe—appeals to the eye. The difference between the ranges at sun-spot maximum and minimum is substantial, the latter in the present case being only about two-thirds of the former.

TABLE VII.—CONNECTION BETWEEN SUNSPOT FREQUENCY AND DECLINATION RANGES.

Year . . .	1890.	1891.	1892.	1893.	1894.	1895.	1896.	1897.	1898.	1899.	1900.
Sunspot frequency (Wolfer) . . .	7.1	35.6	73.0	84.9	78.0	64.0	41.8	26.2	26.7	12.1	5.5
Diurnal inequality range—											
At Kew . . .	7.3	8.5	9.8	10.7	9.8	9.5	8.5	7.8	7.6	7.3	6.8
At Pavlovsk . . .	6.3	7.3	8.7	9.6	8.6	8.2	7.4	6.8	6.3	6.0	6.2
Absolute daily range—											
At Kew . . .	10.7	13.7	17.7	15.6	16.5	15.6	14.5	12.1	12.3	11.3	9.2
At Pavlovsk . . .	12.1	16.0	21.0	17.8	20.4	18.1	17.5	14.6	14.7	13.1	10.5
At Pavlovsk—											
Mean range in month . . .	28.2	46.3	93.6	48.3	84.1	47.4	52.4	43.8	46.6	38.3	32.8
Total range in year . . .	42.1	92.3	194.0	87.1	145.6	73.9	88.7	101.1	118.9	63.8	94.2

Passing to the absolute daily range, we have now to deal with a quantity which is considerably influenced by magnetic storms. Here, again, the ranges in the years of many sun-spots are conspicuously the larger, but the parallelism with the variation of sun-spot frequencies is less close. In particular, 1893, the year of sun-spot maximum, shows at both Kew and Pavlovsk a distinctly smaller absolute range than either of the adjacent years, especially 1892. Of the last two lines in Table VII., the first gives the arithmetic mean of the differences observed at Pavlovsk between the extreme positions of the compass needle during each month of the year, while the second gives its total range during the year. These ranges, especially the last, are determined by the size of the principal magnetic storms. In both cases 1892 occupies the premier, and 1894 the second position. 1893 lags far behind; in the case of the annual range it even follows 1900, which had the smallest sun-spot frequency of the whole 11 years. According to Table VII. the close parallelism

visible between sun-spot frequency and the regular diurnal inequality becomes more and more obliterated as we pass from the regular to the less regular, and from these to the highly irregular daily changes of terrestrial magnetism. This phenomenon is capable of several explanations. The regular and irregular changes of the magnetic elements may be mainly due to distinct causes, and the irregular changes may have little if any dependence on the condition of the solar surface. On the other hand, sun-spots of similar area may be of widely different character. There may in short be only an occasional Napoleon among sun-spots capable of producing large magnetic disturbance on the earth; and mere areas or frequencies may possess little or no significance in this connection. A third possibility is that the magnetic condition on any one day depends on the solar condition of a considerable number of previous days.

§ 11. A general parallelism between sun-spot frequency and the range of the regular diurnal inequality, such as appears in Table VII., is far from proving any intimate connection between the two phenomena on the same day. Table VIII. gives the results of an attempt which I made to find out whether the parallelism extends to individual days' results.

TABLE VIII.

RELATION OF SUN SPOT AREA (GREENWICH) TO ABSOLUTE DECLINATION RANGE (KEW) ON SAME DAY AND ON THREE SUBSEQUENT DAYS.

—	Algebraic Excess of Declination Range Over Mean from all Days.							
	10 Days (each Month) of Largest Spot Area.				10 Days (each Month) of Least Spot Area.			
	Same Day.	1 Day After.	2 Days After.	3 Days After.	Same Day.	1 Day After.	2 Days After.	3 Days After.
1890-1900	+ 0'·17	+ 0'·25	+ 0'·48	+ 0'·53	- 0'·32	- 0'·45	- 0'·38	- 0'·35
1894	+ 1'·23	+ 1'·55	+ 1'·61	+ 1'·69	- 1'·44	- 1'·92	- 1'·62	- 1'·36
1895	- 0'·85	- 0'·22	+ 0'·06	- 0'·17	+ 1'·19	+ 1'·41	+ 1'·29	+ 0'·92

The days of each month were divided into three groups. The first group included the 10 days in which the Greenwich sun-spot areas were the largest, the third group the 10 days in which they were least. If any close parallelism existed between the solar and magnetic phenomena on the same day, we should expect the mean of the absolute declination ranges from the first group of days to be much larger than the mean for the whole month, and that from the third group to be much less. Taking all the months of the years 1890-1900, there is a difference in the direction indicated, but it is

exceedingly small. The mean range from the days made up of the 10 days a month of largest sun-spot area exceeds the mean range from all days of the 11 years by $0\cdot17$, and the mean range from the days composed of the 10 days a month of least sun-spot area falls short of the mean range from all days by $0\cdot32$. The algebraic difference, $0\cdot49$, between these two quantities represents only $3\cdot7$ per cent. of the mean daily range, while the mean sun-spot area for the one group of days was more than 4 times larger than that for the other.

To provide for the possibility that the solar influence takes one or more days to travel to the earth, mean declination ranges were formed not merely for the 10 days of largest or smallest sun-spot area, but also for the 10 days immediately following these, for the 10 days separated by 2 days and yet again for the 10 days separated by 3 days from the days constituting the sun-spot groups. The results appear in Table VIII., and are somewhat more favourable for an association between the magnetic phenomena and the solar phenomena 2 or 3 days previously, than for an association between the phenomena on the same day. The results in some individual years seem quite favourable to a considerable influence of the kind sought for, but other individual years actually associate large diurnal ranges with small sun-spot areas. The contrast between 1894 and 1895 in this respect is very striking.

§12. In the preceding discussion declination has been chiefly referred to, partly because it is the most familiar element, and partly because numerical data for it are the most numerous. In some respects, however, declination records during magnetic storms are inferior in interest to those of horizontal force. This latter element exhibits several curious phenomena, one of which is illustrated by a slide showing the traces obtained at several stations on April 5-6, 1903. After a quiet time lasting several hours, the horizontal force curves all show a sudden rise of force, taking only a few minutes to accomplish. There were corresponding sudden changes in the other magnetic elements, but they were smaller. These sudden movements ushering in magnetic disturbances are not uncommon. They can sometimes be traced all over the earth, wherever observatories exist. So far as can be judged, they occur simultaneously wherever recorded. An interesting illustration of this fact is due to Dr. Van Bemmelen. Comparing the records of 53 sudden commencements of storms at Batavia and Greenwich, and taking the mean result from his measurements of the local times shown by the curves, he found for the difference of local time at the two stations 7h. 7m. 15s., representing a difference in longitude of $106^{\circ} 48' 45''$, while the true difference deduced from ordinary astronomical observations is $106^{\circ} 49' 45''$.

The next slide shows the record of this same disturbance (April 5-6, 1903) obtained at the 'Discovery's' winter quarters. The

sudden commencement is here represented by a very large *oscillatory* movement. This oscillatory character was prominent in all the sudden commencements shown in the Antarctic curves. It represents a phase of which faint traces are sometimes seen in temperate latitudes.

§ 13. The next slide shows the Kew horizontal force record of a disturbance which began suddenly near 2 P.M. on November 13, 1894. It illustrates a second curious, but very common, phenomenon, viz. a fall in the value of the horizontal force which survives the storm. This is a good example of an ordinary disturbance in which the magnetic changes are of considerable size, but show few large oscillations.

§ 14. The next three slides refer to a disturbance of a very different type, viz., the great storm of September 25, 1909. The first shows the record obtained at the Observatory of the Jesuit Fathers at Lunkia-pang in China. The disturbance is large, but the energy apparent is small compared with that indicated by the next two slides, which show the declination and horizontal force traces at Kew. The storm there and at other European stations was remarkable for its large and rapid oscillations. The movements were so rapid that the photographic paper was not sensitive enough to show some of them clearly. Some of the record was also lost through the trace going off the sheet on both sides. The storm was of comparatively short duration, but while it lasted, it displayed an energy unrivalled at Kew during the last 20 years. The only previous Kew records comparable with it which I have seen date from 1859.

Dr. Schmidt, the leading German authority on our subject, assigns to this storm the first place of all recorded since the Potsdam Observatory came into existence some 20 years ago. Table IX. gives his estimate, on an arbitrary scale, of the intensity of the seven largest storms that have been recorded at Potsdam.

TABLE IX.—DR. AD. SCHMIDT'S ESTIMATE OF INTENSITY OF MAGNETIC STORMS.

Date of Storm.	Disturbance at Potsdam.	Date of Storm.	Disturbance at Potsdam.
September 25, 1909 .	3800	September 11, 1908 .	1520
October 31, 1903 .	2860	August 20, 1894 .	1410
February 14, 1892 .	over 1800	February 9, 1907 .	1340
July 20, 1894 .	1580		

§ 15. An old question which has received a good deal of recent attention is whether there is or is not a cyclic period approaching a month in the occurrence of magnetic storms. J. A. Broun, an early

pioneer of magnetic work in Scotland and India, believed his observations to indicate a period of about 26 days. From an elaborate study of many years' storms at Greenwich, Mr. E. W. Maunder deduced a period of 27·275 days, and Mr. Arthur Harvey independently from a study of storms at Toronto deduced the remarkably similar period of 27·246 days. The latest result of this kind is due to the eminent German magnetician already mentioned, Dr. Schmidt, who believes in a period of 29·97 days. Schmidt found evidence of this period in a number of recent storms, and he declares that it exists in the case of very large storms even when separated by many years. He found that the dates of occurrence of 5 out of the 7 largest storms recorded at Potsdam (see Table IX.) could be deduced to a high degree of accuracy from the expression

$$2410000 + 3031 \cdot 0 + n \times 29 \cdot 97,$$

which counts time in days from the commencement of the Julian era. The degree of accordance of the calculated and actual dates is shown in Table X.

TABLE X.—SCHMIDT'S STORM PERIOD FORMULA $2410000 + 3031 \cdot 0 + n \times 29 \cdot 97$.

Putting $n =$	0	1	113	153	185
Calculated date of storm,	3031·0	3061·0	6417·6	7614·4	8575·4
Actual ,, ,,	3030	3061	6419	7616	8575

The eminence of Dr. Schmidt as a magnetician, and the wide experience of Mr. Maunder in sun-spot work, alike demand our respect. But a consideration that must influence one's judgment is that if one of these two gentlemen is correct, the other must apparently be somewhat badly wrong, unless we admit *two* distinct periods.

§ 16. A slide which serves as a chronicle of magnetic history at Kew from August 20 to November 16, 1909, will illustrate some of the difficulties in the way when one attempts either to prove or disprove the existence of a period in magnetic storms.

The upper curve shows the value each day of the absolute declination range at Kew, the lower the value at each midnight of the horizontal force. In the absence of magnetic disturbances, the declination range would presumably vary only with the season, the change from day to day being insignificant, and similarly we should expect the horizontal force to have a nearly constant value at each midnight of a month. Instead of this we see incessant variations from day to day, and the number of days in which the declination range conspicuously overtops the average is considerable. During these days there is usually a distinct fall in the horizontal force, a circumstance also

indicative of magnetic disturbance. The following days were considerably disturbed, August 29, 30, September 21, 25, 30, and October 18, 19, 23, 24; while a variety of other days, *e.g.* August 31 and October 2, 8, and 9, were decidedly more disturbed than the average. If we associate August 30 and September 25 we get a 26-day period, if we associate August 29 and September 25, or September 21 and October 18, we get a 27-day period, if we associate August 31 and September 30 we get a 30-day period, and we have any number of other possible combinations left. It should also be mentioned that disturbed conditions are seldom limited to a few hours of a particular day, and often extend over two or more days. Thus there is usually a good deal that is arbitrary in the value deduced by observation for the interval between two specified storms.

One fact to which attention may be drawn is that the disturbances of September 21, 25 and 30 led to a fall in the horizontal force, from which it is doubtful whether the element had entirely recovered even by the middle of November.

§ 17. Mr. Maunder and Dr. Schmidt both associate their periods with that of the revolution of the sun relative to a point on the earth. This period exceeds the true period of the sun's rotation—which varies considerably with solar latitude—because the earth is travelling round the sun in the direction in which the sun rotates.

The view most in favour at the present time as to the source of magnetic storms is that they are due to some solar discharge, probably from sun-spot areas and of an electrical nature. We may suppose a solar discharge to traverse space like a jet of water; when it overtakes the earth a magnetic storm begins, which continues until the full width of the jet has passed over. If the solar discharge continues long enough, it may sweep over the earth during several successive revolutions of the sun, and so give rise to a series of magnetic storms at nearly equal intervals.

Theories accepting a solar origin for magnetic storms differ as to the nature of the solar discharge.

Nordmann has suggested Röntgen rays, Birkeland cathode rays, and Arrhenius negatively charged particles. On Nordmann's hypothesis the terrestrial phenomena should follow the solar in a few minutes, on Birkeland's hypothesis in a few hours, while according to Arrhenius, the interval might be two days or more.

From time to time observations have been reported favourable to the several hypotheses. Thus Dr. J. S. Lockyer* has recently associated the great storm of September 25 with "tremendous activity" observed on the previous day at South Kensington, in a flocculus associated with a sun-spot then visible. The greatest solar activity, which according to Lockyer was "quite unique in the records made with the spectroheliograph" at South Kensington, was about 10 a.m.

* R. A. S. Notices, November 1909, p. 12.

on the 24th, while the storm commenced shortly before noon on the 25th and was at its maximum about 4h. 30m. p.m. Thus the terrestrial phenomenon followed the solar after an interval of at least $25\frac{3}{4}$ hours, if not of $30\frac{1}{2}$ hours.

On the other hand, Mr. C. Michie Smith,* Director of Kodaikanal Observatory, India, saw no outstanding solar activity until September 28, when there occurred "a sudden and very violent outburst of bright gases" on and near the sunspot then visible. Simultaneously "there was a sudden and large rise in the horizontal force record" at Kodaikanal, and Mr. Michie Smith's opinion is that "there is no reasonable doubt that the solar outburst and the magnetic disturbance were directly connected with each other."

Here, apparently, are two very similar solar phenomena, but the terrestrial phenomena which are supposed to be associated with them by Dr. Lockyer and Mr. Michie Smith respectively follow, the one after more than a day, the other immediately.

Mr. Michie Smith's observation seems exactly parallel to the celebrated one made by Carrington in 1859, which has frequently been quoted as evidence of the solar origin of magnetic storms.

§ 18. The most elaborate investigation hitherto made into the supposed solar origin of magnetic storms is due to Prof. Kr. Birkeland of Christiania, who believes cathode or analogous rays to be the vehicle by which the solar disturbance is propagated to the earth. He has made numerous experiments with cathode rays in a vacuum tube, which contains a miniature earth or "terella." By means of electric currents in wires wound on the terella, a magnetic field is produced similar in type to the earth's field. The slide shows one of Prof. Birkeland's experiments, in which a luminous discharge encircles the terella at its magnetic equator. It was apparently this experiment that suggested Prof. Birkeland's explanation of a certain type of magnetic storm which he terms the "equatorial." These "equatorial" disturbances, according to Birkeland, are normally largest in the earth's equatorial regions, where they consist mainly of a change in the horizontal force, but they are also well marked in temperate latitudes. The cause postulated by Birkeland is a circular electric current in the plane of the earth's magnetic equator, at a height of several thousand miles in the atmosphere. One criticism on the experiment that does not seem to have been met, is that to make up for the small size of his "terella" compared to the earth Birkeland ought to provide it with a magnetic field 10,000 times stronger than he has yet done. This criticism, unfortunately, according to the mathematical calculations of Prof. Störmer,† a Norwegian mathematician, seems to be rather a fundamental one. It is not possible, according to Störmer's analysis, for cathode rays

* Indian Monthly Weather Review, September 1909, p. 90.

† Archives des Sciences Physiques et Naturelles. Geneva, 1907.

emanating from the sun to reach the earth's atmosphere at all, except in a narrow band round each magnetic pole. The earth's magnetic field protects it from the approach of electrified particles, these having to describe spirals round the lines of magnetic force. The larger the mass and the greater the velocity of the particle, for a given electrical charge, the nearer can it approach the earth in the equatorial plane, and the larger is the radius of the zone surrounding each magnetic pole within which the particle can actually reach the earth. The β particles of radium, from their higher velocity, have more penetrating power than ordinary cathode rays, and are in their turn eclipsed by the α rays, whose lesser velocity is more than compensated by their larger mass. According to Störmer, the greatest angular distance from a magnetic pole at which average cathode rays emanating from the sun can reach the earth is only $2^{\circ}4'$, while the corresponding angular distances for β and α rays are respectively $4^{\circ}1'$ and $12^{\circ}7'$. None of these distances is large enough to represent the band of maximum auroral frequency on the earth.

The way in which cathode rays fail to reach the earth, when emitted from the sun in a plane coincident with the earth's magnetic equator, is illustrated by diagrams of Störmer's, of which a slide is shown.

§ 19. Undeterred by these mathematical results, Birkeland assumes that a type of magnetic disturbance which he calls the "polar elementary" storm is due to cathode rays from the sun which get within a few hundred kilometres of the earth's surface at considerable distances from a magnetic pole. The paths of approach and retreat are supposed to be radial and the connecting part horizontal, as shown in the slide, which is a photograph of a drawing by Birkeland. These "polar elementary" storms were observed on a good many occasions at four temporary observatories provided with magnetographs, which Birkeland was able to set up and keep in action in Arctic regions during the winter 1902-3. The characteristics of "polar elementary" storms are their comparatively simple character and short duration, and the fact that their amplitude—unlike that of Birkeland's "equatorial" storms—is much larger in the Arctic than elsewhere. These storms have at least a general resemblance to a special type of disturbance* of which I found numerous examples in the records of the National Antarctic expedition of 1901-4.

§ 20. Birkeland found that frequently, after an "equatorial" storm had been in progress for some hours, one or a series of "polar elementary" storms intervened. He obtained copies of the curves taken at a number of magnetic observatories on the days of the disturbances recorded by his Arctic stations, and he has reproduced these with his own records in a most valuable series of plates published in his recent

* National Antarctic Expedition, 1901-1904. Magnetic Observations, p. 186.

monumental work, "The Norwegian Aurora Polaris Expedition 1902-3, vol. 1." The slide shows part of one of these plates, containing horizontal force curves for March 22, 1903. On this occasion, a "polar elementary" storm supervened after an "equatorial" storm had been in evidence for 8 hours. In the present instance the "equatorial" storm can be recognised readily from Axeloen ($77^{\circ} 41' \text{ N. lat.}$) to Christchurch ($43^{\circ} 32' \text{ S.}$), the extreme stations for which Birkeland had records, and its magnitude appears of the same order everywhere, whereas the "polar elementary" storm, though visible in most if not all of the stations, is much largest in the Arctic.

Whilst recognising to the full the devotion with which Prof. Birkeland has prosecuted his investigations into magnetic storms for over a decade of years, and while admiring his gifts of imagination and the beauty of his experiments, I have to admit that I do not find his explanations convincing. If "equatorial" storms are due, as he believes, to electric currents at great heights above the earth in the magnetic equator, the disturbing force, while approximately horizontal and in the magnetic meridian at places near the magnetic equator, should even in temperate latitudes have a considerable vertical component, and near the magnetic poles the vertical component should largely predominate. I am unable to see these phenomena in the curves of Birkeland's own plates, and I have other evidence which seems very unfavourable to his views. During the time of Birkeland's Arctic expedition the 'Discovery' was at work in the Antarctic, and the simultaneous results obtained there do not seem capable of explanation on Birkeland's hypothesis. I would specially draw attention to two oscillatory movements near the commencement of the "equatorial" storm of March 22, 1903. Though not large, they appeal to the eye in all the horizontal force curves of Birkeland's plate, including that for Kew. Now turn to the next slide, which reproduces the simultaneous Antarctic record. Here we have two oscillatory movements absolutely coincident in time, according to my own measurements, with the oscillations recorded at Kew, but whilst movements can be seen in the vertical force, these are small compared to those in the declination. This is by no means the only case in which movements ascribed by Birkeland to "equatorial" storms were synchronous with well-marked movements in the Antarctic, and the Antarctic movements in the horizontal plane were generally not less but considerably larger than the movements at stations in temperate or equatorial latitudes.

§ 21. The next slide shows a good example of Prof. Birkeland's "polar elementary" storm which occurred on February 10, 1903. The subsequent slides illustrate various types of disturbance recorded in 1902 and 1903 by the magnetographs in the charge of Mr. L. C. Bernacchi, the physicist of the National Antarctic Expedition. They include specimens of what I have called the *special type* of disturbance, of which numerous examples occurred in the midwinter months, usually

in the evening. Many of these disturbances occurred when the sun had been continuously below the Antarctic horizon for weeks. Movements simultaneous with them could be traced through the Christchurch (N.Z.), Mauritius and Bombay curves, even as far as Kew, but the amplitude in these cases diminished notably as the distance from the Antarctic increased. In the Antarctic, as the evening advanced, disturbances of a rapidly oscillating type became more prominent. These attained their maximum development some hours after midnight, about the time when Mr. Bernacchi observed the maximum frequency of aurora.

[C. C.]

GENERAL MONTHLY MEETING,

Monday, March 7, 1910.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. F.R.S., Treasurer and Vice-President, in the Chair.

Edward Bryant, Esq.

Mrs. A. P. Cazenove,

Bernard Dyer, Esq., D.Sc. F.I.C.

Miss Edith Prankerd,

Sidney Skinner, Esq.

Rev. William Robert Trench,

Ralph Vincent, Esq., M.D. B.S. M.R.C.P.

James M. Williams, Esq., F.L.S. F.R.G.S. F.R.A.S.

Arthur Mason Worthington, Esq., C.B. M.A. F.R.S. F.R.A.S.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Ernest de la Rue, Esq., for his present of a Portrait of his father, the late Warren de la Rue, Esq.

The following Lecture Arrangements were announced :—

ARTHUR HARDEN, Esq., D.Sc. Ph.D. F.R.S., Head of Biochemical Department, Lister Institute. Three Lectures on THE MODERN DEVELOPMENT OF THE PROBLEM OF ALCOHOLIC FERMENTATION. On *Tuesdays*, April 5, 12, 19.

PROFESSOR FREDERICK W. MOTT, M.D. F.R.S. F.R.C.P., Fullerian Professor of Physiology, Royal Institution, etc. Three Lectures on THE MECHANISM OF THE HUMAN VOICE. On *Tuesdays*, April 26, May 3, 10.

PROFESSOR A. E. H. LOVE, M.A. D.Sc. F.R.S., Sedleian Professor of Natural Philosophy, University of Oxford. Two Lectures on EARTH TIDES. On *Tuesdays*, May 17, 24.

CHARLES J. HOLMES, Esq., M.A., Director of the National Portrait Gallery. Two Lectures on HEREDITY IN TUDOR AND STUART PORTRAITS. On *Tuesdays*, May 31, June 7.

TOM G. LONGSTAFF, Esq., M.A. M.D. F.R.G.S. M.R.I. Three Lectures on THE HIMALAYAN REGION. On *Thursdays*, April 7, 14, 21.

WALTER MCCLINTOCK, Esq. Three Lectures on BLACKFEET INDIANS IN NORTH AMERICA. On *Thursdays*, April 28, May 5, 12.

WALTER ROSENHAIN, Esq., D.Sc., Superintendent, Metallurgy Department, National Physical Laboratory. Two Lectures on THE CONSTITUTION AND INTERNAL STRUCTURE OF ALLOYS. On *Thursdays*, May 19, 26.

MAJOR RONALD ROSS, C.B. LL.D. D.Sc. F.R.S. F.R.C.S., Nobel Laureate. Two Lectures on MALARIA. On *Thursdays*, June 2, 9.

W. W. STARMER, Esq., F.R.A.M. Three Lectures on BELLS, CARILLONS AND CHIMES. (*With Musical Illustrations.*) On *Saturdays*, April 9, 16, 23.

D. H. SCOTT, Esq., M.A. LL.D. F.R.S., President of the Linnean Society. Three Lectures on THE WORLD OF PLANTS BEFORE THE APPEARANCE OF FLOWERS. On *Saturdays*, April 30, May 7, 14.

PROFESSOR WALTER RALEIGH, M.A., Professor of English Literature, University of Oxford. Two Lectures on 1. JOHNSON WITHOUT BOSWELL; 2. JOHNSON'S LIVES OF THE POETS. On *Saturdays*, May 21, 28.

PROFESSOR J. A. FLEMING, M.A. D.Sc. F.R.S. M.R.I., Pender Professor of Electrical Engineering, University of London. Two Lectures on ELECTRIC HEATING AND PYROMETRY. (*The Tyndall Lectures.*) On *Saturdays*, June 4, 11.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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Carus, Dr. Paul (the Author)—Philosophy as a Science. 8vo. 1909.

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Journal for Feb. 1910. 8vo.

Cracovie Academie des Sciences—Bulletin, 1910, Classe des Sciences, Nos. 1A-1B.

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Douglas, James, Esq., LL.D. M.R.I. (the Editor)—Journals and Reminiscences of James Douglas, M.D. 8vo. 1910. (Privately Printed.)

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Astrophysical Journal for Jan. 1910. 8vo.

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British Homœopathic Review for March, 1910. 8vo.

Chemical News for Feb. 1910. 4to.

Editors—continued.

- Chemist and Druggist for Feb. 1910. 8vo.
 Concrete for Feb. 1910. 8vo.
 Dyer and Calico Printer for Feb. 1910. 4to.
 Electrical Contractor for Feb. 1910. 8vo.
 Electrical Engineer for Feb. 1910. 4to.
 Electrical Engineering for Feb. 1910. 4to.
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 Electrical Times for Feb. 1910. 4to.
 Electricity for Feb. 1910. 8vo.
 Engineer for Feb. 1910. fol.
 Engineer-in-Charge for Feb. 1910. 8vo.
 Engineering for Feb. 1910. fol.
 Horological Journal for Feb. 1910. 8vo.
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 Journal of the British Dental Association for Feb. 1910. 8vo.
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 Law Journal for Feb. 1910. 4to.
 London University Gazette for Feb. 1910. 4to.
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 Mois Scientifique for Jan. 1910. 8vo.
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 New Church Magazine for March, 1910. 8vo.
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 Year Book and Record, 1909. 8vo.
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Imperial Institute—Bulletin, Vol. VII. No. 4. 8vo. 1909.
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Junior Institution of Engineers—Journal, Vol. XIX. 8vo. 1909.
 Korner, Professor G., D.C.L. Hon.M.R.I.—Various Scientific Papers. 8vo and 4to.
London County Council—Gazette for Feb. 1910. 4to.
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 Record, Vol. XXIX. No. 115. 8vo. 1910.
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- Accessions to Library, 1909. 8vo.
- Royal Colonial Institute*—United Empire, Vol. I. No. 3. 8vo. 1910.
- Royal Engineers' Institute*—Journal, Vol. XI. No. 3. 8vo. 1910.
- Royal Society of Arts*—Journal for Feb. 1910. 8vo.
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- Royal Society of London*—Proceedings, A, Vol. LXXXIII. Nos. 562–563; B, Vol. LXXXII. No. 555. 8vo. 1910.
- Philosophical Transactions, A*, Vol. CCX. Nos. 461–463; *B*, Vol. CCI. No. 274. 4to. 1910.
- St. Bartholomew's Hospital*—Reports, Vol. XLV. 8vo. 1910.
- St. Petersburg, Imperial Academy of Sciences*—Bulletin, 1910, Nos. 2–3. 8vo.
- Sanitary Institute, Royal*—Journal, Vol. XXXI. No. 2. 8vo. 1910.
- Selborne Society*—Selborne Magazine for March, 1910. 8vo.
- Smith, B. Leigh, Esq., M.R.I.*—The Scottish Geographical Magazine, Vol. XXVI. No. 3. 8vo. 1910.
- Societa degli Spettroscopisti Italiani*—Memorie. Vol. XXXIX. Disp. 1. 4to. 1910.
- South African Association for the Advancement of Science*—Journal, Vol. VI. Nos. 3–4. 8vo. 1910.
- Statistical Society, Royal*—Journal, Vol. LXXIII. Part 2. 8vo. 1910.
- Stonyhurst College Observatory*—Results of Meteorological Observations, 1909. 8vo. 1910.
- Transvaal, Agricultural Department*—Journal for Jan. 1910. 8vo.
- United Service Institution, Royal*—Journal for Feb. 1910. 8vo.
- United States Department of Agriculture*—Experiment Station Record, Vol. XXII. No. 1. 8vo. 1910.
- Report of Chief of Weather Bureau, 1907–8. 4to. 1909.
- United States Patent Office*—Gazette, Vol. CLI. Nos. 1–4. 8vo. 1910.
- Verains zur Beförderung des Gewerbfleisses in Preussen*—Verhandlungen, 1910, Heft 2. 4to.
- Warsaw, Society of Sciences*—Travaux, 1909, No. 2; 1910, No. 1. 8vo. 1909–10.
- Washington, Library of Congress*—Report of the Librarian for 1908. 8vo.

WEEKLY EVENING MEETING,

Friday, March 11, 1910.

SIR WILLIAM CROOKES, LL.D. D.Sc. F.R.S. Honorary
Secretary and Vice-President, in the Chair.

H. BRERETON BAKER, Esq., M.A. D.Sc. F.R.S.

Ionisation of Gases and Chemical Change.

THE term "catalytic" was introduced by Berzelius to describe a number of chemical actions which would only take place in the presence of a third substance, which itself was apparently unchanged throughout the reaction. The first cases of such actions were investigated by Sir Humphry Davy in 1817. He showed that many mixtures of gases were caused to unite in the presence of finely divided platinum, at temperatures far below those at which union ordinarily took place. Some years afterwards Faraday investigated similar actions, and attempted to explain them by a supposed condensation of the gases on the surface of the metal.

Thirty years ago, Prof. H. B. Dixon investigated the behaviour of carbon monoxide and oxygen when they were dried as completely as possible, and he discovered that under these circumstances electric sparks caused no explosion. Some years before, Wanklyn had discovered that purified chlorine did not act on sodium, but he did not identify the impurity, now known to be a trace of water, which causes the vigorous action which takes place under ordinary circumstances.

In 1882 Cowper investigated the action of dried chlorine on several metals, and found that the removal of moisture in many cases inhibited the reaction.

In the following year, working in Prof. Dixon's laboratory at Balliol College, I found that purified carbon could be heated to redness in dried oxygen, and that sulphur and phosphorus could be distilled in the same gas without burning. In the investigations which followed, some thirty simple reactions have been tried by myself and others. It has been shown that hydrogen and chlorine can be exposed to light without explosion, ammonia and hydrogen chloride mixed without union, sulphur trioxide can be crystallised on lime, ammonium chloride and mercurous chloride give undissociated vapours, hydrogen and oxygen can be exposed to a red heat without explosion, and lastly in 1907 nitrogen trioxide was obtained as an undissociated gas for the first time by carefully drying the liquid, and evaporating into a dried atmosphere.

The amount of water necessary to carry on these chemical reactions is extremely small, certainly less than 1 mg. in 300,000 litres. There is no accepted explanation of its catalytic effect, and in the same way the catalytic power of platinum is still a mystery. In 1893, Sir J. J. Thomson* showed that if the combination of atoms in a molecule is electrical in its nature, the presence of liquid drops of water, or drops of any liquid of high specific inductive capacity, would be sufficient to cause a loosening of the tie between the atoms, and this might result in chemical combination of the partially freed atoms to form new molecules. He showed in the same paper that drying a gas very completely stopped the passage of a current of 1200 volts. In the same year I was able in the same way to prevent the passage of discharge from an induction coil, a discharge which would traverse a spark gap of three times the distance in undried gas.

Shortly after the discovery of Röntgen rays it was found that they would ionise a gas through which they passed. At the time it was thought that this ionisation was similar to that taking place in electrolysis. If this were so the rays would probably cause chemical union to take place even in a dried gas, and accordingly Prof. Dixon and I undertook some experiments on the subject which were published in a joint paper.† The results were negative, no chemical action could be detected. Since that time the ionisation of gases has been shown to be of quite a different nature. The negative ion has been shown to be a particle of the mass of about $\frac{1}{1800}$ th that of the hydrogen atom, and the positive ion is the residue. Since the ionisation of gases is different from that in electrolysis, the retention of this term is much to be deprecated. It is suggested that the term ionisation should be retained for electrolytic dissociation, and for the different process which takes place in gases under the action of Röntgen rays, etc., a new name, electromerism, should be adopted. The electron would thus be the negative electromer.

It is probable that electrolysis and true ionisation may take place in gases, as in the decomposition of steam by electric sparks of a particular length. An experiment, recently devised, seems to show that in mercury vapour, which ordinarily consists of atoms, something of the nature of ionisation without electrolysis can take place. If oxygen be admitted to the interior of a mercury lamp from which the current has just been cut off, a considerable quantity of mercuric oxide is produced, although the temperature of the lamp (about 150°) is far lower than would suffice to bring about the union of ordinary mercury vapour with oxygen.

In order to test further the question as to whether electromerism can bring about chemical change, I have investigated the action of radium bromide on very pure and dry hydrogen and oxygen. The gases were sealed up with some radium bromide contained in an

* Phil. Mag. xxxvi. 321.

† Chem. Soc. Jour. 1896.

open silica tube. The containing vessel was provided with a vacuum gauge, by means of which the combination of $\frac{1}{3000}$ th part of the gases could be easily detected. No action whatever was observed, although the substances were left in contact for two months. A further experiment showed that, as was to be expected, very dry air undergoes electromerism when subjected to the action of radium. Two more tubes were then set up, similar to the first, containing mixtures of carbon monoxide and oxygen, one very dry, and the other containing traces of moisture, and although the radium bromide was in contact with them for more than three months not the slightest contraction could be observed. In these cases therefore electromerism produces no chemical change.

There, was, however, a possibility that electromerism might bring about a chemical action in a mixture of gases which was under conditions which were nearly, but not quite, suitable for chemical action to take place. The gaseous mixtures mentioned only combine, even when moist, at a red heat. Since the experiments were done at 20°, they only show that electromerism does not produce chemical action in gases which are otherwise unable to combine.

There remained the possibility that if gases were just on the point of combining, increasing the electromerism might accelerate the rate of action. I sought for a case of simple chemical union which would proceed at a manageable temperature, and at a rate which could be measured. Of those tried, the reaction between hydrogen and nitrous oxide was found to be the most suitable. The gases used were as pure as possible, but dried only by passing through phosphorus pentoxide tubes. They were found to combine with great uniformity when heated in clean Jena glass tubes to 530°. An electric resistance furnace was used, consisting of a wide silica tube which formed the heated chamber. It is known that many substances when heated, produce electromers in a gas: lime is fairly efficient, thoria more so, and, of course, radium bromide most of all. In the first experiment two tubes of the same Jena glass, containing the hydrogen and nitrous oxide mixture, were heated side by side. One contained some lime and in order to make the conditions as similar as possible, an equal quantity of powdered Jena glass was introduced into the other. As soon as the requisite temperature was reached, the action proceeded rapidly in the tube containing lime, the rate in the first five minutes being five times the rate of combination in the tube containing only powdered glass. After fifteen minutes the second tube had caught up the first, and the rates of union were equal up to the completion of the action. With thoria the effect was still more marked, the rate increasing to twenty times the rate in the tube containing the glass. Finally about 2 mg. of radium bromide was heated in the mixture of gases. As soon as the combining temperature was reached the gases in the radium bromide tube exploded.

From these three experiments it is seen that as the amount of

electromerism was increased, there was a rapid increase in chemical action.

I have recently been able to show that if the union of carbon monoxide and oxygen takes place in a strong electric field, which has the effect of removing electromers, the chemical action is diminished. Similar experiments are in progress with the mixture of hydrogen and chlorine, combining under the influence of light.

The next experiment tried, illustrates one way in which the electromerism of a gas may bring about chemical change. Hydrogen sulphide and sulphur dioxide can be mixed at the ordinary temperature in presence of traces of moisture, but in presence of liquid water, decomposition takes place into sulphur and water. The gases were dried before mixing by calcium chloride, which leaves about 4 mg. of water vapour per litre in the gas. After mixing, a small open silica tube containing about 2 mg. of dried radium bromide was introduced. After six hours no apparent change had taken place in the gas: there was no deposit of sulphur on the sides of the jar, and it seemed at first as if no action had been produced. On opening the jar, however, an inrush of air was noticed, and the contents were almost odourless. On heating the radium tube a large quantity of water was driven off, and a copious sublimate of sulphur was seen. The whole of the gaseous contents of the jar had condensed in the small tube containing the radium bromide. The explanation of this action of radium bromide is probably simple. Water vapour condenses on the electromers emitted, liquid drops are formed, and in them the chemical action takes place.*

Prof. Townsend has recently published an account of some experiments, in which he has shown that there is a very marked decrease in the mobility of negative electromers in the presence of an amount of water vapour represented by a pressure of $\frac{1}{50}$ th mm. The air, in his experiments, was subjected to the action of Röntgen rays.

It is concluded that water in a form approaching to that of a drop, is condensed on the electron even when a very small quantity is present. If this deposition of water molecules on electromers goes on when the amount of water present is still smaller, the theory of Sir J. J. Thomson affords a satisfactory explanation of the influence of moisture on chemical change, since some electromers are always present in ordinary gases.

[H. B. B.]

* I have invariably noticed that water collects in tubes containing radium preparations exposed to undried air. The salts are not at all deliquescent, the crystals appearing quite sharp-edged under the microscope. I found that 10 mg. of radium bromide exposed to an atmosphere saturated at 0° for two days caused a deposition of water on its surface weighing 1·5 mg.

WEEKLY EVENING MEETING.

Friday, March 18, 1910.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. D.Sc. F.R.S.,
Treasurer and Vice-President, in the Chair.

PROFESSOR SIR J. J. THOMSON, M.A. LL.D. D.Sc. F.R.S. *M.R.I.*,
Professor of Natural Philosophy, Royal Institution.

The Dynamics of a Golf Ball.

THERE are so many dynamical problems connected with golf that a discussion of the whole of them would occupy far more time than is at my disposal this evening. I shall not attempt to deal with the many important questions which arise when we consider the impact of the club with the ball, but confine myself to the consideration of the flight of the ball after it has left the club. This problem is in any case a very interesting one, it would be even more interesting if we could accept the explanations of the behaviour of the ball given by many contributors to the very voluminous literature which has collected round the game; if these were correct, I should have to bring before you this evening a new dynamics, and announce that matter when made up into golf balls obeys laws of an entirely different character from those governing its action when in any other condition.

If we could send off the ball from the club, as we might from a catapult, without spin, its behaviour would be regular, but uninteresting; in the absence of wind its path would keep in a vertical plane, it would not deviate either to the right or to the left, and would fall to the ground after a comparatively short carry.

But a golf ball when it leaves the club is only in rare cases devoid of spin, and it is spin which gives the interest, variety, and vivacity to the flight of the ball. It is spin which accounts for the behaviour of a sliced or pulled ball, it is spin which makes the ball soar or "dunk," or execute those wild flourishes which give the impression that the ball is endowed with an artistic temperament, and performs these eccentricities as an acrobat might throw in an extra somersault or two for the fun of the thing. This view, however, gives an entirely wrong impression of the temperament of a golf ball, which is in reality the most prosaic of things, knowing while in the air only one rule of conduct, which it obeys with unintelligent conscientiousness, that of always following its nose. This rule is the key to the behaviour of all balls when in the air, whether they are golf balls, base balls, cricket balls, or tennis balls. Let us, before entering into

the reason for this rule, trace out some of its consequences. By the nose of the ball we mean the point on the ball furthest in front. Thus if, as in Fig. 1, C the centre of the ball is moving horizontally to the right, A will be the nose of the ball; if it is moving horizon-

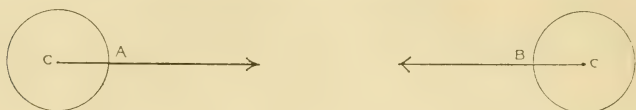


FIG. 1.

tally to the left, B will be the nose. If it is moving in an inclined direction CP, as in Fig. 2, then A will be the nose.

Now let the ball have a spin on it about a horizontal axis, and suppose the ball is travelling horizontally as in Fig. 3, and that the

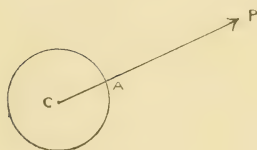


FIG. 2.

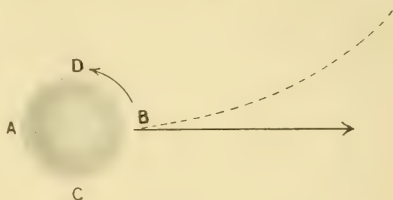


FIG. 3.

direction of the spin is as in the figure, then the nose A of the ball is moving upwards, and since by our rule the ball tries to follow its nose, the ball will rise and the path of the ball will be curved as in the dotted line. If the spin on the ball, still about a horizontal axis,

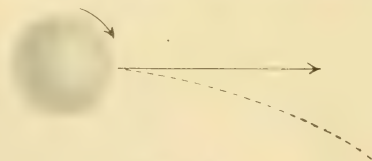


FIG. 4.

were in the opposite direction as in Fig. 4, then the nose A of the ball would be moving downwards, and as the ball tries to follow its nose it will duck downwards, and its path will be like the dotted line in Fig. 4.

Let us now suppose that the ball is spinning about a vertical axis, then if the spin is as in Fig. 5, as we look along the direction of the

flight of the ball the nose is moving to the right; hence by our rule the ball will move off to the right, and its path will resemble the dotted line in Fig. 5, in fact, the ball will behave like a sliced ball. Such a ball, as a matter of fact, has spin of this kind about a vertical axis.

If the ball spins about a vertical axis in the opposite direction as in Fig. 6, then, looking along the line of flight, the nose is moving to the left, hence the ball moves off to the left, describing the path

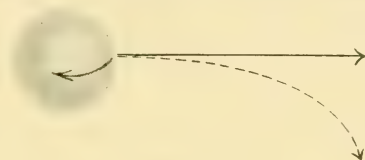


FIG. 5.

indicated by the dotted line; this is the spin possessed by a "pulled" ball.

If the ball were spinning about an axis along the line of flight, the axis of spin would pass through the nose of the ball, and the spin would not affect the motion of the nose; the ball following its nose would thus move on without deviation.

Thus, if a cricket ball were spinning about an axis parallel to the line joining the wickets, it would not swerve in the air, it would, however, break in one way or the other after striking the ground; if, on the other hand, the ball were spinning about a vertical axis, it

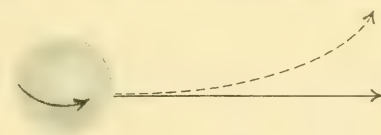


FIG. 6.

would swerve while in the air, but would not break on hitting the ground. If the ball were spinning about an axis intermediate between these directions it would both swerve and break.

Excellent examples of the effect of spin on the flight of a ball in the air are afforded in the game of base ball; an expert pitcher by putting on the appropriate spins can make the ball curve either to the right or to the left, upwards or downwards; for the sideway curves the spin must be about a vertical axis, for the upward or downward ones about a horizontal axis.

A lawn-tennis player avails himself of the effect of spin when he puts "top spin" on his drives, i.e., hits the ball on the top so as to make it spin about a horizontal axis, the nose of the ball travelling

downwards as in Fig. 4; this makes the ball fall more quickly than it otherwise would, and thus tends to prevent it going out of the court.

Before proceeding to the explanation of this effect of spin I will show some experiments which illustrate the point we are considering. As the forces acting on the ball depend on the *relative* motion of the ball and the air, they will not be altered by superposing the same velocity on the air and the ball; thus, suppose the ball is rushing forward through the air with the velocity V , the forces will be the same if we superpose on both air and ball a velocity equal and opposite to that of the ball; the effect of this is to reduce the centre of the ball to rest, but to make the air rush past the ball as a wind moving with the velocity V . Thus, the forces are the same when the ball is moving and the air at rest, or when the ball is at rest and the air moving. In lecture experiments it is not convenient to have the ball flying about the room, it is much more convenient to keep the ball still and make the air move.

The first experiment I shall try is one made by Magnus in 1852; its object is to show that a rotating body moving relatively to the air

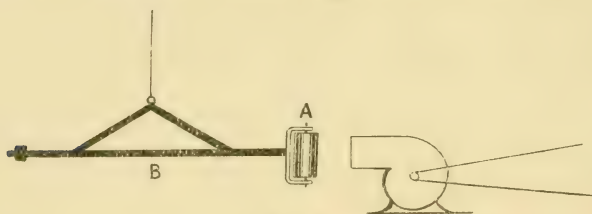


FIG. 7.

is acted on by a force in the direction in which the nose of the body is moving relatively to its centre: the direction of this force is thus at right angles, both to the direction in which the centre of the body is moving, and also to the axis about which the body is spinning. For this purpose a cylinder A (Fig. 7) is mounted on bearings so that it can be spun rapidly about a vertical axis: the cylinder is attached to one end of the beam B, which is weighted at the other end, so that when the beam is suspended by a wire it takes up a horizontal position. The beam yields readily to any horizontal force, so that if the cylinder is acted on by such a force, this will be indicated by the motion of the beam. In front of the cylinder there is a pipe D, through which a rotating fan driven by an electric motor sends a blast of air which can be directed against the cylinder. I adjust the beam and the beam carrying the cylinder, so that the blast of air strikes the cylinder symmetrically; in this case, when the cylinder is not rotating the impact against it of the stream of air does not give rise to any motion of the beam. I now spin the cylinder, and you see that when the blast strikes against it the beam moves off sideways. It goes off

one way when the spin is in one direction, and in the opposite way when the direction of spin is reversed. The beam, as you will see, rotates in the same direction as the cylinder, which an inspection of Fig. 8 will show you is just what it would do if the cylinder were acted upon by a force in the direction in which its nose (which, in this case, is



FIG. 8.

the point on the cylinder first struck by the blast) is moving. If I stop the blast, the beam does not move even though I spin the cylinder, nor does it move when the blast is in action if the rotation of the cylinder is stopped; thus both spin of the cylinder and movement of it through the air are required to develop the force on the cylinder.



FIG. 9.

Another way of showing the existence of this force is to take a pendulum whose bob is a cylinder, or some other symmetrical body, mounted so that it can be set in rapid rotation about a vertical axis. When the bob of the pendulum is not spinning the pendulum keeps swinging in one plane, but when the bob is set spinning the plane in which the pendulum swings no longer remains stationary, but rotates slowly in the same sense as the bob is spinning (Fig. 9).

We shall now pass on to the consideration of how these forces arise. They arise because when a rotating body is moving through the air the pressure of the air on one side of the body is not the same as that on the other : the pressures on the two sides do not balance, and thus the body is pushed away from the side where the pressure is greatest.

Thus, when a golf ball is moving through the air, spinning in the direction shown in Fig. 10, the pressure on the side A B C, where



FIG. 10.

the velocity due to the spin conspires with that of translation, is greater than that on the side A D B, where the velocity due to the spin is in the opposite direction to that due to the translatory motion of the ball through the air.

I will now try to show you an experiment which proves that this is the case, and also that the difference between the pressure on the two sides of the golf ball depends upon the roughness of the ball.

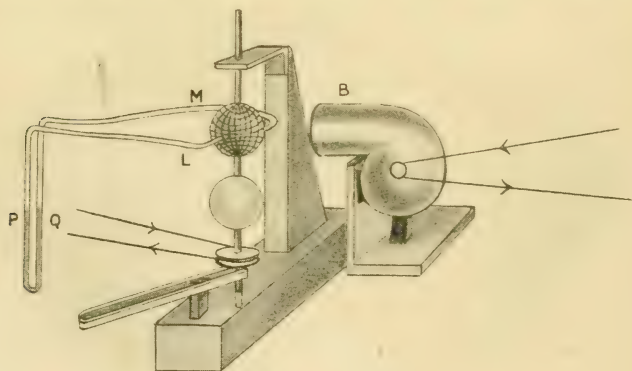


FIG. 11.

In this instrument, Fig. 11, two golf-balls, one smooth and the other having the ordinary branble markings, are mounted on an axis, and can be set in rapid rotation by an electric motor. An air-blast produced by a fan comes through the pipe B, and can be directed against the balls; the instrument is provided with an arrangement by which the supports of the axis carrying the balls

can be raised or lowered so as to bring either the smooth or the bramble-marked ball opposite to the blast. The pressure is measured in the following way: LM are two tubes connected with the pressure-gauge PQ ; L and M are placed so that the golf balls can just fit in between them; if the pressure of the air on the side M of the balls is greater than that of the side L the liquid on the right-hand side Q of the pressure-gauge will be depressed; if, on the other hand, the pressure at L is greater than that at M the left-hand side P of the gauge will be depressed.

I first show that when the golf balls are not rotating there is no difference in the pressure on the two sides when the blast is directed against the balls; you see there is no motion of the liquid in the gauge. Next I stop the blast and make the golf balls rotate; again there is no motion in the gauge. Now when the golf balls are spinning in the direction indicated in Fig. 11, I turn on the blast, the liquid falls on the side Q of the gauge, rises on the other side. Now I reverse the direction of rotation of the balls, and you see the motion of the liquid in the gauge is reversed, indicating that the high pressure has gone from one side to the other. You see that the pressure is higher on the side M where the spin carries this side of the ball into the blast, than on L where the spin tends to carry the ball away from the blast. If we could imagine ourselves on the golf ball, the wind would be stronger on the side M than on L , and it is on the side of the strong wind that the pressure is greatest. The case when the ball is still and the air moving from right to left is the same from the dynamical point of view as when the air is still and the ball moves from left to right; hence we see that the pressure is greatest on the side where the spin makes the velocity through the air greater than it would be without spin.

Thus, if the golf ball is moving as in Fig. 12, the spin increases the pressure on the right of the ball, and diminishes the pressure on the left.

To show the difference between the smooth ball and the rough one, I bring the smooth ball opposite the blast; you observe the difference between the levels of the liquid in the two arms of the gauge. I now move the rough ball into the place previously occupied by the smooth one, and you see that the difference of the levels is more than doubled, showing that with the same spin and speed of air blast the difference of pressure for the rough ball is more than twice that for the smooth.

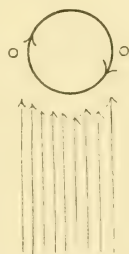


FIG. 12.

We must now go on to consider why the pressure of the air on the two sides of the rotating ball should be different. The gist of the explanation was given by Newton nearly 250 years ago. Writing to Oldenburg in 1671 about the dispersion of light, he says, in the course of his letter, "I remembered that I had often seen a tennis-

ball struck with an oblique racket describe such a curved line. For a circular as well as progressive motion being communicated to it by that stroke, its parts on that side where the motions conspire must press and beat the contiguous air more violently, and there excite a reluctancy and reaction of the air proportionately greater." This letter has more than a scientific interest—it shows that Newton set an excellent precedent to succeeding mathematicians and physicists by taking an interest in games. The same explanation was given by Magnus, and the mathematical theory of the effect is given by Lord Rayleigh in his paper on "The Irregular Flight of a Tennis-Ball," published in the 'Messenger of Mathematics,' vol. vi. p. 14, 1877. Lord Rayleigh shows that the force on the ball resulting from this pressure difference is at right angles to the direction of motion of the ball, and also to the axis of spin, and that the magnitude of the force is proportioned to the velocity of the ball multiplied by the velocity of spin, multiplied by the sine of the angle between the direction of motion of the ball and the axis of spin. The analytical investigation of the effects which a force of this type would produce on the movement of a golf ball has been discussed very fully by Professor Tait, who also made a very interesting series

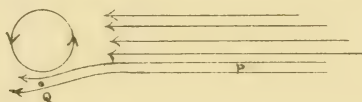


FIG. 13.

of experiments on the velocities and spin of golf balls when driven from the tee and the resistance they experience when moving through the air.

As I am afraid I cannot assume that all my hearers are expert mathematicians, I must endeavour to give a general explanation without using symbols, of how this difference of pressure is established.

Let us consider a golf-ball, Fig. 13, rotating in a current of air flowing past it. The air on the lower side of the ball will have its motion checked by the rotation of the ball, and will thus in the neighbourhood of the ball move more slowly than it would do if there were no golf ball present, or than it would do if the golf ball were there but was not spinning. Thus if we consider a stream of air flowing along the channel P Q, its velocity when near the ball at Q must be less than its velocity when it started at P; there must, then, have been pressure acting against the motion of the air as it moved from P to Q, i.e., the pressure of the air at Q must be greater than at a place like P, which is some distance from the ball. Now let us consider the other side of the ball: here the spin tends to carry the ball in the direction of the blast of air; if the velocity of the surface of the ball is greater than that of the blast, the ball will

increase the velocity of the blast on this side, and if the velocity of the ball is less than that of the blast, though it will diminish the velocity of the air, it will not do so to so great an extent as on the other side of the ball. Thus the increase in pressure of the air at the top of the ball over that at P, if it exists at all, will be less than the increase in pressure at the bottom of the ball. Thus the pressure at the bottom of the ball will be greater than that at the top, so that the ball will be acted on by a force tending to make it move upwards.

We have supposed here that the golf ball is at rest, and the air rushing past it from right to left; the forces are just the same as if the air were at rest, and the golf ball rushing through it from left to right. As in Fig. 13, such a ball rotating in the direction shown in the figure will move upwards, i.e., it will follow its nose.

It may perhaps make the explanation of this difference of pressure easier if we take a somewhat commonplace example of a similar effect. Instead of a golf ball, let us consider the case of an Atlantic liner, and, to imitate the rotation of the ball, let us suppose that the passengers are taking their morning walk on the promenade deck, all circulating round the same way. When they are on one side of the boat they have to face the wind, on the other side they have the wind at their backs. Now when they face the wind, the pressure of the wind against them is greater than if they were at rest, and this increased pressure is exerted in all directions, and so acts against the part of the ship adjacent to the deck; when they are moving with their backs to the wind, the pressure against their backs is not so great as when they were still, so the pressure acting against this side of the ship will not be so great. Thus the rotation of the passengers will increase the pressure on the side of the ship when they are facing the wind, and diminish it on the other side. This case is quite analogous to that of the golf ball.

The difference between the pressures on the two sides of the golf ball is proportional to the velocity of the ball multiplied by the velocity of spin. As the spin imparted to the ball by a club with a given loft is proportional to the velocity with which the ball leaves the club; the difference of pressure when the ball starts is proportional to the square of its initial velocity. The difference between the average pressures on the two sides of the ball need only be about one-fifth of one per cent. of the atmospheric pressure to produce a force on the ball greater than its weight. The ball leaves the club in a good drive with a velocity sufficient to produce far greater pressures than this. The consequence is that when the ball starts from the tee spinning in the direction shown in Fig. 14, this is often called underspin, the upward force due to the spin is greater than its weight, thus the resultant force is upwards, and the ball is repelled from the earth instead of being attracted to it. The consequence is that the path of the ball curves upward as in the curve A, instead of downwards as

in B, which would be its path if it had no spin. The spinning golf ball is in fact a very efficient heavier than air flying machine, the lifting force may be many times the weight of the ball.

The path of the golf ball takes very many interesting forms as the amount of spin changes. We can trace all these changes in the arrangement which I have here, and which I might call an electric golf links. With this apparatus I can subject small particles to forces of exactly the same type as those which act on a spinning golf ball.

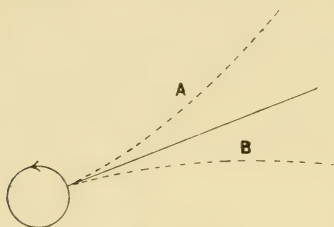


FIG. 14.

These particles start from what may be called the tee A (Fig. 15). This is a red hot piece of platinum with a spot of barium oxide upon it, the platinum is connected with an electric battery which causes negatively electrified particles to fly off the barium and travel down the glass tube in which the platinum strip is contained: nearly all the air has been exhausted from this tube. These particles are luminous, so that the path they take is very easily observed. We have now got our

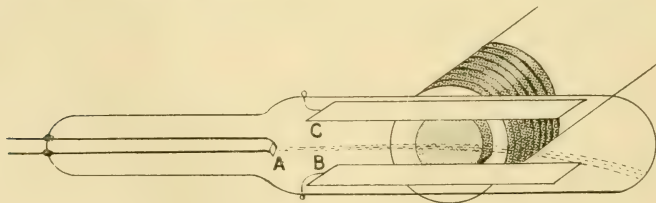


FIG. 15.

golf balls off from the tee, we must now introduce a vertical force to act upon them to correspond to the force of gravity on the golf ball. This is easily done by the horizontal plates B C, which are electrified by connecting them with an electric battery; the upper one is electrified negatively, hence when one of these particles moves between the plates it is exposed to a constant downwards force, quite analogous to the weight of the ball. You see now when the particles pass between the plates their path has the shape shown in

Fig. 16; this is the path of a ball without spin. I can imitate the effect of spin by exposing the particles while they are moving to magnetic force, for the theory of these particles shows that when a magnetic force acts upon them, it produces a mechanical force which is at right angles to the direction of motion of the particles, at right angles

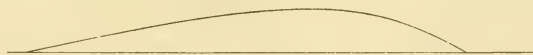


FIG. 16.

also to the magnetic force and proportional to the product of the velocity of the particles, the magnetic force and the sine of the angle between them. We have seen that the force acting on the golf ball is at right angles to the direction in which it is moving at right angles to the axis of spin, and proportional to the product of the velocity of

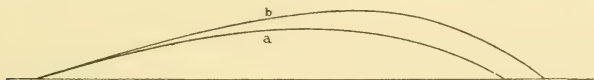


FIG. 17.

the ball, the velocity of spin and the sine of the angle between the velocity and the axis of spin. Comparing these statements you will see that the force on the particle is of the same type as that on the golf ball if the direction of the magnetic force is along the axis of spin and the magnitude of the force proportional to the velocity of spin,

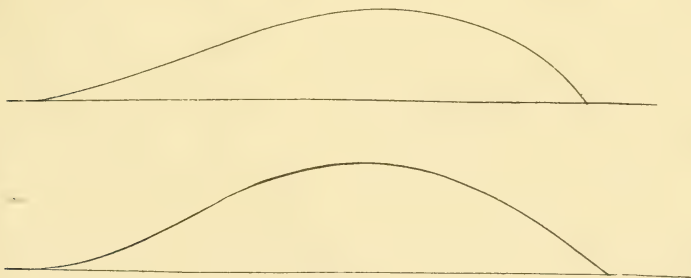


FIG. 18.

and thus if we watch the behaviour of these particles when under the magnetic force we shall get an indication of the behaviour of the spinning golf ball. Let us first consider the effect of under-spin on the flight of the ball: in this case the ball is spinning as in Fig. 3 about a horizontal axis at right angles to the direction of flight. To imitate this spin I must apply a horizontal magnetic force at right

angles to the direction of flight of the particles. I can do this by means of the electro-magnet. I will begin with a weak magnetic force, representing a small spin. You see how the path differs from the one when there was no magnetic force; the path, to begin with, is flatter though still concave, and the carry is greater than before—see Fig. 17, *a*. I now increase the strength of the magnetic field, and you will see that the carry is still further increased, Fig. 17, *b*. I increase the spin still further, and the initial path becomes convex instead of concave, with a still further increase in carry, Fig. 18. Increasing the

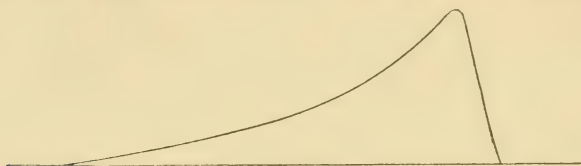


FIG. 19.

force still more, you see the particle soars to a great height, then comes suddenly down, the carry now being less than in the previous case (Fig. 19). This is still a familiar type of the path of the golf ball. I now increase the magnetic force still further, and now we get a type of flight not to my knowledge ever observed in a golf ball, but which would be produced if we could put on more spin than we are able to do at present. You see there is a kink in the curve, and at one part of the path the particle is actually travelling backwards (Fig. 20). Increasing the magnetic force I get more kinks, and we have a type



FIG. 20.

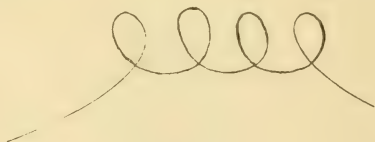


FIG. 21.

of drive which we have to leave to future generations of golfers to realise (Fig. 21).

By increasing the strength of the magnetic field I can make the curvature so great that the particles fly back behind the tee, as in Fig. 22.

So far I have been considering under-spin. Let us now illustrate slicing and pulling; in these cases the ball is spinning about a vertical axis. I must therefore move my electromagnet, and place it so that it produces a vertical magnetic force (Fig. 23). I make the force act one way, say downwards, and you see the particles curve

away to the right, behaving like a sliced ball. I reverse the direction of the force and make it act upwards, and the particles curve away to the left, just like a pulled ball.

By increasing the magnetic force we can get slices and pulls much more exuberant than even the worst we perpetrate on the links.

Though the kinks shown in Fig. 20 have never, as far as I am aware, been observed on a golf-links, it is quite easy to produce them

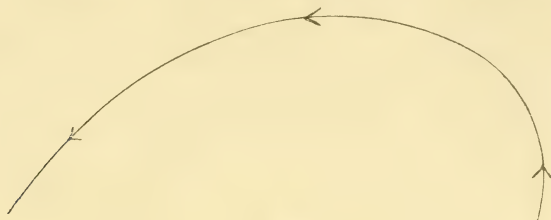


FIG. 22.

if we use very light balls. I have here a ball A made of very thin india-rubber of the kind used for toy balloons, filled with air, and weighing very little more than the air it displaces; on striking this with the hand, so as to put underspin upon it, you see that it describes a loop, as in Fig. 24.

Striking the ball so as to make it spin about a vertical axis, you

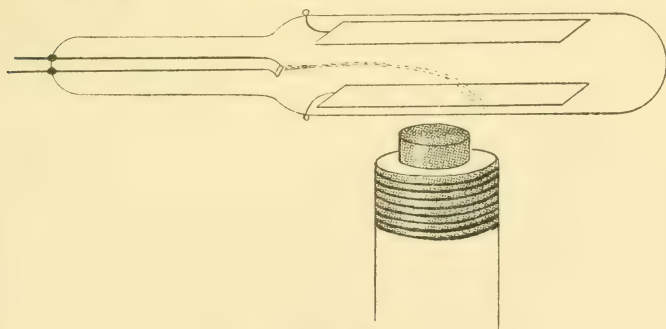


FIG. 23.

see that it moves off with a most exaggerated slice when its nose is moving to the right looking at it from the tee, and with an equally pronounced pull when its nose is moving to the left.

One very familiar property of slicing and pulling is that the curvature due to them becomes much more pronounced when the velocity of the ball has been reduced, than it was at the beginning when the velocity was greatest. We can easily understand why this

should be so if we consider the effect on the sideways motion of reducing the velocity to one-half. Suppose a ball is projected from A in the direction AB, but is sliced; let us find the sideways motion BC due to slice. The sideways force is, as we have seen, proportional to the product of the velocity of the ball and the velocity of spin, or if we keep the spin the same in the two cases, to the velocity of the ball; hence, if we halve the velocity we halve the sideways force, hence, in the same time the displacement would be halved too, but when the velocity is halved the time taken for the ball to pass from A to B is doubled. Now the displacement produced by a constant force is proportional to the square of the time: hence, if the force had remained constant, the sideways deflection BC would have been increased four times by halving the velocity, but as halving the velocity halves the force, BC is doubled when the velocity is halved: thus the sideways movement is twice as great when the velocity is halved.

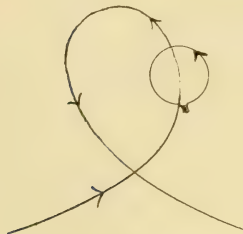


FIG. 24.



FIG. 25.

If the velocity of spin diminished as rapidly as that of translation the curvature would not increase as the velocity diminished, but the resistance of the air has more effect on the speed of the ball than on its spin, so that the speed falls the more rapidly of the two.

The general effect of wind upon the motion of a spinning ball can easily be deduced from the principles we discussed in the earlier part of the lecture. Take, first, the case of a head-wind. This wind increases the relative velocity of the ball with respect to the air; since the force due to the spin is proportional to this velocity, the wind increases this force, so that the effects due to spin are more pronounced when there is a head-wind than on a calm day. All golfers must have had only too many opportunities of noticing this. Another illustration is found in cricket: many bowlers are able to swerve when bowling against the wind who cannot do so to any considerable extent on a calm day.

Let us now consider the effect of a cross-wind. Suppose the wind is blowing from left to right, then, if the ball is pulled, it will be

rotating in the direction shown in Fig. 26: the rules we found for the effect of rotation on the difference of pressure on the two sides of a ball in a blast of air show that in this case the pressure on the front half of the ball will be greater than that on the rear half, and thus tend to stop the flight of the ball. If, however, the spin was that for a slice, the pressure on the rear half would be greater than

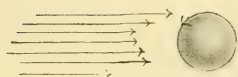


FIG. 26.

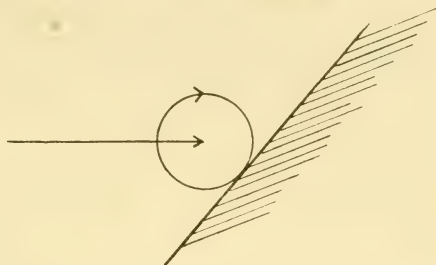


FIG. 27.

the pressure in front, so that the difference in pressure would tend to push on the ball and make it travel further than it otherwise would. The moral of this is that if the wind is coming from the left we should play up into the wind and slice the ball, while if it is coming from the right we should play up into it and pull the ball.

I have not time for more than a few words as to how the ball

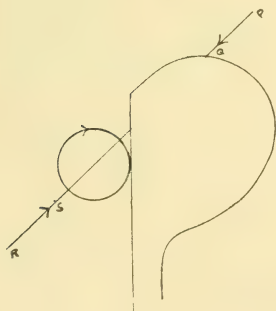


FIG. 28.



FIG. 29.

acquires the spin from the club. But if you grasp the principle that the action between the club and the ball depends only on their *relative* motion, and that it is the same whether we have the ball fixed and move the club, or have the club fixed and project the ball against it, the main features are very easily understood.

Suppose Fig. 27 represents the section of the head of a lofted club

moving horizontally forward from right to left, the effect of the impact will be the same as if the club were at rest and the ball were shot against it horizontally from left to right. Evidently, however, in this case the ball would tend to roll up the face, and would thus get spin about a horizontal axis in the direction shown in the figure; this is underspin, and produces the upward force which tends to increase the carry of the ball.

Suppose, now, the face of the club is not square to its direction of motion, but that looking down on the club its line of motion when it strikes the ball is along P Q (Fig. 28), such a motion as would be

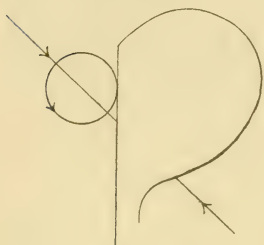


FIG. 30.



FIG. 31.

produced if the arms were pulled in at the end of the stroke, the effect of the impact now will be the same as if the club were at rest and the ball projected along R S, the ball will endeavour to roll along the face away from the striker; it will spin in the direction shown in the figure about a vertical axis. This, as we have seen, is the spin which produces a slice. The same spin would be produced if the motion of the club was along L M and the face turned so as to be in the position shown in Fig. 29, i.e., with the heel in front of the toe.

If the motion and position of the club were as in Figs. 30 and 31, instead of as in Figs. 28 and 29, the same consideration would show that the spin would be that possessed by a pulled ball.

[J. J. T.]

GENERAL MONTHLY MEETING,

Monday, April 4, 1910.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S.,
President, in the Chair.

Kenneth Alfred Wolfe Barry, Esq.

Herbert Campbell, Esq.

Robert Henry Cole, Esq., M.D.

Sigismund Goetze, Esq.

Emanuel Green, Esq.

Hugh Greenwood Hammersley, Esq.

Alexander Harvey, Esq.

Joseph Kitchin, Esq.

Sir James Reid, Bart., G.C.V.O. K.C.B. M.D.

Miss Harriet Urquhart,

Sir Francis Edward Younghusband, K.C.I.E. D.Sc. LL.D.

were elected Members of the Royal Institution.

Professor Sir James Dewar, acting on behalf of the Honorary Secretary, announced the decease of Professor Knut Johan Ångström on March 4, and of Geheimer Regierungs Rath Professor Hans Landolt on March 14, 1910, and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted :—

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution and by Science in the decease of their Honorary Member, Professor Knut Johan Ångström, Professor of Physics at the Royal University, Upsala [son of the distinguished Professor Anders Jonas Ångström, who was a former Honorary Member of the Royal Institution], celebrated for his discoveries in Spectrum Analysis, especially directed to the infra red absorption of the constituent gases of the atmosphere, in addition to the methods and determination of the constant of Solar Radiation.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with Madame Ångström and the family in their bereavement.

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution and by Science in the decease of their Honorary Member, Geheimer Regierungs Rath Professor Hans Landolt, Honorary Fellow of the Chemical Society, and Professor of Chemistry at the University of Berlin from 1891 until 1905. The relations between the molecular refraction of chemical compounds, and the refraction of the constituent atoms discovered by Professor Landolt, have become of great importance for the discrimination of the constitution of organic compounds. His work on

the polariscope has led to the construction of an instrument unsurpassed for exactness which has been introduced all over the civilised world. His manipulative skill particularly befitted him for the classical work which occupied him till shortly before his death, by which he has incontestably proved that the law of the conservation of the weight of matter is as fundamental as that of the conservation of energy.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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Astrophysical Journal for March, 1910. 8vo.

Athenæum for March, 1910. 4to.

Author for April, 1910. 8vo.

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Chemist and Druggist for March, 1910. 8vo.

Concrete for March, 1910. 8vo.

Dyer and Calico Printer for March, 1910. 4to.

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 Electrical Times for March, 1910. 4to.
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 Illuminating Engineer for March, 1910. 8vo.
 Ion for March, 1910. 8vo.
 Journal of the British Dental Association for March, 1910. 8vo.
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WEEKLY EVENING MEETING,

Friday, April 8, 1910.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L.
LL.D. F.R.S., President, in the Chair.

PROFESSOR PERCIVAL LOWELL, A.B. LL.D.,

Author of "Mars and its Canals," "Mars as the Abode of Life,"
"The Evolution of Worlds," etc.; Director of the Observatory
at Flagstaff, Arizona; Honorary Professor of Astronomy at the
Massachusetts Institute of Technology.

Lowell Observatory Photographs of the Planets.

THE pictures which I have the honour of showing you to-night represent the results of the new planetary photography originated at Flagstaff in 1903-1905, and now beginning to be successfully copied elsewhere, notably this last summer by M. le Comte de la Baume-Pluvinel and M. Baldet in France, who from the summit of the Pic du Midi de Bigorre succeeded themselves in getting imprints of the canals of Mars. Although the method was originally designed to exhibit the markings of what is practically our nearest neighbour in space, it has since been applied to the other planets with an outcome as surprising as it is satisfactory. Little details which one would not have supposed could sit still long enough for their pictures to be taken stand out unmistakably on the plates; the faint equatorial wisps of Jupiter offering a good example of such tractability, though by no means the most remarkable.

That the canals of Mars should be made to write their own signatures on a photographic plate was the occasion of the invention of the process, which, after long and patient study by my assistant, Mr. Lampland, they were finally induced to do. To his marvellous feat the best tribute was that of Schiaparelli, who, after recognising the canals on the print sent him, wrote me in wonder that photography could be made to do such work, "I would never have believed it possible." Since then further improvement has been reached to which almost every member of the staff has contributed. The process is based upon what our visual study of the planets has taught us to be the crux in the matter: the all-importance of definition. For this reason the older celestial photography which furnishes such beautiful pictures of the stars and nebulae was here impotent. This will be realised when one considers that the whole

disk of a planet could be put inside the image of a single star. For a like cause reflectors cannot be employed ; for with them all faults, instrumental or atmospheric, are magnified three-fold over those of a lens. They may give imposing-looking pictures, but the finer detail is lost ; a fact which is evident at once to an expert. Now it is in the registration of this finer detail that the accomplishment lies, and which from a scientific point of view marks its importance.

Study of the conditions leading to definition has made these photographs possible, just as lack of such study alone makes possible the scepticism one sometimes hears. Thus it is a well-known fact with us that the main markings of a disk may come out sharp, while the delicate ones are obliterated by a blur which otherwise eludes detection. This applies as much to photographic as to visual results, and it is this defect that a reflector introduces. Another optical mistake, which has latterly been hailed as showing that the lines are not lines but a series of dots, was made the other day in France. The observer saw perfectly correctly, but one with knowledge of the optics of a telescope in our air should have known that the effect observed was the inevitable result of using an aperture which the seeing did not warrant, as he could easily have assured himself by looking at the shattered rings in the synchronous image of a star. Even in our far better Flagstaff atmosphere the best results are got by diaphragming the aperture down.

In photography we cannot diaphragm down to advantage because we need the light, and this is one reason why photographs cannot rival an expert eye. Visual observations conducted by an eye fitted by nature, and trained by experience, must always surpass the best the camera can do.

What the new process does do is to monochromatise the light as nearly as possible. This is accomplished by a colour-screen, and a plate sensitised in accord with it. Then at the moment of exposure every precaution is taken that all movement shall be as nearly nil as can be secured within the instrument itself, and in the air without it. Lastly, he who would photograph the canals most successfully must first have seen them, that he may know when his opportunity arrives.

Planetary photography is not intended, nor is it destined, to supersede visual observation. Research on the planets must rest in future, as in the past, on the ultimate power of the eye and of the brain behind it, whether this take the form of telescopic, spectroscopic, or other perhaps new line of inquiry. But in certain ways the sensitive plate may supplement the retina. Position is one of these, contrast another. For the eye to place in their proper posts all the markings of a multi-featured disk in the short time at its disposal is a well-nigh impossible task. The film registers them at once in situ. Values are another thing the photographs bring out clearly. They exaggerate contrast, it is true, as compared with the eye ; but this is

no detriment. Rather the reverse, for it furnishes a greater scale for measurement.

In looking at the photographs two things must be borne in mind. One is that the irregularities due to the grain of the plate must not be attributed to the images. Thus, within the limits set by the grain the lines on Mars show as lines, not as a patchwork. This is perfectly apparent when they are carefully scanned. When we consider that the original images are only 5 mm. in diameter we realise the strain of lantern exhibition. Even so they are magnified 200 times in the taking. They are then further enlarged on the slide, and lastly thrown greatly increased upon the screen. The wonder is that they stand this limelight publicity at all.

The second point is that we are not dependent on them for our minute knowledge of the planet. A good eye trained to the subject sees at least ten times as delicately as the film. But it must be an eye suited to planetary work, which is quite a different eye from that good at faint satellite or nebula detection. It is very important to remember this, for not only is there a physiologic reason for it, but mistake of it is often made in high quarters. When an observer records a polar flattening as twice and four times what hydrodynamics permit, his forte lies elsewhere than in planetary research.

Three planets will now show you their presentments: Mars, Jupiter and Saturn. I was minded at first to omit Mars, passing by this old acquaintance with a nod, but so great have I found the interest in him here as elsewhere that he has been put beside the others.

As an example of the delicacy of the detail to be described on him, not only by the eye but in the photographs, may be instanced the sight of one of the many vicissitudes of his changeful year, which suddenly appeared one day when least expected. The event was the first frost of the season in the Antarctic regions of Mars, detected visually at Flagstaff on November 16. The patch was at once photographed, and is plainly apparent on the plate. To chronicle thus the very weather on our neighbour will convince anyone that interplanetary communication has already begun, and that, too, after the usual conventional manner of ordinary mundane greetings.

My next mention shall show you the pitch of precision to which measurements of these little prints can attain. It is well known that the south polar cap of Mars is not centred on the pole, but lies some 6° off it, in longitude 20° or thereabouts. When the images showing the cap at two different longitudes were measured the measures revealed distinctly the excentring of the cap, and even registered with some accuracy its amount and position. When we reflect what this means, it looks as if Mr. Crommelin's belief that areology would stand indebted to the photographs for help in its geodetic survey is in a fair way to be realised.

It would be possible in these photographs to take you on many a

journey to that other world, but to-night one more such interplanetary voyage must suffice. This shall give you sight of the great new canals that appeared last September in a region of the planet where no canals had ever showed before. That you may fully realise what you are to see the plate shall be accompanied by a drawing in which of course the phenomenon stands much better displayed, and a word of preface may not be out of place. To begin with, you should know that the lines you will see are certainties, not matters admitting of the slightest question for all their strange regularity, and so seen by all those who from the most prolonged and careful study are qualified to speak. Schiaparelli described them as looking as if they had been laid down with rule and compass, and not only I, but all of my assistants, have seen them thousands of times the same. Nor are they near the limit of vision in our air, which sometimes sets the planet against the sky as if etched in a steel engraving.

In the second place, the technical word "canals" does not mean canals that are dug, but artificially fertilised strips of country, connected with, and vivified by, the turning to such account of the melting of the polar cap. Lastly, I may say that by saying that organic life exists there we do not mean human beings. And now to the facts recently observed.

On September 30 last, when the region to the east of the Syrtis Major came round into view again after its periodic hiding of six weeks due to the unequal days of the Earth and Mars, two imposing canals were seen leading up from the Syrtis to the south east, which had not been there at the preceding presentation. Research showed that not only had they never previously been seen, but that they could never have existed as such before. The long and full records of the observatory extending over fifteen years made it possible to be absolutely sure of this. Yet these canals with several subsidiary ones fitted into the general canal system as if they had always made part of it.

Not only was their coming into existence established by the drawings, but the photographs of previous years testified to the same unheralded advent. Their presentments on the screen will show you this, and by comparing the drawings and photographs made at the same epoch the oneness of the two becomes evident, while the change of both with the Martian seasons is clearly portrayed.

Turning now to Jupiter, we find a completely different set of features registered on the plates, no less corroborative of the drawings made of him at Flagstaff, but utterly unlike those of Mars. Their symmetry is immediately striking, and then no less is its purely latitudinal character. They are belts, bright and dark, banding the disk halfway to the poles. Their behaviour, however, indicates in them no regard for the sun, as they are quite oblivious both to the planet's day and to his year. They last indifferently through both, and disappear at their own good time. That the brighter are clouds

and the darker the gaps between seems inferable. But they are not as our clouds. With us the heat that causes cloud comes from without; with Jupiter from within. Sun-occasioned the one, self-evolved the other. We have visual evidence of this internal heat of Jupiter in the cherry red that tinges his darker belts as if we there looked down into the seething cauldron below. We have theoretic proof of it, too, in the oblateness the disk presents taken in connection with what we know to be the planet's mean density. In two articles shortly to appear in the *Philosophical Magazine* those who care for mathematics will find that heat alone enables Jupiter to keep his youthful figure, and furthermore that his shape shows him to consist of a comparatively small kernel wrapped in a huge husk of cloud. Even those who do not care for the oldest of the sciences must admit a certain grandeur in it when theory can thus plumb depths experiment may never fathom.

These belts have another peculiarity. Their several parts are travelling at idiosyncratic rates. With them it is a go-as-you-please race, in which each outruns or falls behind its neighbour. On this interesting subject we owe most to your fellow countryman, Mr. Stanley Williams, who for some years has acted as timekeeper and referee of this Jovian family contest. In future he will have no mean rival in the photographic plate. Not that it sees as well, but that it may be measured at leisure by any investigator who likes.

There is one feature you will mark in the photographs which has had a long and eventful history. I refer to the great red spot. Detected in 1879, it lasted as such to within a few years. Rather a long life for a hole in the clouds! Now, properly speaking, we see only the grave in which it lies buried, the oval shell it once occupied. But these same photographs were in a sense the means of bringing its cradle also to light. Sixty years ago, a cycle of Cathay, Sir William Huggins made a fine series of drawings of the planet, and on receiving the present pictures was struck by the resemblance of the two. In consequence he sent me prints of his. On scanning them, my eye was caught by an oval placed as the present one lies. Clearly it was the cradle prepared already for the great red spot twenty years in advance. He had been present before its birth, as he is still, happily, present after its demise.

The third point we may mention in these photographs is their revelation of the equatorial wisps. Some years ago Mr. Scriven Bolton detected a most curious set of markings lacing Jupiter's bright equatorial belt. His discovery met with the usual approved disapprobation which has been the orthodox reception of astronomical advance since Galileo's time. Were a discovery to be hospitably hailed it would prove disconcerting to the discoverer, who would instantly suspect something wrong. Eventually the subject was referred to us for corroboration. This we were able promptly to secure. A singular phenomenon they proved to be, criss-cross fila-

ments of shading traversing the belt from triangular spots at its edges, for all the world like the lacings of a sail that hold the bolt-rope to its spar. Though perfectly evident to the eye, we hardly hoped to catch them on a plate. Nevertheless, Mr. E. C. Slipher did, and innumerable other images of them have since been got by us; their pictures you will presently see for yourselves upon the screen. Why such peculiar rents should be torn in the planet's great cloud envelop we cannot yet explain, but further news about them has still more lately come to us from the planet to which we now pass, the great ringed planet Saturn.

In some respects Saturn is the most difficult of the three planets to photograph, certainly the most tiring. So faintly is it illuminated that what takes but two seconds for Mars, takes twenty or more for Saturn. To keep the image of the planet upon its guiding cross-wires for that length of time, with the nervous knowledge that any slip will be fatal, seems an eternity. Since sensations measure existence, it may be commended as a sure though not happy way to prolong one's life.

On the resultant images may be seen abundant detail. Cassini's division is there as large as life and somewhat broader, due to the difficulty of keeping it still; so also is the shading of the inner side of ring B, and the tones of the several portions of ring A. The ball appears finely, its belts standing out even more than to the eye, and the duskiest of its polar hoods being peculiarly pronounced. The shadow of the ball upon the rings is, of course, salient, and so is the shadow of the rings upon the ball. This much is evident at a glance, but there is more to be made out by him who examines closely.

If we consider the images of November 4, which happen to be mine, we shall notice a dark band below the rings where they cross the ball, and one which is but dusky above them. Now at this date both the sun and the earth were above the plane of the rings, as we see the image, the sun the higher; the sun's relative latitude being

$$- 12^{\circ} 18'$$

that of earth

$$- 11^{\circ} 4'$$

We saw in consequence the shadow of the rings A and B underneath the rings themselves. This accounts for the dark band below. What then was the dusky band above? It could not be the shadow of these rings, for the shadow could not fall on both sides of them at once, nor could it be seen above. A little consideration will reveal to us what this band was. Inside of ring B toward the planet lies the crêpe-ring C. It is a semi-transparent ring because its particles are widely scattered, instead of seeming solid like the outer rings where the particles lie closer together. Their constitution we owe

to perhaps the greatest mind of the last century, your own Clerk-Maxwell. This, then, was the explanation: in the dusky band we were looking through the crêpe-ring on to its own shadow thrown upon the ball. Thus the crêpe-ring revealed its presence unmistakably, not by being seen, but by being seen through.

If now we compare these images of November 4 with those taken by Mr. E. C. Slipher on September 9 we note a marked contrast in the two fringes of shadow. This corroborates what we have just deduced, for at this time the relative positions of the sun and earth were reversed.

In the case of Saturn, we have as another interesting detail, the excellent instance it affords of contrast. From the bright equatorial belt, the most brilliant part of the whole picture, we notice a regular gradation of tints down to the faintest parts of the rings. For it is noteworthy that the dark belts of the planet are not so dark as these. This grading is particularly serviceable for being practically that of the eye. For the colour screen and plate used were such as to give us the light from that portion of the spectrum of which the eye takes greatest cognizance. The relative effect, therefore, on the plate is the same as on the retina.

Lastly, we come to what is one of the greatest triumphs of the whole process: the self-recording of the wisps of Saturn. It was in September that these wisps were first detected visually, independently, by my assistant, Mr. E. C. Slipher, and myself. Curiously enough they were suspected synchronously on the photographic images, and on later ones were definitely seen. Their counterpart almost precisely those of Jupiter, though of course in very faint replica. Here comes in the beauty of the photographic method. Instead of taking but a single image, twenty or more are taken one after the other on a single plate. Meanwhile the colour-screen is moved. Thus any detail in the image due to defect on the plate proclaims its origin by its singularity, and in the same manner the colour-screen betrays its self-written markings. If a detail is repeated on several images in place it must be real, however faint.

As we take our leave of Saturn let me point out the beautiful elliptical figures of the rings thus shown, a symmetrical correctness wonderfully pleasing to the eye, and which the best of drawings fails to reproduce.

From the detail these photographs have thus proved themselves able to depict, they mark a new departure in planetary research. While, on the one hand, they exhibit to the world at large something of the advance recently achieved in our knowledge of the solar system: on the other, they constitute in themselves the beginning of a set of records in which the future of the planets may be confronted with its archived past, and which shall endure after those who first conceived such registry shall have long since passed away. They can never take the place of first-rate visual observation, but they will form a firm

foundation for whatever shall subsequently be seen, and will enable such changes as must inevitably ensue to be the better collated and compared. They are the histories of the planets written by themselves, their autobiographies penned by light ; and in their grand historical portrait gallery, where the planets' pasts live on for ever in immortal youth, astronomers yet to come may see the earlier stages of the great cosmic drama which is slowly but surely working itself out.

[P. L.]

WEEKLY EVENING MEETING,

Friday, April 15, 1910.

SIR FRANCIS LAKING, Bart., G.C.V.O. M.D. LL.D.,
Vice-President, in the Chair.

PROFESSOR WILLIAM J. POPE, M.A. F.R.S.

The Chemical Significance of Crystal Structure.

LARGE numbers of chemical substances occur on the earth's surface as definite geometrical forms bounded by plane faces; these polyhedral shapes are called crystals. Inspection of the crystal forms assumed by mineral substances shows that, roughly speaking, each crystalline substance affects some specific geometrical shape which is characteristic for the material; further that, whilst crystals of any particular mineral attain vastly different dimensions and are bounded by planes which vary greatly in relative area, one geometrical feature remains constant. The angles between corresponding pairs of faces on any two crystals of the same substance are the same, notwithstanding the existence of difference in size, or in relative face magnitude between the two crystals. The constancy of interfacial angle amongst crystals of the same substance is a law of nature, and has been amply demonstrated by the very careful crystallographic measurements made by Tutton during the last 20 years.

It is, however, not essential to study mineral substances alone in order to obtain a knowledge of the laws governing crystal growth. Great numbers of laboratory products can be caused to crystallise by condensation from some fluid condition; thus, the crystals of various alums exhibited were obtained by slow evaporation of aqueous solutions of these salts.

The examination of a crystal shows that many of its physical properties differ according to the direction in the crystal in which the property is determined: the hardness of crystals, the speed at which light travels through them, and many other properties, are commonly dependent on the direction in which the material is examined.

The dependence of crystal properties on direction indicates the most essential feature of the crystal to be a definite and orderly arrangement of its ultimate particles; this arrangement is referred to as the crystal structure. Further evidence that crystals possess an arranged structure is furnished by the observation that crystallisation is not necessarily a spontaneous process. Thus, on melting benzophenone and rapidly cooling the clear molten mass, the liquid state is

retained for many hours at a temperature far below the normal melting point of the compound. But on inoculating the liquid with a trace of crystalline benzophenone crystallisation immediately commences and rapidly becomes complete. The introduction of a small particle of crystalline or arranged material into the liquid mass provides a nucleus upon which the molecules are able to deposit themselves in a similar crystalline arrangement; the process thus started quickly becomes propagated throughout the entire mass. The lack of spontaneity in the process of crystallisation leads occasionally to quite unexpected results. Thus, tetrahydroquinaldine has been known for many years, and has been prepared by numbers of chemists. It has always been obtained as a liquid, and has never been supposed capable of existing in the crystalline state at ordinary temperatures; even when cooled in liquid air it merely becomes a thick resin, and does not crystallise. But on dissolving a few drops of it in a little light petroleum and cooling the solution thus obtained in liquid air, the tetrahydroquinaldine crystallises out; on transferring a trace of the crystalline material obtained to the liquid substance at the ordinary temperature, the liquid mass is seen to immediately crystallise. This well-known substance, hitherto known only in the liquid state at ordinary temperatures, really exists in a more stable condition as a crystalline solid.

Many substances are capable of crystallising in two or more distinct crystalline forms of which one is, in general, the more stable at any particular temperature. The physical properties of the several crystalline modifications of any one substance are quite distinct and characteristic for the particular crystalline form and, in many instances, even the colours of the several modifications are different. An example of this is afforded by pouring boiling water into a beaker coated with cuprous mercuric iodide; the brilliant scarlet crystalline form stable at ordinary temperatures, when heated in this way, becomes converted into another crystalline modification which is nearly black. The change is a reversible one, and the differences between the properties of the two crystalline modifications are to be attributed to differences in the mode of arrangement of the molecules in the two cases; the two modifications, in fact, possess different crystalline structures.

Although vast numbers of observations, such as the preceding, lead to the conclusion that crystals are structured substances, it is not essential that the crystal should be a solid substance; during recent years large numbers of crystalline liquids have been discovered. On allowing melted cholesteryl chloride to cool rapidly a brilliant display of interference colours is seen owing to the particles of the substance assuming crystalline or orderly arrangement whilst still retaining the liquid condition.

Having very briefly reviewed some of the many reasons for concluding that crystals are structured edifices, the nature of the architecture which they exhibit may now be considered. All the properties of crystalline solids harmonise with one simple assumption as to the

manner in which the parts of the structure are arranged ; this assumption is that the structure is a geometrically "homogeneous" one, that is, a structure the parts of which are uniformly repeated throughout, corresponding points having a similar environment everywhere within the edifice. The assumption of geometrical homogeneity as the characteristic of crystalline solids leads at once to the great problem solved by the crystallographers of the nineteenth century. This consisted in the inquiry as to how many types of homogeneous arrangement of points in space are possible, to the study of those types and to their identification, in symmetry and other respects, with the known systems into which crystalline solids fall. This work was commenced by the German crystallographer Frankenheim in 1830, and completed by the English geometrician Barlow in 1894. Briefly stated, the final conclusion has been attained that 230 geometrically homogeneous modes exist of distributing material, or points representing material throughout space, and that these 230 homogeneous types of structure, the so-called homogeneous "point-systems," fall into the 32 types of symmetry exhibited by crystalline solids. Models of a number of homogeneous point systems illustrating some of these types are exhibited.

It is, however, obvious that the limitation of the possibilities of solid crystalline arrangement to 230 types marks but one stage in the determination of the nature of crystal structure, and throws no direct light on the relation between crystal structure and chemical constitution. Although by the end of the nineteenth century we had learnt that corresponding points of the units of crystalline structures form homogeneous point-systems, the great problem still remained of determining what are the entities which become homogeneously arranged, for what reason they become so arranged, and in what way the conclusions drawn by modern chemistry are reflected in crystal structure. This problem was a legacy to the twentieth century, and it now remains to indicate briefly the extent to which it has been solved and the results of chemical importance which have accrued during its investigation.

The problem may be most easily visualised in connection with some comparatively simple case, that, for instance, presented by the crystalline forms assumed by the elements themselves. It is generally admitted that an elementary substance consists of identical atoms, each of which acts as a centre of operation of attractive and repulsive forces. In a solid crystalline structure the atoms are obviously not free to travel through the mass, each, if not indeed fixed to a particular spot, being retained within a certain minute domain ; each of these domains must be regarded as possessing a centre which marks the mean position of the atom.

The crystalline condition of an element may consequently be defined as one of equilibrium between forces of attraction and repulsion emanating from or referable to a flock of points homogeneously

arranged in space, that is to say, of points of a homogeneous point-system. Under these conditions, the space occupied by a crystalline element, a homogeneous assemblage of identically similar atoms, may be partitioned into identically similar cells in such a manner that the boundaries of a single cell shall enclose the entire domain throughout which a particular atom exercises predominant influence. Since it is postulated that every point in the space is subject to the dominating influence of some next neighbouring atomic centre, it follows that the cells fit together so as to occupy the whole available space without interstices. Nothing is here said about the shape of

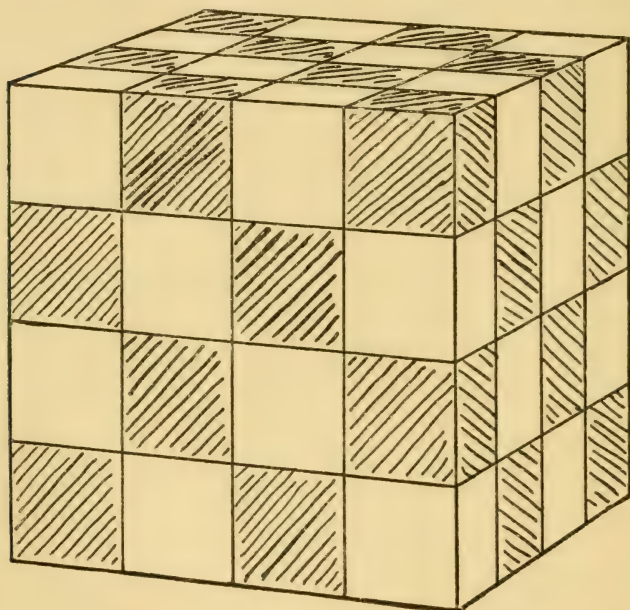


FIG.1.

the cells; but since, in the case of an elementary substance, the atomic centres are all alike, so too will be the cells.' Before proceeding to discuss the actual shapes of the cells referred to, it will be convenient to illustrate more graphically the mode of treating the problem which is here introduced with the aid of a particular point-system connected with the crystalline structure of elementary substances.

The point-system in question may be derived in the following manner. Space is first partitioned into cubes by three sets of parallel planes at right angles to one another (Fig. 1); a point is then placed at each cube corner and at the centre of each cube face.

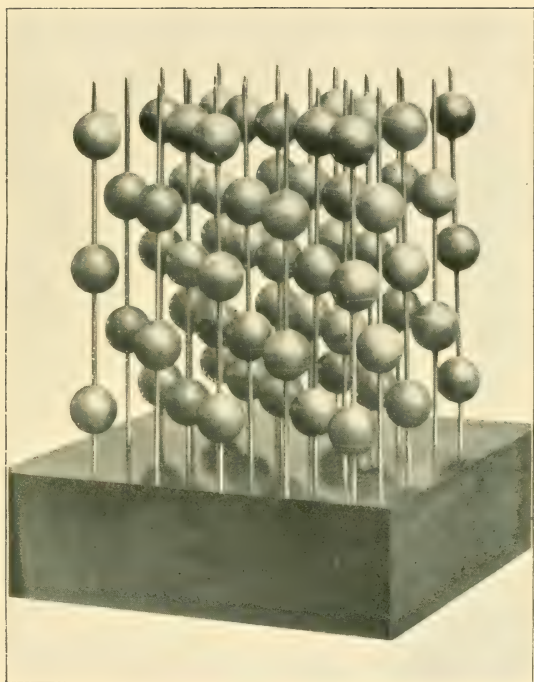


FIG. 2.

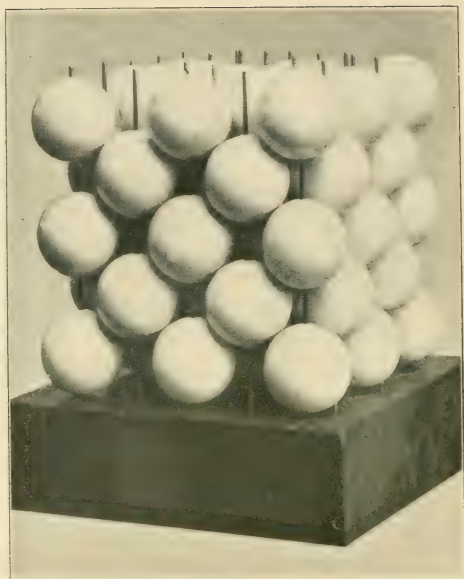


FIG. 3.



FIG. 4.

The cubes of the partitioning, having served their purpose, may now be removed, leaving one of the 230 types of homogeneous point-systems (Fig. 2). Imagine next that each point of the system expands uniformly in all directions until it touches its neighbours; a system of spheres packed together in contact is thus obtained (Fig. 3), and, on examination, it is found that no way exists of packing these equal spheres more closely together than the one thus derived. The system is therefore termed the cubic closest-packed assemblage of equal spheres and, being derived in the manner described, still retains the high symmetry of the cube; the fragment shown, in fact, outlines a cube. Three directions at right angles in it, those which are parallel to the three cube edges, are seen to be identical in kind: this identity in kind in the three rectangular directions, a , b and c , is conveniently expressed by the ratio, $a : b : c = 1 : 1 : 1$.

On removing spheres from one corner of the cubic closest-packed assemblage of equal spheres a close triangularly arranged layer is disclosed, and, by similarly treating each corner of the fragment of assemblage, the cube outline gives place to one of octahedral form. The assemblage is now seen to be built up by the superposition of the disclosed triangularly arranged layers, the hollows in one layer serving to accommodate the projecting parts of the spheres in adjacent layers. When this operation is performed it is perceived, however, that two ways of stacking the layers homogeneously are possible. The first of these, in which the fourth layer lies immediately over the first, the fifth over the second, and so on, yields the cubic closest-packed assemblage. The alternative mode of stacking, in which the third layer lies immediately over the first, the fourth over the second, and so on, exhibits the same closeness of packing as the first, but possesses the symmetry of the hexagonal crystal system; it is accordingly termed the hexagonal closest-packed assemblage of equal spheres (Fig. 4). Examination of the hexagonal assemblage shows that the horizontal directions, in the planes of the layers, are not identical in kind with vertical directions, perpendicular to the planes of the layers. Corresponding dimensions in these two directions, a and c , are in the ratio of

$$a : c = 1 : \sqrt{\frac{2}{3}} = 1 : 0.8165.$$

The final step in the treatment of the closest-packed assemblages of equal spheres consists in converting them into the corresponding assemblages of cells fitting together without interstices which have been already mentioned; it may be carried out in these, and in all other cases, by causing the component spheres to expand uniformly in all directions until expansion is checked by contact with the expanding parts of neighbouring spheres. The cubic closest-packed assemblage then becomes a stack of twelve-sided polyhedra, rhombic dodecahedra, which are so fitted together as to fill space without interstices. It is

now seen that the even rate of expansion from each point of the original point-system which gives rise to the closely packed stack of rhombic dodecahedra, symbolises an even radiation in all directions of the forces of which the atom is the centre of emanation. On applying the same operation of expansion to the spheres present in hexagonal closest-packing, each becomes converted into a dodecahedron, although of symmetry different from that of the rhombic dodecahedron. In each of the two cases the system exhibits the important property that, with a given density of distribution of the centres, a maximum distance prevails between nearest centres; these two systems thus represent the equilibrium arrangements of the postulated forces of repulsion exerted between near centres, the repulsions between more distant ones being neglected.

It will be sufficiently evident from what has been said that the function of the spherical surfaces in the closest-packed assemblages of spheres, as representing crystal structures, is merely a geometrical one; these surfaces are employed only as so much scaffolding by the aid of which may be derived arrangements exhibiting a maximum number of equal distances between neighbouring centres, and no physical distinction is to be made between portions of space lying within the spheres and portions forming part of the interstices between them. Insistence on this point is necessary, because many investigators have made use, quite illegitimately, of spheres for the representation of atomic domains, piling the spheres together in what they have termed open packing; this term seems to imply that some physical difference can subsist between the portions of space lying within the spheres and those lying without. The one kind of space is apparently regarded as susceptible to atomic influence in some sense not exhibited by the other. To state this view in any definite manner probably suffices to demonstrate its superficiality: the question of ascertaining what proportion of the total space is available for atomic occupation by the use of assemblages of spheres does not arise because the spheres used are solely the geometrical instruments for producing equality amongst the atomic distances, and so determining the prevailing equilibrium conditions.

So far as the enquiry has been carried, it would seem that the elements should crystallise either in the cubic or the hexagonal system, and that in the latter case corresponding dimensions in the horizontal and vertical directions should be in the ratio of $a : c = 1 : 0.8165$. The facts are summarised in Table I.

Of the elements which have been crystallographically examined, 50 per cent. are cubic; their crystal structure is simulated by the cubic closest-packed assemblage of equal spheres. Another 35 per cent. belong to the hexagonal system, and that these are correctly represented by the hexagonal closest-packed assemblage of equal spheres is indicated by the fact that for the hexagonal elements, the ratio of corresponding dimensions in the horizontal and vertical direc-

tions approximates to the value $a : c = 1 : 0.8165$, deduced for the model assemblage.

The task of accounting for the 15 per cent. of the crystalline elements which have been examined and found to crystallise in systems

TABLE I.—RELATION BETWEEN CRYSTAL FORM AND MOLECULAR COMPLEXITY.

Crystal System.	Ele- ments.	Number of Atoms in Molecules of Com- pound Inorganic Substances.					Organic Com- pounds
		2	3	4	5	More than 5	
Cubic	50	68.5	42	5	12	5.8	2.5
Hexagonal	35	19.5	11	35	38	14.6	4.0
Tetragonal	5	4.5	19	5	6	7	5.0
Orthorhombic	5	3.0	23.5	50	36	27.3	34.0
Monosymmetric	5	4.5	3	5	6	37.3	47.5
Anorthic	0	0	1.5	0	2	8	7.0
Number of cases summa- rised in each vertical column	40	67	63	20	50	673	585

The proportion of substances crystallising in each system is stated above as a percentage.

other than the cubic or hexagonal still remains. A little inspection shows that the crystal forms of these elements in every case approach very closely to one or other of the two of highest symmetry, namely the cubic or the hexagonal; one example of this will now suffice. The values of corresponding dimensions in three directions in space for the monosymmetric form of the element sulphur are given by the axial ratios $a : b : c = 0.9958 : 1 : 0.9998$, $\beta = 95^\circ 46'$. The slight departure of these dimensions from the corresponding values for the cubic closest-packed assemblage, in which $a : b : c = 1 : 1 : 1$, $\beta = 90^\circ$, at once suggests that the monosymmetric modification of sulphur is derived from the latter assemblage by some minute distortion. Such a distortion indicates a very trifling departure from uniformity in the influence exerted in different directions from each atomic centre, and may either arise from some want of symmetry in the individual atoms, or in a reduction of the symmetry caused by some grouping of the atoms; two or more atoms might thus be more closely connected in some way with one another than with other next neighbouring atoms.

Having shown that the crystalline forms of the elements are in complete harmony with the conception that crystal structures can be homogeneously divided into similar cells of polyhedral shapes approximating closely to the spherical, reference may now be made to some simple compounds, those, namely, in which the molecule consists of two dissimilar atoms.

The conception of the equilibrium of centred forces which has

been shown fertile in the case of the crystalline elements can be immediately applied to the binary compounds; as before, each atom will be represented by forces emanating from a centre, and equilibrium will demand closest packing of the spheres used, just as in the previous case. The atomic centres will now, however, be of two kinds, and the question arises as to whether the domains of atomic influence to be described about them will be all of the same magnitude or whether two magnitudes of spheres must be employed, one for each element present. This question is difficult to answer by reference to the facts already reviewed above; probably the only indication which the latter afford in this connection is that closest-packing of a considerable variety of different magnitudes would certainly be most unlikely to lead to the close similarity of crystal form observed as between the elements and the binary compounds. A direct answer is, however, provided as the result of investigating the crystalline forms of organic substances, to which reference will presently be made; this investigation has led to the discovery of a definite law which governs the magnitudes of the several kinds of atomic domain concerned in any crystalline compound substance. It is found that the magnitudes of the atomic domains in any crystalline compound are very approximately in the ratio indicated by the fundamental valencies of the corresponding elements. Since the molecules of nearly all the binary compounds which have been crystallographically examined contain in the molecule one atom each of two elements of the same valency, the polyhedral cells from which a crystalline binary compound must be supposed built up are all, in general, of approximately the same magnitude. The fact that most binary compounds, like most elements, crystallise in either the cubic or the hexagonal system, represents one of the simple results of this law of valency volumes.

The binary compounds thus, in general, affect crystalline structures which are derived from the cubic or the hexagonal closest-packed assemblage of equal spheres: one-half of the spheres, selected homogeneously, represent atoms of the one element and the remainder atoms of the second element. The mode in which the necessary homogeneous selection may be made in the cubic assemblage, without altering the values of corresponding dimensions in three rectangular directions, is shown in a model.

The crystalline forms of the binary compounds are in accordance with what has been above foreshadowed. Table I. indicates that in geometrical respects the crystalline binary compounds closely resemble the elements; 68.5 per cent. of those examined are cubic and 19.5 per cent. hexagonal, the remaining 12 per cent. crystallising in systems of lower symmetry than these. The axial ratios, $a : c$, of all the hexagonal binary compounds known are stated in Table II.; all approximate closely to the value, $a : c = 1 : 0.8165$, for the model hexagonal closest-packed assemblage of equal spheres.

TABLE II.—HEXAGONAL BINARY COMPOUNDS.

		$a : c$	
Beryllium oxide	BeO	1 : 0·8153	
Zinc oxide	ZnO	1 : 0·8039	
Zinc sulphide	ZnS	1 : 0·8175	
Cadmium sulphide	CdS	1 : 0·8109	
Silver iodide	AgI	1 : 0·8196	
The ratio, $1 : \sqrt{\frac{2}{3}}$		1 : 0·8165	

In connection with the elements and binary compounds it is noteworthy that the mode of treatment described appears practically to eliminate molecular aggregation of the atoms as a factor in determining the crystalline structure; that is to say, the distance separating two neighbouring atom centres is the same whether those atoms belong to the same or to different molecules. Another interesting fact is that, whilst the elements and binary compounds for the most part crystallise in the cubic or hexagonal systems, substances of greater molecular complexity rarely crystallise in these highly symmetrical systems; thus, of a great number of organic compounds examined, 2·5 and 4·0 per cent. only belong to the cubic and hexagonal crystalline systems respectively (Table I). This observation is important as one of many indications that the cells into which the crystal structure of a complex compound are partitionable are not, in general, all of the same volume. Further investigation shows that the volumes of the polyhedral cells representing the atomic domains of the several elements present in a complex crystalline compound are governed by the law of valency volumes to which reference has already been made. The correctness of this conclusion concerning the proportionality between the numbers expressing the fundamental valencies of the elements and the volumes of the corresponding spheres of atomic influence has been abundantly verified, not only by the laborious process of working out a large number of cases, but in several other ways which may be more rapidly indicated. The following are illustrations of the latter kind of verification.

Table III. states the composition and axial ratios, $a : b : c$, of a series of four crystalline minerals which differ in composition by the increment, Mg_2SiO_4 ; the sums of the valencies of the atoms composing the different molecular aggregates are stated under the heading, W . The increment, Mg_2SiO_4 , also occurs as the crystalline mineral forsterite, of which the axial ratios have been determined. It is evident that the ratio, a/b , has approximately the same value of 1·08 for all four members of the series, and that practically all differences in relative dimensions are expressed by the ratio, c/b . On dividing the valency volume, W , by the corresponding value for c/b in each case, the quotients 11·7, 12·1, 12·3, 12·4 and 12·7 are obtained respectively for the substances prolectite, chondrodite, humite, clinohumite, and forsterite. The relative dimension, c/b , is thus roughly proportional to the sum of the valencies in this set of

minerals. The comparison may, however, be made more accurately by including the changes in both relative dimensions, a/b and c/b , in the calculation in the following manner. The "equivalence parameters" are the rectangular dimensions, x , y and z , of a rectangular block having the volume W , and are in the ratio of the axial ratios, $a : b : c$. The parameters x and y preserve almost constant values throughout the series, and addition of the increment, Mg_2SiO_4 , leads to a practically constant increase of about 2.86 in the dimension z , on passing from one mineral to the next in the series. The mineral forsterite also gives nearly the same x and y values as before, and its z value, 2.87, is equal to the differences between consecutive pairs of z values in the main series; these differences vary between 2.85 and 2.88. The axial ratios and equivalence parameters of forsterite can indeed be calculated with considerable accuracy from the data available for the series of four minerals.

TABLE III.—THE HUMITE MINERALS.

Proectite	.	MgSiO_4 ,	$2\text{Mg}(\text{F},\text{OH})$.	$W = 22$
Chondrodite	.	$\text{Mg}_3(\text{SiO}_4)_2$,	$2\text{Mg}(\text{F},\text{OH})$.	$W = 38$
Humite	.	$\text{Mg}_5(\text{SiO}_4)_3$,	$2\text{Mg}(\text{F},\text{OH})$.	$W = 54$
Clinohumite	.	$\text{Mg}_7(\text{SiO}_4)_4$,	$2\text{Mg}(\text{F},\text{OH})$.	$W = 70$

The increment is Mg_2SiO_4 , namely, forsterite, with $W = 16$.

		Axial Ratios.			Equivalence Parameters.		
		a	b	c	x	y	z
Proectite	.	1.0803	1	1.8862	2.389	2.210	4.169
						Diff. =	2.851
Chondrodite	.	1.0863	1	3.1447	2.425	2.232	7.020
						Diff. =	2.877
Humite	.	1.0802	1	4.4033	2.428	2.247	9.897
						Diff. =	2.858
Clinohumite	.	1.0803	1	5.6588	2.435	2.254	12.755

Values for the increment, forsterite.

Observed	.	1.0757	1	1.2601	2.449	2.277	2.869
Calculated	.	1.0823	1	1.2775	2.429	2.245	2.867

The relations here displayed may be rendered more obvious by a series of models (Fig. 5). Rectangular blocks having as the horizontal dimensions the x and y values, and as vertical dimension the z value, for forsterite, when superposed upon a similar set of blocks having the corresponding dimensions for proectite, form a stack exhibiting the equivalence parameters of chondrodite; superposing on this a second set of forsterite blocks leads to a stack showing the equivalence parameters of humite, and on again repeating the operation, a stack with the dimensions of clinohumite results. From the numerical data and the models exhibited it must be regarded as definitely proved that, in this series, the volumes appro-



FIG. 5.

priated by the constituent atoms are, in any one member, directly proportional to the valency numbers of the corresponding elements.

Another set of observations of a very convincing character, although of a totally different kind, is laid out in Table IV.

TABLE IV.—MOLECULAR VOLUMES OF THE NORMAL PARAFFINS
AT THEIR MELTING POINTS.

—	W.	Melting Point t° .	Molecular Volumes.	
			Observed at t° .	Calculated as $W \times S$.
$C_{11}H_{24}$	68	— 26.5	201.4	201.96
$C_{12}H_{26}$	74	— 12.0	219.9	219.78
$C_{13}H_{28}$	80	— 6.2	237.3	237.60
$C_{14}H_{30}$	86	+ 4.5	255.4	255.42
$C_{15}H_{32}$	92	+ 10.0	273.2	273.24
$C_{16}H_{34}$	98	+ 18.0	291.2	291.06
$C_{17}H_{36}$	104	+ 22.5	309.0	308.88
$C_{18}H_{38}$	110	+ 28.0	326.9	326.70
$C_{19}H_{40}$	116	+ 32.0	344.7	344.52
$C_{20}H_{42}$	122	+ 36.7	362.5	362.34
$C_{21}H_{44}$	128	+ 40.4	380.3	380.16
$C_{22}H_{46}$	134	+ 44.4	398.3	398.00
$C_{23}H_{48}$	140	+ 47.7	416.2	415.80
$C_{24}H_{50}$	146	+ 51.1	434.1	433.62
$C_{27}H_{56}$	164	+ 59.5	487.4	487.08
$C_{31}H_{64}$	188	+ 68.1	558.4	558.36
$C_{32}H_{66}$	194	+ 70.0	576.2	576.18
$C_{35}H_{72}$	212	+ 74.7	629.5	629.64

Mean value of $S = 2.970$.

Experimental determinations of the molecular volumes of a long series of normal paraffins, made on the liquid substances at temperatures at which the materials are in physically similar conditions, are stated in column 4. Since the valency of carbon is four times that of hydrogen it would be anticipated from the crystallographic conclusions previously drawn, that each carbon atom should appropriate four times as large a space for occupation as one hydrogen atom; the quotient of the molecular volume by the valency sum or valency volume, W , should consequently lead to the same value, S , in the case of all the hydrocarbons. The mean value of S , namely, the atomic volume of hydrogen, is thus calculated as 2.970, and that it is constant within very narrow limits is seen on comparing columns 4 and 5, the latter of which states the product of the valency volume, W , by the value 2.970. The simple relation between the atomic volumes of carbon and hydrogen in the liquid normal paraffins indicated in the above table was recently pointed out by Lebas, and is

abundantly confirmed by numerous series of determinations in addition to that now quoted. It is thus definitely proved that the law of valency volumes, first enunciated on the ground of the crystallographic evidence, holds rigidly in the case of these liquid substances.

Sufficient has been said to demonstrate that a method has now been devised by means of which the vast stores of accurate goniometric measurements collected by crystallographers during the past century can be interpreted and that the requisite interpretation has in many cases already been given. Professor Liveing, in a discourse delivered in this room nineteen years ago, suggested that crystalline forms are the outcome of the accepted principles of mechanics; the aid of these, and of these alone, has been invoked to show that crystalline structures result from the equilibrium of the attractive and repulsive forces radiating from the atomic centres.

[W. J. P.]

WEEKLY EVENING MEETING,

Friday, April 22, 1910.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L.
LL.D. F.R.S., President, in the Chair.

T. THORNE BAKER, Esq. F.C.S. A.I.E.E.

The Telegraphy of Photographs, Wireless and by Wire.

IT frequently happens that when two alternate processes are available for certain work, and one of them is considerably less practical than the other, the less practical one is possessed of much higher scientific interest. This may certainly be said of the telegraphy of pictures and photographs. The whole of the methods of transmission can be classed as either purely mechanical, or dependent on the physical properties of some substance which, like selenium, is sensitive to light.

The latter methods are of no little scientific interest, and, although very delicate and for the moment obsolete, there is every likelihood of their coming into more extended use later on.

The telegraphy of pictures differs only from the transmission of ordinary messages in that the telegraphed signals, recorded by a marker on paper, must essentially occupy a fixed position. In the case of an ordinary telegram it matters little whether the received message occupy two, three or more lines when written out on paper, but when a picture is telegraphed every component part of it must be recorded in a definite position on the paper.

Suppose you greatly enlarge a portrait, and divide it up by ruled lines into a thousand square parts. Suppose also that the photograph is printed on celluloid, so that it is transparent. If, now, the portrait be held in front of some even source of illumination, it will be seen that each square—each thousandth part—is of different density. The light parts of the photograph will consist of squares of little density, the dark parts, of squares of greater density, and so on. In this way the photograph is analysed into composite sections, each section corresponding precisely to a letter in a message; letters and spaces recombined form words and messages; squares of different densities recombined, in correct position, form a photograph.

I propose to deal with the more practical system first, which, as already pointed out, is perhaps the less interesting from the theoretical point of view. The telegraph system has been employed by the 'Daily Mirror' for the transmission of photographs since July 1909, and has been worked very regularly between Paris and London, and Manchester and London.

Instances of its use may be recognised in the publication of photographs taken in court in the recent Steinheil case at Paris, when photographs of witnesses or prisoners were sometimes received in London actually before the court rose at which they were taken, a clear day being gained in the time of publication.

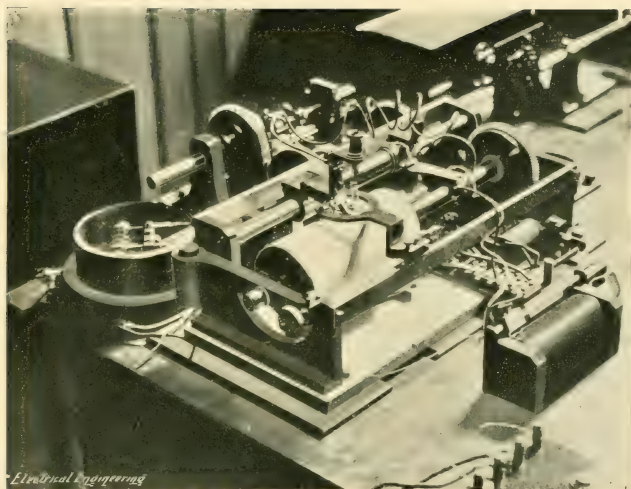
The method of telegraphing photographs that has been employed on a large scale by the 'Daily Mirror' may be called a practical modification of several early attempts. The effect of an electric current to discolour certain suitable electrolytes or to set free an element or ion that can be used to form with a second substance a coloured product was employed in many early forms of instruments for telegraphing writing, etc. If we break up a photographic image in the way already described into lines which interrupt the current for periods depending on their width, these interrupted currents can be used at the receiving station to form coloured marks which join up en masse to form a new image. My telegraphic process is thus briefly as follows :—

At the sending station we have a metal drum revolving under an iridium stylus, to the drum being attached a half-tone photograph printed on lead foil. Current flows through the photographic image to the line and thence to the receiver. The receiver consists of a similar revolving metal drum over which a platinum stylus traces. Every time the transmitter style comes in contact with a clear part of the metal foil current flows to the receiver, and a black or coloured dot or mark appears on the chemical paper. But you will readily understand that if our reproduction—built up of these little marks, which have to be made at the rate of some two hundred per second—is to be accurate, each mark must be only exactly as long, in proportion, as the clear metal space traversed by the stylus.

It will be easier to explain the system by means of the rough diagram shown in the figure. The transmitting instrument is shown on the left, the receiver on the right. A metal drum is revolved by a motor, one revolution every two seconds ; over this a metal stylus or needle traces a spiral path in the same way as a phonograph. On the drum is fixed a half-tone photograph broken up into lines, and printed in fish glue upon a sheet of lead foil. I will show one of these line photographs on the screen, and you will see that the light and shade of the picture is made up of masses of thinner or thicker lines, with clear spaces in between.

As the stylus traces over such a photograph, its contact with the metal base is interrupted every time one of these fish-glue lines comes beneath it, and for such a time as depends, of course, on the *width* of the line. The transmitting instrument thus sends into the telegraph lines a series of electric currents whose periods of duration are determined by the width of the lines composing the photograph.

A similar stylus S_2 traces an exactly similar path over a revolving drum in the receiving instrument, but round this drum is wrapped



PHOTOGRAPH SHEWING A PORTION OF THE PHOTO-TELEGRAPHIC
APPARATUS.

a piece of absorbent paper impregnated with a colourless solution, which turns black or brown when decomposed by an electric current.

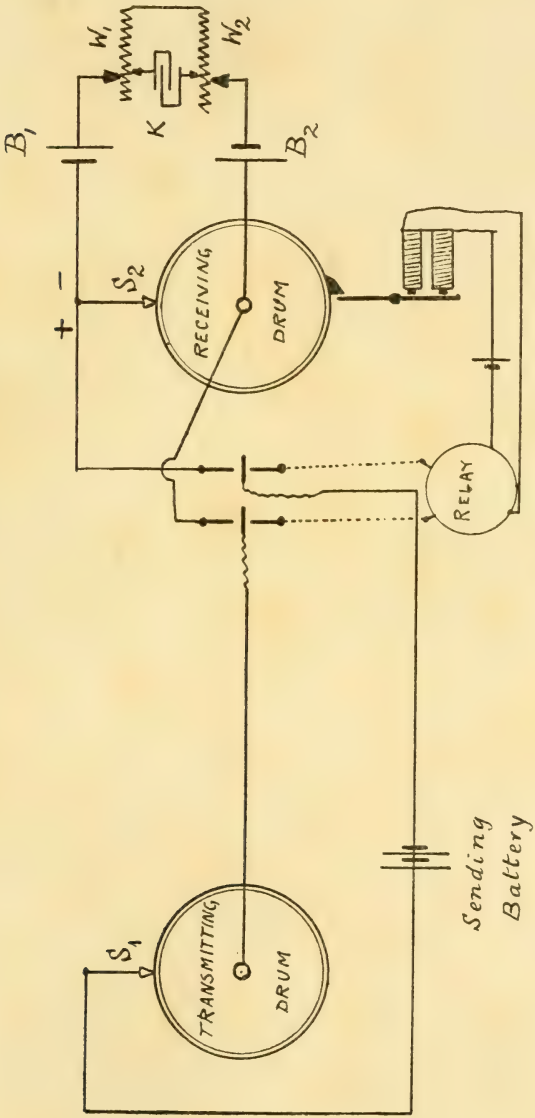


FIG. 1.

What happens, then, is that every brief current which passes through the paper causes a mark to appear on it. The width of the mark depends on the duration of the current—or should do—so that you will see that these marks gradually combine to recompose the photographic image.

This method is all very well in the laboratory, but when we come to try it over a long distance the capacity of the line at once causes serious interference. It is well known that if a current be sent to some apparatus such as a telegraph from a distance, the current having to pass through long wires whose capacity is appreciable, a certain time is taken for the current to charge the line, and the line discharges itself into the apparatus with comparative slowness. If the circuit be closed by means of a Morse key, the time of contact at the key being a sixth of a second—a common time of duration of a short tap—the discharge of current from the cable would be considerably longer than one-sixth of a second. When, therefore, we are sending signals through the line at the rate of 175 per second, it is not difficult to see that every signal will run into the next dozen or so at the receiving apparatus, and the result will be a hopelessly confused mass of overlapping marks. This is well illustrated in Fig. 2, where A shows a series of taps passed through a cable of

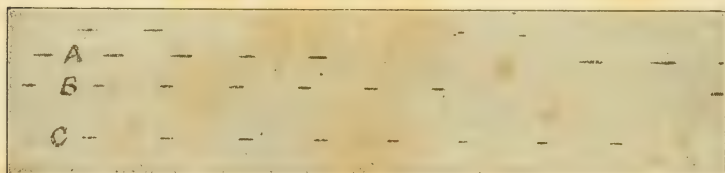


FIG. 2.

high capacity into the telegraph receiver; instead of getting a series of sharp dots or short lines, we get elongated lines ending off in tails. Without the capacity we get the short lines as shown in the B series. These short definite lines are again obtained, even when the capacity is present, in series C, but in this case I had shunted on to the receiver what I have termed the line balancer, a modified form of shunt apparatus embodying the principles of *wiping out* residuary currents from the cable in the way frequently made use of in duplex telegraphy.

The use of this apparatus has rendered commercial the old ideas of telegraphing by the electrolytic method, and as many as three hundred sharply defined chemical marks can be recorded in one second by its means. The method of application will be seen if we have the last slide shown again (Fig. 1); here shunted on to the line (which is

closed by the stylus S_2 and the metal drum) is a circuit containing two batteries B_1 and B_2 and the two sections of a divided 1000 ohms resistance, W_1 and W_2 . Shunted across the variable contacts of the resistances is a variable condenser K . By varying the resistances W_1 and W_2 we can vary the power of the current used to sweep out the residuary charges in the line; the current can, of course, flow through the chemical paper on the drum, but the pole of the battery B_1 connected to the style is of opposite sign to that of the line unit connected to it.

When the leakance on the line is great and evenly distributed, less reverse current from the balancer is necessary, this being quite in accordance with Heaviside's formulæ for telephony over lines with capacity and inductance. It is interesting to note, also, that by increasing the voltage of the reverse batteries B_1 and B_2 , considerable greater contrast can be obtained in the pictures; the finer the half-tone screen employed in splitting up the photographs into lines, the higher, again, must the voltage of B_1 and B_2 be made.

I should like to take up a few moments in referring to the actual utility of photo-telegraphy. The demand by the public for illustrations in their daily papers must be admitted. News is telegraphed in order to expedite its publication, and photographs illustrating this news can therefore be telegraphed advantageously. But where a large installation and establishment, with accumulators, a large instrument and an operator to work it are required, the cost of telegraphing every individual picture becomes quite out of proportion to its value. It is therefore that I would call especial attention to the portable instruments, the first one of which is shown for the first time to-night. A photographer going to obtain pictures of some important function or interesting event can take the machine with him, prepare his pictures and telegraph them to his head office, and when the event is over he simply returns with the apparatus. For criminal investigations the portable instrument will, I feel sure, become of considerable value also. Through the continued courtesy shown by the Postmaster-General and Major O'Meara, the Engineer-in-Chief, we have been given every facility for developing the work, and I believe that the uses of the portable instrument will before long have been amply demonstrated.

If a picture revolving beneath a tracer has to redraw itself, as it were, on a piece of paper perhaps hundreds of miles away, it is obvious that each mark redrawn must occupy a precisely similar spot on the new paper as it does in the original picture. As cylinders or drums are used in picture telegraphy, this means that they must revolve in perfect unison. If one drum were to gain on the other we should have, in the case of a portrait, a nose being recorded where the eye ought to be, or something equally disastrous; in fact, if the two machines get the least bit out of step the received picture is completely ruined. The method of synchronising used by Professor

Korn has proved very satisfactory, and has been adopted in practically all systems of photo-telegraphy. The motors which drive each drum are run at about 3000 revolutions per minute, and geared down very considerably, so that the drums themselves revolve, perhaps, at 30 revolutions per minute; the motors are run from secondary batteries of ample capacity to ensure smooth working, and should be run for a sufficient time before beginning a transmission to allow of their warming up.

The speed of each motor is controlled by a regulating resistance in series with the field magnets, and the speed is ascertained by means of a frequency meter, which indicates the number of revolutions per second. The dial of this meter is shown on the screen. A set of tuned steel tongues are fixed in front of a magnet, which is supplied with alternating current obtained from slip rings on the motor, and each tongue has a different period of vibration. When the alternations in magnetism correspond with the period of vibration of any one spring, that spring vibrates, and thus serves as an indication of the speed of the motor.

The receiving drum is revolved a little quicker than the transmitting drum. It consequently completes its revolution before the transmitter. It is then stopped by a steel check, and is obliged to wait until the other drum has caught it up. When the transmitting drum has completed *its* turn, a fleeting contact comes into play, a reverse current is sent to the receiving instrument, this is led into a polarised relay which actuates an electromagnet, and this magnet removes the check.

Thus, however much one drum gets out of step with the other, the fault is limited to each revolution, and both drums must always start off in unison for each new revolution. I have found that where each operator endeavours to keep his motor running uniformly by regulating the resistance according to the fluctuations recorded by the frequency meter, the personal element makes itself visible in the results; straight lines appear wavy, and the synchronism is not at all good. I therefore tried very carefully calibrating the motors by timing first, and then arranged that, once started, the motors should not be touched; the gain in speed of each is approximately the same if both motors are run from secondary batteries of the same ampère-hour capacity, and in this way we have obtained the most perfect results as regards synchronisation.

The great advantage of this process is that the whole operation is in full view, whereas with systems in which the received picture is obtained on a photographic film one has to develop such film before it is possible to discover whether anything is wrong. With the receiver described, the operator keeps his hand on the sliding contact of the resistances, and merely adjusts their position during the first two or three seconds according to the condition of the electrolytic marks, i.e., whether crisp and concise or not. The



FASHION PLATE TRANSMITTED BY PROFESSOR KORN'S
TELAUTOGRAPH.

PLATE III.



PHOTOGRAPH WIRED FROM PARIS TO LONDON
BY THE AUTHOR'S TELECTOGRAPH.

transmitting cylinder can be used as the receiving cylinder, and the apparatus is thus reduced to the limits of simplicity.

Towards the end of last year I designed a portable machine, two of which Mr. Sanger-Shepherd has just completed, embodying in them a number of improvements of his own, and these machines, which have worked successfully on their trials, are shown on the lecture table to-night. They are suitable for line or wireless work, and will, I believe, prove of great value in naval and military operations.

The 'Daily Mirror' inaugurated the Paris-London photographic service in November 1907 with Professor Korn's selenium instruments, which I shall briefly describe, as Korn is now making two new selenium apparatus with a view to transmitting photographs from New York to London. In this system use is made of the fact that the electrical resistance of the metal selenium varies according to the strength of illumination to which it is subjected, a beam of light passed through the light and dark parts of a photograph in succession being used to vary the strength of an electric current sent to the receiving apparatus.

In Korn's selenium transmitter light is concentrated from a Nernst lamp to pass through a revolving glass cylinder, round which a transparent photograph (printed on celluloid) is fixed, the beam traversing the film at its brightest part, where the rays come to a focus. The light which passes through the picture is reflected by a prism inside the cylinder on to the selenium cell, through which the current passes. Across the circuit is shunted a galvanometer of the Einthoven pattern, containing two fine silver strings free to move laterally in a strong magnetic field. These are represented by A B, the magnet poles being M M. When a bright part of the photograph admits of light falling on the sensitive cell current passes through A B, and it shifts aside, allowing light from a Nernst lamp N₂ to enter the prism P, whence it is reflected on to the second cell S S. The telephone lines connecting the two instruments go direct to the wires of a similar galvanometer, which is in series with the galvanometer of the transmitting instrument. If we imagine M M to be the receiving galvanometer, then we remove the prism P, and the light acts on a sensitive photographic film attached to the drum C, which revolves synchronously with the glass cylinder of the sending instrument.

The inertia of selenium once overcome, the metal immediately becomes of great use for many purposes. Professor Korn's method of *compensation* is to let the light fall at the same time on two cells of opposite characteristics; one has great inertia and small sensitiveness, the other low inertia and great sensitiveness. By using the two cells on opposite sides of a Wheatstone bridge, dividing the battery into two parts for the other sides, the deflection in the galvanometer is very rapid. You will see the effect from the two

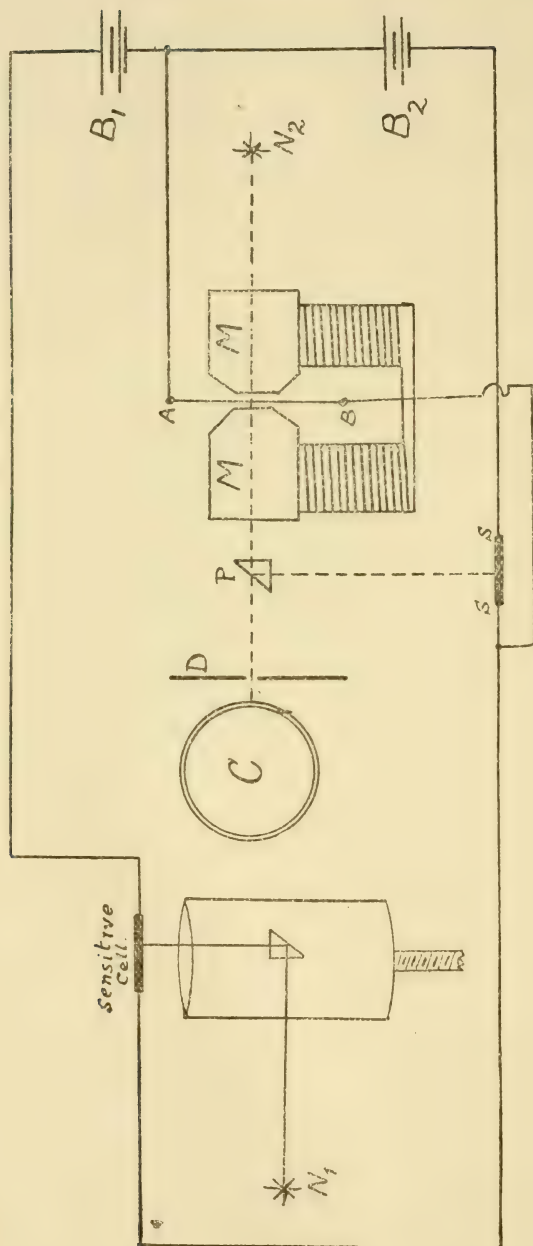


FIG. 3.

curves now shown on the screen; that above the axis along which exposure is measured is the sensitive cell, that below this axis the cell of low sensitiveness. Clearly the current passed through the galvanometer is that obtained by joining the sums of the ordinates. This gives the small curve shown as the shaded portion. When the illumination is thrown on the cell the current rises very rapidly instead of gradually, whilst when it is suddenly shut off (at P in the upper curve) it drops to zero almost instantly instead of falling gradually.

I shall now show, by means of a meter, an image of the pointer of which will be projected on to the screen, how the inertia of selenium is overcome. You will first see that if I take away the screen so as to allow light to fall on the selenium cell, current passes into the galvanometer, and the needle slowly deflects several degrees. Now, I quickly shut off the light by intercepting it with the screen, and the needle comes slowly backwards. Such sluggish movement would be impossible for the purposes of photo-telegraphy, where at least half a dozen changes per second are required to be recorded abruptly even in transmitting the simple portraits to which the selenium process is limited.

Now, using two cells of different characteristics and a Wheatstone bridge arrangement, I will once more allow light to fall suddenly on the two cells simultaneously, and you will see that the galvanometer needle records the change in resistance of the combination quite quickly; the combination is even more noticeable when the light is suddenly shut off again, the needle returning to zero with great rapidity. This compensated arrangement of selenium cells at once renders their use of practical value for various physical and optical measurements. Professor Korn has found that for an increase in the illumination δI , the current obtained is given by the equation

$y = a \cdot \delta I \cdot e^{-\beta t - \frac{1}{m}}$, where y is the current, a the sensitiveness of the cell, β and m its inertia constants, and e the base of Napierian logarithms. For two cells to be combined to the greatest advantage we must have them such that if their equations are respectively

$$y_1 = a_1 \delta I \cdot e^{-\beta_1 m - \frac{1}{t}}$$

and

$$y_2 = a_2 \delta I \cdot e^{-\beta_2 m - \frac{1}{t}}$$

then

$$\frac{d(y_1 - y_2)}{dt} = 0.$$

This makes the condition for good compensation that

$$a_1 \beta_1 = a_2 \beta_2.$$

m is usually almost constant, and with suitable Giltay cells is about $\frac{2}{3}$.

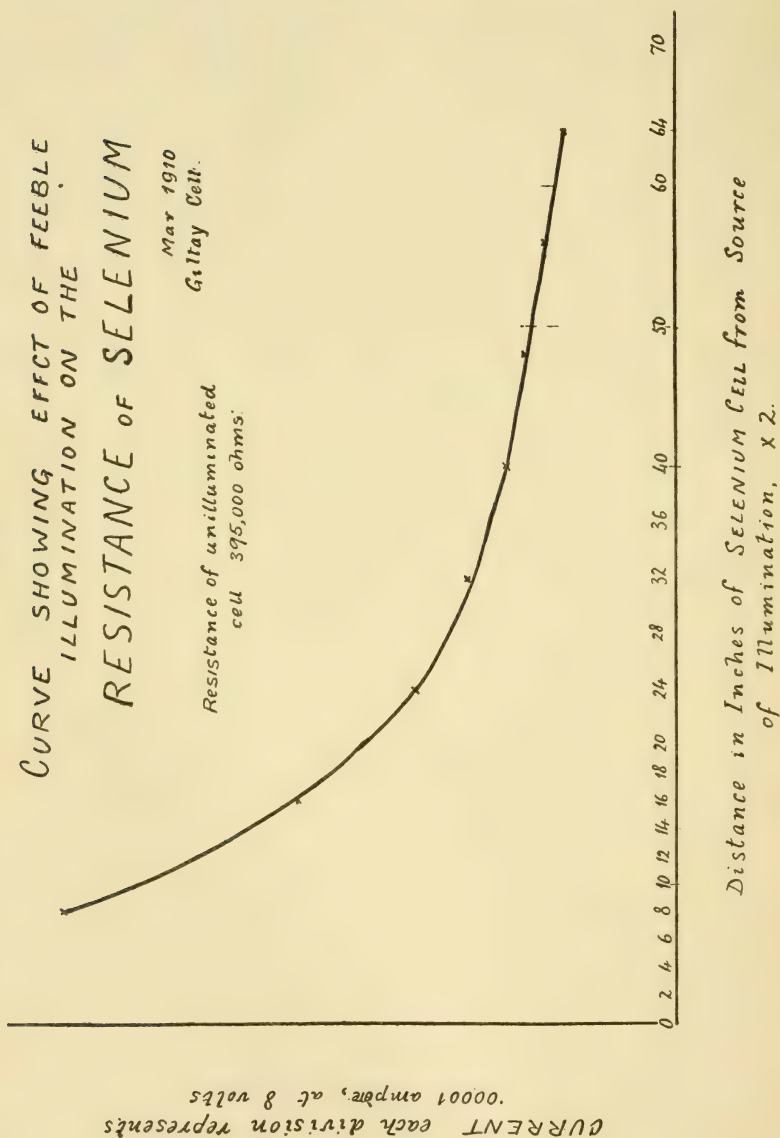


FIG. 4.

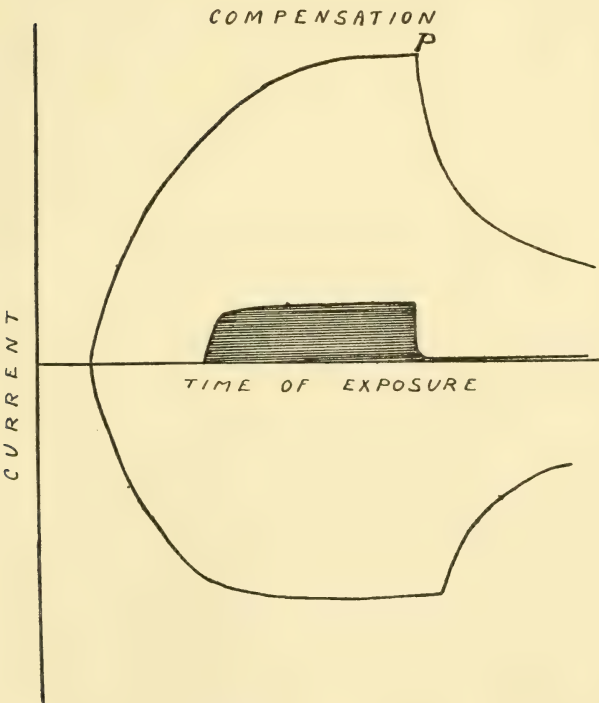
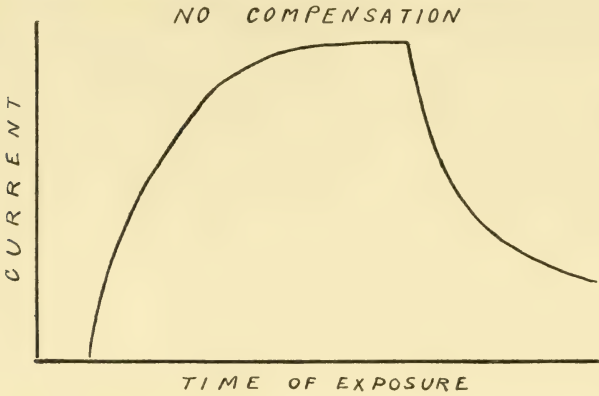


FIG. 5.

In practical language the condition for compensation is that the principal cell should have great sensitiveness and a small inertia constant, the compensation cell low sensitiveness and a high inertia constant, the product of sensitiveness and inertia constant being the same in the case of both cells.

The physical properties of selenium are of such importance that I feel I may be allowed to digress for a few moments to show one way in which they may be utilised to solve a problem that has long occupied many investigators, viz., the satisfactory measurement of the beam of heterogeneous rays from an X-ray tube. Whenever a new tube is used in radiographic work, a different voltage, or different interrupter or coil, the time of exposure for the photographic plate has to be determined anew. The strength of the tube under any conditions can, however, be determined by means of a simple piece of apparatus which I have constructed, the working of which I shall now be able to show you.

If the X-rays fall on a fluorescent screen of barium platino-cyanide, the screen absorbs them and emits yellowish-green visible rays; this transformed energy is capable of affecting a very sensitive selenium cell when placed in contact with the screen, the resistance becoming less the greater the fluorescence. You will see here a selenium cell of approximately 395,000 ohms resistance, over which is placed a small fluorescent screen of the same size; the cell is put in series with a battery of 100 volts and a milli-ampère metre, the divisions of which may be made to correspond to some arbitrary scale, or to the time necessary for the exposure of a given make of photographic plate.

The dividing of the dial depends on two things: first, the characteristic curve of the selenium cell connecting its resistance with the strength of illumination, the linear distance of the source from the cell being in this case the most convenient to employ.

Secondly, this characteristic curve must be modified to meet the case of illumination by the rays from the anti-cathode, which do not necessarily diminish in their power to make the screen fluoresce as the square of the distance from it. You will see on the screen the characteristic curve of a selected selenium cell for feeble illumination, the maximum being of about the same wave-length as that of the fluorescence, showing the relation between resistance and distance separating the source of illumination and the cell, and also the modified curve showing a similar relation between resistance and distance between anti-cathode and cell, with the screen in contact. The portion of the first curve most nearly asymptotic is best to employ for the work, and from the second curve the dial scale of the metre can be easily calibrated. If, now, I vary the height of the X-ray tube from the measuring apparatus, you will see that the metre needle is deflected less as the distance between tube and cell is increased. The actual instrument is provided with a scale divided

so as to show comparative times of exposure, and by its use radiographic work can be greatly facilitated.

It is interesting to note that the effect of the rays on the fluorescent screen, as estimated by the selenium cell, differs less with increasing distance, the further the anti-cathode is from it :—

Distance of Anti-cathode from Apparatus.	Current Recorded in Milli-amperes.	Difference.
Inches.		
6	0.33	..
8	0.27	0.06
10	0.22	0.05
12	0.20	0.02
14	0.18	0.02
16	0.16	0.02

A good deal of time has, I am afraid, been taken up in giving details of apparatus, but I will now show some of the results that have been obtained in practice. The selenium machines already referred to were operated between Paris, Manchester and London until the end of the year 1908. The first photograph received [slide] was of the King, and was received at the 'Daily Mirror' installation in November 1907. Several results will now be shown in the lantern, and you will observe that they are all composed of parallel lines which widen or "thin" according to the density of the picture. These lines correspond to the movement of the shutter attached to the strings of the Einthoven galvanometer, which regulates the thickness of the spot of light focussed on the revolving sensitive film. This spot of light traces a spiral line round the film, which, when developed, is laid flat, and the spiral becomes resolved into so many parallel lines.

Late in 1908 Professor Korn introduced his telautograph, in which a Caselli transmitter, such as already described for the telecograph, is used, and a line sketch or half-tone photograph is attached to the drum. The receiver is similar to that used in the selenium machines, a spot of light cast on a revolving sensitive film being shut off every time current flows through the wire of the galvanometer and displaces it; when displaced the shadow of the wire falls over a fine slit placed in front of the film, and so prevents the light from passing through to it. A line sketch transmitted from Paris to London in this way is now shown. The methods of synchronising the sending and receiving cylinders is the same as that used in the telecograph, but Professor Korn's work was done prior to mine, and his arrangements were therefore copied by me. Similar methods have been adopted for many years, however, in certain systems of ordinary telegraphy.

There is a great deal of interesting matter connected with the
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efficiency of the galvanometer-receiving apparatus, and the vast amount of careful work done by Professor Korn to increase it, which time quite forbids my mentioning, and I will therefore pass on to the latest phase of photo-telegraphic work—the experiments now being carried out to effect wireless transmissions.

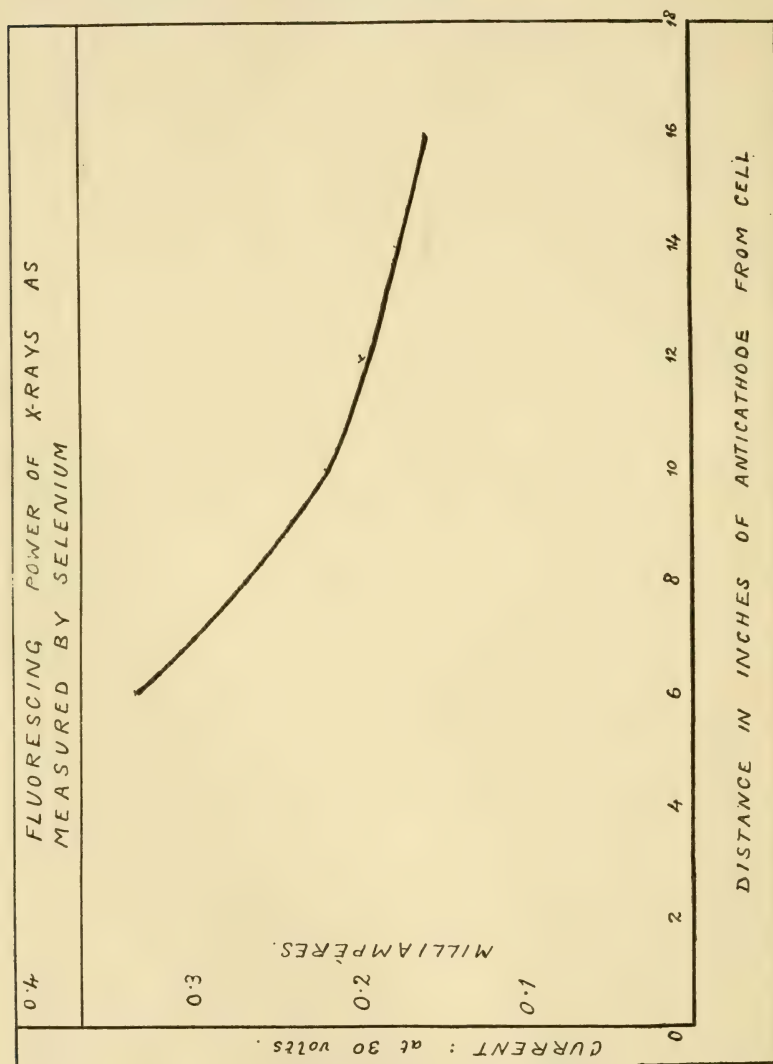


FIG. 6.

The wireless apparatus for transmitting sketches, writing, or simple photographic images over distances up to about fifty miles may perhaps be looked upon as rather rudimentary, but I shall be able to show from actual results that it is at any rate practicable, and it is certainly more simple than any method based on later wireless researches.

I will first show you an experiment, for the simplicity of which I must ask your pardon; but it illustrates so clearly how easy it really is to transmit a photograph by wireless under ideal conditions. I have here a small electric lamp, coupled up with the local side of a relay and battery, the relay being actuated by means of a coherer detector. At the other side of the platform there is a Morse key, which, when depressed, closes the primary circuit of an induction coil, the secondary being coupled up in the usual way to give oscillations. When I press the key, and thereby send a signal, you see that the lamp at once lights up. If the coherer be tapped the lamp is extinguished, and another tap of the Morse key causes it to light again.

Now suppose that the taps of the Morse key were controlled by the lines in a photograph or sketch, and that the light from the lamp were concentrated on a revolving photographic film, and you will see at once how a photograph could be transmitted by wireless telegraphy.

Such a process would be utterly impracticable commercially, but my telegraphic system can be used with success in its place. A line picture prepared in the way already described is attached to the drum of the transmitter, and the intermittent current which is ordinarily passed into the telephone line goes into an electromagnet, M in Fig. 4, which then attracts a soft iron diaphragm attached to brass springs, which are fixed to two rigid supports. Every time current flows through the magnet coils this diaphragm is attracted to it, and the platinum contacts P Q are brought together; when the current flows, and P Q are in contact, the primary circuit of a transformer is closed, and the secondary having a spark gap and being inductively coupled to the aerial and earth, a signal is transmitted into space. Thus in the wireless transmitter the only difference from ordinary telegraphy lies in the fact that the length of the signals and their distance apart is regulated by the lines composing the sketch or photograph.

When working with high voltages in the primary, such as 110, arcing is liable to take place, and hence the distance between *p* and *q* when not attracted must be considerable. This means that the distance between the diaphragm clamps, *ss* in the figure, must be short, and the German-silver spring of which the diaphragm is made must be thick, these two conditions making the natural period of vibration very short. I have, however, found that by interposing a mercury motor-interrupter in the primary circuit, arcing is

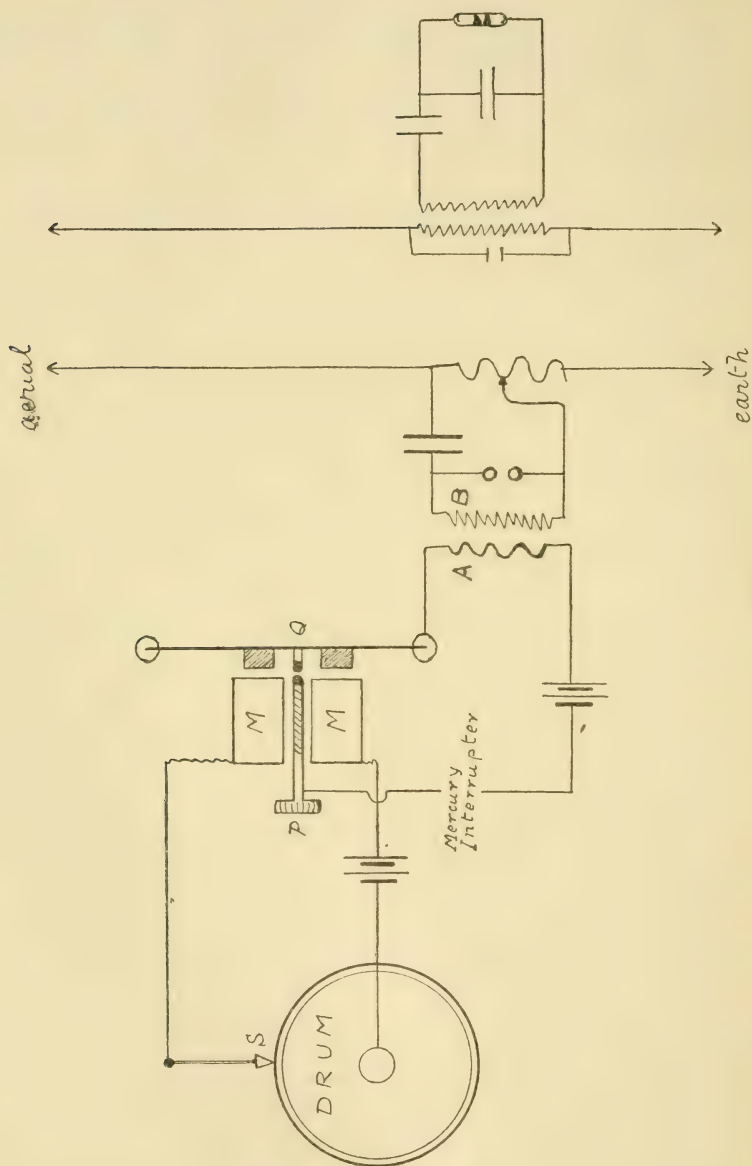


FIG. 7.

almost entirely avoided, as if an arc be formed the current is interrupted an instant later and the arcing ceases in consequence.

The receiving apparatus is very simple, and depends, for short-distance work, upon a coherer cymoscope, the decohering apparatus being of a particular character. Every time an oscillation passes to the antenna, the coherer becomes conductive in the ordinary way, and a relay is actuated; this relay is usually made to start a hammer vibrating, the hammer hitting the coherer, and thus causing it to loose its conductive power. But a vibrating hammer is useless for the photo-telegraphic receiver, and it is essential to have one strike only on the coherer for each signal detected.

The form of apparatus I have employed for this purpose is seen diagrammatically in the next lantern-slide (Fig. 8). EE is the magnet which is actuated by the relay R. It then attracts an

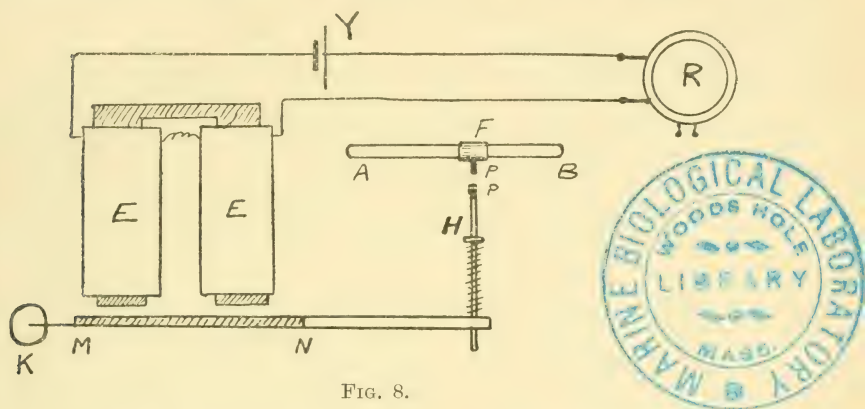


FIG. 8.

armature MN, which moves towards the magnet poles and brings a resilient hammer H, fitted with a platinum contact *p*, against the coherer. The coherer AB is also fitted with a collar F and contact pin, so that in the act of striking the coherer the hammer closes a local circuit, and so causes a black mark to appear on the chemical paper. Successive distinct marks can be obtained in .017 second in this way, which is considerably more rapid, I believe, than a decoherer was given credit for.

There is not sufficient time to show an actual transmission by wireless, and I should to make it clear that only sketches of the simplest character are at present being transmitted. But as you will see from the result now thrown on the screen—a simple portrait of his Majesty the King—the images are recognisable, and merely require slightly more detail to make them quite comparable with the early results in line obtained by Professor Korn's tel-autograph.

Another result shows a plan transmitted by wireless: here an

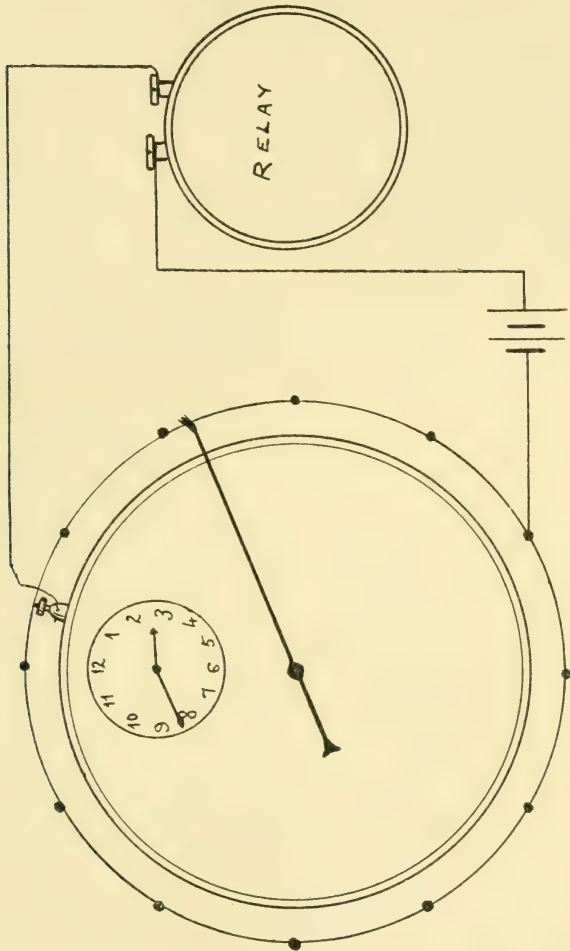
island is seen represented and a lighthouse—or it might be a fort—and by means of letters the positions of sections of an army on the island are supposed to be designated, while the shaded portion might mean that the “enemy” is in that part of the island. Such plans as these could be drawn direct in shellac ink on a slip of metallic foil, placed upon a portable machine coupled to a portable military wireless set, and communicated from one section of an army to another. The small portable machines I have already shown are used for the wireless transmissions, and they possess the advantage that “tapping” of the communications would be quite impossible. It is for this reason that I think the method would be of such value for military and naval purposes: even supposing that anyone wishing to intercept a plan or written message were to have an exactly similar instrument, with the same dimensions, screw-threads and so on, by merely altering the rate of running by 5 or 10 per cent., according to prearranged signals, the picture as received by the intercepting party would be quite unintelligible and confused.

We have already seen that in the telegraphy of a picture by any system accurate synchronising of the sending and receiving apparatus is essential. Where a metallic circuit links the transmitting and receiving instruments together the matter is an easy one, and we have seen in what way it is effected. But when dealing with wireless work the question of synchronism becomes more serious. I have employed two methods, each of which appears to answer satisfactorily, and as they are very important I will devote a few moments to their description.

The first method secures accurate synchronism independently of any wireless communication. You have already seen how in the ordinary telegraphic work the receiving cylinder is driven rather faster than the sending one, and when it finishes up a complete turn too soon it is arrested until the sending cylinder has caught it up, when the latter sends a reverse current which is responsible for its release. But in the wireless apparatus both sending and receiving cylinders are driven too fast, so to speak—that is, they are made to revolve in $4\frac{3}{4}$ seconds instead of a nominal 5. A check comes into play at the end of the revolution, and the cylinder is stopped until the 5 seconds are completed, the motor working against a friction clutch in the ordinary way during the stop. At the end of the fifth second each cylinder is automatically released by chronometric means, in the manner shown in the next diagram. [Lantern slide.]

Here you will see that a special form of clock is used, with a centre seconds hand which projects beyond the face by about an inch, and to the end of it is attached a brush of exceedingly fine silver wires. At every twelfth part of the circumference of the clock dial is fixed a platinum pin, and consequently every five seconds the little brush wipes against the convex surface of one of them. Each of these pins is connected with one terminal of a battery B, the other side of the

battery leading to the relay R, as does also the centre seconds hand. Therefore each time the brush wipes against a pin the circuit is closed, and the relay throws into action the local circuit connected up with the terminals T T. This circuit excites an electromagnet, which



CHRONOMETRIC SYNCHRONISER FOR WIRELESS APPARATUS.

FIG. 9.

attracts an armature and pulls away the check which is holding back the cylinder. At the end of each 5 seconds the cylinders consequently recommence turning.

Well calibrated clocks of the pattern used will keep good time for

the period taken to transmit a picture, one gaining on the other quite an inappreciable amount, depending on the friction of the brush against the pins. By this means the two cylinders are kept in very fair synchronism independently of any wireless communication, and the less the interval between the stopping and restarting of the cylinders be made, the more accurate and satisfactory will be the effect.

The other method of synchronising is controlled by electromagnetic oscillations. Let us suppose that a coherer is being used as cymoscope, the transmitting cylinder is kept running without any interruption, but by means of a fleeting contact it sends out a wave at the conclusion of its turn, a bare space in the picture being necessary about half a second beforehand, so that no waves are sent out for the half-second previously. The receiving cylinder is driven too quickly, and checked at the end of the revolution. It then by means of a cam pressing down a spring lever throws out of circuit the marking current and brings into circuit the relay which actuates the electromagnetic release. Consequently, when the synchronising wave is received, the coherer causes the relay to work, the release is effected, and the receiving cylinder starts a new revolution in unison with the transmitter.

This means of synchronising is only possible in cases where a cymoscope is employed that is capable of actuating a relay, and you will therefore see that it is out of the question except for short distances. I am therefore using the chronometric system already described in the apparatus, and it is being embodied in the quartz fibre apparatus I am now about to describe. I must first remark that the wireless work has been greatly facilitated by the courteous assistance so readily given by the Marconi Company.

The general form of the Einthoven galvanometer is well known, and the modified type of it used by Professor Korn for phototelegraphic purposes has been already shown. If now we make the magnetic field very much more intense by building the field magnets heavier, and using a large number of ampere turns in the winding, and also employ a "string," which is very much more elastic than the silver ribbon, the displacement of the string will be correspondingly greater. The silvered quartz fibre used by Duddell for this purpose gives an extremely sensitive instrument, and very appreciable displacement is obtained with the current from one dry cell passing through 35 to 90 megohms resistance.

It is not long since Professor Fleming explained at this Institution the valve receiver for detecting wireless oscillations; in ordinary wireless telegraphy the minute alternating currents are rectified, and sounds are heard in the telephone in circuit owing to small unidirectional currents. If these currents be passed through the silvered quartz string of the galvanometer the string is shifted. If, therefore, we cause a shadow of the string to lie over a fine slit any displacement will cause the slit to be opened, as it were; the shadow will be

shifted off the slit, and light will be free to pass through it. Oscillations corresponding to the lines in a photograph or sketch could therefore be utilised to cause shifting of the shutter in the manner I have already described for Korn's telautograph, and a sensitive photographic film could be revolved on a drum behind the slit to receive the picture. Such an apparatus is now in course of preparation, but the amount of light that passes through the slit is extremely small, owing to the fineness of the fibre. Mr. Sanger-Shepherd has therefore attached a minute shutter to the fibre, crossing the optic axis; this enables me to use a very much wider slit, and also to adopt the alternative procedure for reception, which you will now see represented in the diagram on the screen.

For photographic reception the oscillation is passed into the valve detector, and thence to the quartz fibre A B, which is stretched across the field of the magnets (not shown), whose poles are bored with a tunnel through which the beam of light is directed. When the fibre is displaced light is enabled to pass through a fine slit W, and so act on the photographic film. Where, however, the shutter is attached to the fibre a much wider slit can be used, and then a pair of narrow compensated selenium cells S S are placed behind the slit W, a positive lens being interposed. When a signal corresponding to a dot in the photograph (i.e. the traversal of a line by the stylus) is received, the fibre shifts, light falls on the cells S S, and their resistance is decreased sufficiently to enable the battery E to actuate the relay R. This closes a local circuit, in which the teletograph receiver is included, and a mark appears on the paper. In this way a visible record is obtained, which greatly facilitates the process.

Wireless photo-telegraphy may eventually prove of more utility than the closed-circuit methods, because it would bring America within reach of this country, and would enable communication to be made where telephone or telegraph lines did not exist. It is not limited to photographs—banking signatures, sketches, maps, plans and writing could be transmitted. But I would point out most particularly that the work is as yet in the very earliest stages, and that in giving you some account of it to-night I may be bringing before your notice methods and systems on which a few years hence you will look back with a smile—as curious merely from a historical point of view.

[T. T. B.]

WEEKLY EVENING MEETING,

Friday, April 29, 1910.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L.
F.R.S., President, in the Chair.

TEMPEST ANDERSON, Esq. M.D. D.Sc. *M.R.I.**Matavanu: A New Volcano in Savaii (German Samoa).*

THOUGH not the seat of government, Savaii is the largest of the Samoan Islands in the Central Pacific Ocean. It has a backbone of volcanic mountains, some of which rise to a height of over 4000 feet; most of them are extinct or dormant, but there have been several small eruptions within the last 200 years, and one as lately as 1902.

The Volcano of Matavanu was formed in 1905 to the north of the main ridge, and near the centre of the island. The early part of the eruption was characterised by explosions, and the ejecta were mainly solid, but later on an enormous quantity of very fluid basic lava has been discharged. This has flowed by a sinuous course of about 10 miles into the sea, devastated some of the most fertile land in the Island, and covered it up with lava fields probably not less than twenty square miles in area.

The crater contains a lake, or rather river, of molten lava so fluid that it rises in incandescent fountains, beats in waves on the walls, and rushes with great velocity down into a gulf or tunnel at one end of the crater. The lava, still liquid, runs in a passage, or perhaps system of passages, under the surface of the lava field, its course being traceable by a line of large fumaroles till, still in a fluid condition, it reaches the sea, into which it flows with energetic explosions and the discharge of large volumes of steam, black sand, and fragments of lava. Where the action is less violent a structure resembling that of some varieties of pillow lava is produced.

Photographs were shown on the screen illustrating the crater, the lava fields, with their subsidences and tunnels, the explosions, as well as others which enabled a comparison to be made between the devastated and untouched parts of the Island.

[T. A.]

ANNUAL MEETING,

Monday, May 2, 1910.

THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L. LL.D. F.R.S.,
President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1909, testifying to the continued prosperity and efficient management of the Institution, was read and adopted, and the Report on the Davy Faraday Research Laboratory of the Royal Institution, which accompanied it, was also read.

Thirty-three new Members were elected in 1909.

Sixty-three Lectures and Twenty Evening Discourses were delivered in 1909.

The Books and Pamphlets presented in 1909 amounted to about 258 volumes, making, with 728 volumes (including Periodicals bound) purchased by the Managers, a total of 986 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and the Honorary Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year :

PRESIDENT—The Duke of Northumberland, K.G. P.C. D.C.L. LL.D. F.R.S.

TREASURER—Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S.

SECRETARY—Sir William Crookes, LL.D. D.Sc. F.R.S.

MANAGERS.

Sir Thomas Barlow, Bart., K.C.V.O. M.D.
LL.D. D.Sc. F.R.S. Pres.R.C.P.
William Phipson Beale, Esq., M.P. K.C.
F.C.S.
Henry E. Armstrong, Esq., LL.D. D.Sc.
F.R.S.
The Right Hon. Sir Henry Burton Buckley,
P.C. M.A.
Sir John Wolfe Barry, K.C.B. LL.D. F.R.S.
Sir Henry Cunynghame, K.C.B. M.A.
Alfred B. Kempe, Esq., M.A. D.C.L. Treas.R.S.
Sir William Huggins, O.M. K.C.B., D.C.L.
F.R.S.
Sir Francis Laking, Bart., G.C.V.O. M.D.
Alexander C. Ionides, Esq. [LL.D.
George Matthey, Esq., F.R.S.
Rudolph Messel, Esq., Ph.D. F.C.S.
The Right Hon. Earl of Plymouth, P.C.
C.B. M.A. D.L. J.P.
The Right Hon. Sir James Stirling, P.C.
LL.D. F.R.S.
Sir Philip Watts, K.C.B. LL.D. D.Sc. F.R.S.

VISITORS.

William Arthur Brailey, Esq., M.D. M.A.
M.R.C.S.
Sir Frederick Fison, Bart., M.A. F.C.S.
James Mackenzie Davidson, Esq., M.B.
C.M.
Arthur Croft Hill, Esq., M.D. M.R.C.S.
John William Gordon, Esq.
James Dundas Grant, Esq., M.D. M.A.
F.R.C.S.
Major-General Sir Coleridge Grove,
K.C.B.
Charles Edward Groves, Esq., F.R.S.
A. Henry Savage Landor, Esq.
Sir Alexander Mackenzie, Mus.Doc. D.C.L.
LL.D.
Robert Mond, Esq., M.A. F.R.S.E.
Major Percy A. MacMahon, R.A. Sc.D.
F.R.S.
Commendatore G. Marconi, LL.D. D.Sc.
Emile R. Merton, Esq.
Samuel West, Esq., M.D. M.A. F.R.C.P.

WEEKLY EVENING MEETING,

Friday, May 6, 1910.

THE RIGHT HON. SIR JOHN FLETCHER MOULTON, P.C. M.A.
F.R.S., Vice-President, in the Chair.

SIR ALMROTH E. WRIGHT, M.D. Sc.D. F.R.S. *M.R.I.*

Auto-inoculation.

[NO ABSTRACT.]

[In consequence of the lamented death of His Majesty King Edward, the Patron of the Institution, the Evening Meetings were discontinued for two weeks.]

GENERAL MONTHLY MEETING,

Monday, May 9, 1910.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S.,
President, in the Chair.

It was announced that His Grace the President had nominated the following gentlemen as Vice-Presidents for the ensuing year :—

Sir Thomas Barlow, Bart., K.C.V.O. M.D. LL.D. D.Sc. F.R.S.
Pres.R.C.P.

The Right Hon. Sir Henry Burton Buckley, P.C. M.A.

Sir William Huggins, O.M. K.C.B. D.C.L. LL.D. F.R.S.

Alfred Bray Kempe, Esq., M.A. D.C.L. Treas.R.S.

Sir Francis Laking, Bart., G.C.V.O. M.D. LL.D.

George Matthey, Esq., F.R.S.

Sir James Crichton-Browne, M.D. LL.D. D.Sc. F.R.S.
(Treasurer).

Sir William Crookes, LL.D. D.Sc. F.R.S. (Honorary Secretary).

The Special Thanks of the Members were returned to Henry Francis Makins, Esq., for his Donation of Fifty Pounds to the Fund for the Promotion of Experimental Research at Low Temperatures.

The Meeting adjourned to Monday, May 23, at Five o'clock, for the purpose of passing a Resolution of Condolence on the occasion of the lamented death of His Majesty King Edward VII., Patron of the Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Secretary of State for India—2nd Report on Fruit Experiments at Pisa. 8vo. 1910.

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XIX. 1o Semestre, Fasc. 5-7. 8vo. 1910.

Agricultural Society, Royal—Journal, Vol. LXX. 8vo. 1909.

Allegheny Observatory—Publications, Vol. I. No. 21. 4to. 1910.

American Geographical Society—Bulletin, Vol. XLII. No. 3. 8vo. 1910.

American Philosophical Society—Proceedings, Vol. XLVIII. No. 193. 8vo. 1909.

List of Fellows. 8vo. 1910.

Asiatic Society, Royal—Journal for April, 1910. 8vo.

Asiatic Society, Royal (Bombay Branch)—Journal, Vol. XXIII. No. 64. 8vo. 1910.

Association of Accountants—Journal, Vol. III. No. 9. 8vo. 1910.

- Astronomical Society, Royal*—Monthly Notices, Vol. LXX. No. 5. 8vo. 1910.
Memoirs, Vol. LIX. Part 4. 4to. 1910.
Automobile Club—Journal for April, 1910.
Ball, Sir Robert S., M.A. LL.D. F.R.S. (the Author)—Contributions to the Theory of Screws. 8vo. 1910.
Bankers, Institute of—Journal, Vol. XXXI. Part 5. 8vo. 1910.
Batavia, Royal Magnetical and Meteorological Observatory—Regenwaarne-
 mingen in Nederlandisch-Indie, 1908. Deel I.-II. 8vo. 1909.
Belgium, Royal Academy of Sciences—Bulletin, 1910, Nos. 1-2. 8vo.
Boston Public Library—Bulletin, Vol. III. No. 1. 8vo. 1910.
Boston Society of Natural History—Proceedings, Vol. XXXIV. Nos. 5-8. 8vo.
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Occasional Papers, VII. Fauna of New England, No. 11. 8vo. 1909.
British Architects, Royal Institute of—Journal, Third Series, Vol. XVII. Nos.
 11-13. 4to. 1910.
British Astronomical Association—Journal, Vol. XX. No. 6. 8vo. 1910.
Memoirs, Vol. XVI. Part 4. 8vo. 1910.
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Canada, Office of the Archivist, Ottawa—Journal of the Yukon, 1847-48. By
 A. H. Murray. 8vo. 1910.
Chemical Industry, Society of—Journal, Vol. XXIX. No. 6-8. 8vo. 1910.
Chemical Society—Proceedings, Vol. XXVI. No. 370. 8vo. 1910.
Journal for April, 1910. 8vo.
Chicago, John Crerar Library—15th Annual Report, 1909. 8vo. 1910.
Cracovie, Académie des Sciences—Bulletin, 1910, Classe des Sciences, Nos. 2A-
 3B; Classe de Philologie, 1909, Nos. 9-10. 1910, Nos. 1-2. 8vo.
East India Association—Journal, New Series, Vol. I. No. 2. 8vo. 1910.
Editors—Aeronautical Journal for April 1910. 8vo.
Agricultural Economist for April-May, 1910. 4to.
American Journal of Science for April, 1910. 8vo.
Astrophysical Journal for April, 1910. 8vo.
Athenæum for April, 1910. 4to.
Author for May, 1910. 8vo.
Chemical News for April, 1910. 4to.
Chemist and Druggist for April, 1910. 8vo.
Concrete for April, 1910. 8vo.
Dyer and Calico Printer for April, 1910. 4to.
Electrical Contractor for April, 1910. 8vo.
Electrical Engineer for April, 1910. 4to.
Electrical Engineering for April, 1910. 4to.
Electrical Review for April, 1910. 4to.
Electrical Times for April, 1910. 4to.
Electricity for April, 1910. 8vo.
Engineer for April, 1910. fol.
Engineer-in-Charge for April, 1910. 8vo.
Engineering for April, 1910. fol.
Horological Journal for April, 1910. 8vo.
Illuminating Engineer for April, 1910. 8vo.
Journal of the British Dental Association for April, 1910. 8vo.
Journal of Physical Chemistry for March-April, 1910. 8vo.
Law Journal for April, 1910. 8vo.
London University Gazette for April, 1910. 4to.
Model Engineer for April, 1910. 8vo.
Mois Scientifique for March-April, 1910. 8vo.
Motor Car Journal for April, 1910. 4to.
Musical Times for April, 1910. 8vo.
Nature for April, 1910. 4to.
New Church Magazine for May, 1910. 8vo.

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 Page's Weekly for April, 1910. 8vo.
 Physical Review for April, 1910. 8vo.
 Science Abstracts for March-April, 1910. 8vo.
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 Terrestrial Magnetism for March, 1910. 8vo.
 Zoophilist for April, 1910. 4to.
Electrical Engineers, Institution of—Journal, Vol. XLIV. No. 200. 1910. 8vo.
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Franklin Institute—Journal, Vol. CLXIX. No. 3-5. 8vo. 1910.
Geneva, Société de Physique—Memoires, Vol. XXXVI. Fasc. 2. 4to. 1910.
Geographical Society, Royal—Journal, Vol. XXXV. No. 5. 8vo. 1910.
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Manchester, Municipal School of Technology—Report of the Godlee Observatory, 1909. 8vo. 1910.
Microscopical Society, Royal—Journal, 1910, Part 2. 8vo.
Monaco, Musée Océanographique—Bulletin, Nos. 163-166. 8vo. 1910.
Montpellier, Académie des Sciences—Bulletin, April 1910. 8vo.
Munich, Royal Bavarian Academy of Sciences—Abhandlungen, Band XXV. Ab. 1-3. Sup. Band II. Ab. 7-8. Band III. Ab. 1. 4to. 1909.
 Sitzungsberichte, 1909, Ab. 15-20; 1910, Ab. 1-4. 8vo. 1909-10.
National Church League—Gazette for April, 1910. 8vo.
National Physical Laboratory—Collected Researches, Vol. VI. 4to. 1910.
Navy League—The Navy for April, 1910. 8vo.
New York, Society for Experimental Biology—Proceedings, Vol. VII. No. 3. 8vo. 1910.
Onnes, Dr. H. K.—Communications from the Physical Laboratory of the University of Leiden, No. 114. 8vo. 1910.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for March, 1910. 4to.
Pennsylvania University—Contributions from the Zoological Laboratory, Vol. XV. 8vo. 1910.
Pharmaceutical Society of Great Britain—Journal for April, 1910. 8vo.
Philadelphia Academy of Natural Sciences—Proceedings, Vol. LXI. Part 3. 8vo. 1909.
Photographic Society, Royal—Journal, Vol. L. No. 4. 8vo. 1910.
Post Office Electrical Engineers, Institution of—Journal, Vol. III. Part 1. 8vo. 1910.
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Röntgen Society—Journal, Vol. VI. No. 23. April, 1910. 8vo.
Royal Colonial Institute—United Empire, Vol. 1. Nos. 4-5. 8vo. 1910.

- Royal Dublin Society*—Proceedings: Economic, Vol. II. No. 1; Scientific, Vol. XII. Nos. 24-29. 8vo. 1910.
- Royal Engineers' Institute*—Journal, Vol. XI. No. 5. 8vo. 1910.
- Royal Scottish Society of Arts*—Transactions, Vol. XVIII. Part 3. 8vo. 1910.
- Royal Society of Arts*—Journal for April, 1910. 8vo.
- Royal Society of Edinburgh*—Proceedings, Vol. XXX. Part 4. 8vo. 1910.
- Royal Society of London*—Proceedings, A, Vol. LXXXIII. No. 565. 8vo. 1910.
- Philosophical Transactions, A, Vol. CCX. No. 465. 4to. 1910.
- St. Petersburg, Imperial Academy of Sciences*—Bulletin, 1910, Nos. 6-7. 8vo.
- Sanitary Institute, Royal*—Journal, Vol. XXXI. No. 4. 8vo. 1910.
- Selborne Society*—Selborne Magazine for April-May, 1910. 8vo.
- Smith, B. Leigh, Esq., M.R.I.*—The Scottish Geographical Magazine, Vol. XXVI. Nos. 4-5. 8vo. 1910.
- Smithsonian Institution*—Miscellaneous Collections, Vol. LIV. Nos. 1922, 1924-26. 8vo. 1910.
- Report of U.S. National Museum, 1909. 8vo.
- Società degli Spettroscopisti Italiani*—Memorie, Vol. XXXIX. Disp. 3. 4to. 1910.
- South African Association for the Advancement of Science*—Journal, Vol. VI. No. 6. 8vo. 1910.
- Sopote, M., Esq., B.Sc. (the Author)*—The Grades of Life: being Letters on Immortality. 8vo. 1910.
- Standards, Deputy Warden of*—Report of Board of Trade on Proceedings under the Weights and Measures Acts for 1909. 4to. 1910.
- Statistical Society, Royal*—Journal, Vol. LXXIII. Part 4. 8vo. 1910.
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- Arkiv: Botanik, Band IX. Hafte 2; Matematik, Band VI. Hafte 1; Zoologi, Band VI. Hafte 1. 8vo. 1909-10.
- Les Prix Nobel en 1907. 8vo. 1909.
- United Service Institution, Royal*—Journal for April, 1910. 8vo.
- United States Department of Agriculture*—Experiment Station Record, Vol. XXII. Nos. 3-4. 8vo. 1910.
- Farmer's Bulletin, 391. 8vo. 1910.
- United States Patent Office*—Gazette, Vol. CLII. No. 5; Vol. CLIII. Nos. 1-3. 8vo. 1910.
- Upsala, Observatoire Météorologique*—Bulletin, 1909, Vol. XLI. 4to. 1910.
- Verein zur Beförderung des Gewerbflusses in Preussen*—Verhandlungen, 1910, Heft 4. 4to.
- Vienna Imperial Geological Institute*—Verhandlungen, 1910, No. 1. 8vo.
- Warsaw, Society of Sciences*—Comptes Rendus, 1910, No. 2. 8vo. 1910.
- Washington Philosophical Society*—Bulletin, Vol. XV., pp. 133-187. 8vo. 1910.
- Western Society of Engineers*—Journal, Vol. XV. No. 1. 8vo. 1910.
- Yorkshire Archaeological Society*—Journal, Vol. XX. Part 4. 8vo. 1909.
- Catalogue of Library, Part III. Additions, 1902-09. 8vo. 1909.
- Zoological Society of London*—Transactions, Vol. XIX. Parts 4-5. 4to. 1910.
- Proceedings, 1909, Part 4. 8vo. 1910.
- Report for 1909. 8vo.
- Zurich Naturforschenden Gesellschaft*—Vierteljahrsschrift, 1909, Heft 3-4. 8vo.

MEDAL.

- National Battlefields Commission, Quebec*—Medal in Commemoration of the Tercentenary of the Founding of Quebec by Champlain.

ADJOURNED GENERAL MEETING,

Monday, May 23, 1910.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. D.Sc. F.R.S., Treasurer
and Vice-President, in the Chair.

The Honorary Secretary reported, That he had received a communication from His Grace the President expressing regret that, owing to an important engagement, he was unable to attend the Meeting.

The following Address to the King was read and approved by all the Members present, standing, and authorised to be signed by His Grace the Duke of Northumberland, K.G., the President :—

TO THE KING'S MOST EXCELLENT MAJESTY.

*The humble Address of the Members of the Royal Institution of
Great Britain.*

MOST GRACIOUS SOVEREIGN,

We, your Majesty's most loyal and dutiful subjects, the Members of the Royal Institution of Great Britain, established for the promotion of Science, Literature and the Arts, respectfully crave leave to approach your Royal and Imperial Person with expressions of heartfelt sorrow at the loss sustained by your Majesty, by Queen Alexandra, the Royal Family, the Empire, and the World at large, in the death of your Majesty's revered Father, our late most gracious and beloved Sovereign, the Patron of this Institution.

We also desire to offer our most hearty and respectful congratulations upon your Majesty's Accession to the Throne, and to testify our sincere attachment to your Royal Person.

That your Majesty may long reign in Prosperity and Happiness over a free and affectionate people is the sincere wish and earnest prayer of

Your Majesty's Loyal and Dutiful Subjects,
The Members of the Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, May 27, 1910.

THE RIGHT HON. SIR HENRY BURTON BUCKLEY, P.C. M.A.,
Vice-President, in the Chair.

CAPTAIN ROBERT F. SCOTT, C.V.O. R.N. D.Sc. F.R.G.S.

The Forthcoming Antarctic Expedition.

IN setting forth the plans of the coming Antarctic Expedition, I am anxious to give such information as will enable those who have a close or personal interest in the enterprise to picture our doings during those long months when we must necessarily be cut off from communication with the world. At the same time I am too familiar with the unexpected happenings on such a venture to suppose that plans can be exactly or even closely followed, and I do not wish it to be supposed that I have failed to contemplate the possibility of circumstances which may upset some, if not all, calculations, and cause the results of the expedition to be very different from those which I am now attempting to foreshadow.

There is, unfortunately, a sharp difference of opinion as to the value of Polar exploration, and as to the results of Polar expeditions. The general public, whose knowledge of such matters is derived from the sensational press, can count success only in degrees of latitude, and hitherto it has been content to accept little more than bare assertion in support of such claims. On the other hand, there have been those better informed, and even eminent men, who have held or have affected a contempt for all result but that which accrues from the more advanced scientific study of the regions visited.

Within these limits there is every shade of opinion as to the relative value of the objects to be pursued, and beyond them there is, and I fear will ever remain, the class which sees no good at all in Polar exploration. Excepting this last, I would express the opinion that there is much to be said for all points of view.

I submit that the effort to reach a spot on the surface of the globe which has hitherto been untrodden by human feet, unseen by human eyes, is in itself laudable; and when the spot has been associated for so long a time with the imaginative ambitions of the civilised world, and when it possesses such a unique geographical position as a pole of the Earth, there is something more than mere sentiment, something more than an appeal to our sporting instinct in its attainment; it appeals to our national pride and the maintenance of great traditions, and its quest becomes an outward visible sign that we are still a nation

able and willing to undertake difficult enterprises, still capable of standing in the van of the army of progress.

But though this attainment of a pole of the Earth be in itself a high enterprise worthy of national attention, it must be obvious that there are various ways in which such a project can be undertaken. It is possible to conceive the record of a journey to the pole which would contain only an account of the number of paces taken by the party, the food eaten, or the clothes worn. The interest of such a record would be entirely marred by our disappointment that so rare an opportunity to add to human knowledge should have been missed.

It becomes, therefore, a plain duty for the explorer to bring back something more than a bare account of his movements; he must bring us every possible observation of the conditions under which his journey has been made. He must take every advantage of his unique position and opportunities to study natural phenomena, and to add to the edifice of knowledge those stones which can be quarried only in the regions he visits. Such a result cannot be achieved by a single individual or by a number of individuals trained on similar lines. The occasion calls for special knowledge and special training in many branches. I have entered into these preliminary explanations in order to show the objects I have had in view in organising the expedition.

I have arranged for a scientific staff larger than that which has been carried by any previous expedition, and for a very extensive outfit of scientific instruments and impedimenta. Doubtless there are those who will criticise this provision in view of its published object—that of reaching the South Pole. But I believe that the more intelligent section of the community will heartily approve of the endeavour to achieve the greatest possible scientific harvest which the circumstances permit.

In discussing my plans, it is perhaps as well to start with an itinerary. The *Terra Nova* will leave London in a few days—that is, on June 1. She is to sail to Portsmouth for the adjustment of her compasses, and thence to Cardiff to complete her cargo of coal. She leaves Cardiff on June 15, and will reach Cape Town, after a call at Madeira, about August 1. After a week's stay she will sail for Melbourne, reaching that port approximately on September 13. After a week at Melbourne the ship will sail to Sydney, and thence to Lyttelton, New Zealand, where she is timed to arrive on or about October 13. The few stores that have not been shipped in London, such as petrol for motor sledges, forage for the ponies, and a supply of frozen mutton, will be taken on board at Lyttelton, as also the ponies, dogs, and motor sledges. As is generally known, a member of the expedition, Mr. Meares, left London several months ago to proceed to Siberia to collect the twenty ponies and thirty dogs which I have decided to take. I have received most satisfactory accounts of his progress, and feel confident that the animals that he will ship

from Vladivostock *viâ* Kobe and Sydney to our base, will be as good as it is possible to obtain for our purposes.

Hitherto it has been the custom for expeditions to sail to the south in the latter end of December, as it has been thought impossible to penetrate the ice-pack at an earlier date. I think that in a large ship like the *Terra Nova*, which has considerable power for a vessel of her type; it is well worth making the trial to secure the advantage which would be obtained by penetrating the pack at an earlier date; and I therefore propose to leave New Zealand towards the end of November. If all goes well, we should reach McMurdo Sound towards the end of December. Should delays occur, they may be profitably employed in taking soundings and making biological investigations.

Immediately on arrival in McMurdo Sound the hut, provisions, and equipment of the Western party will be landed. The party will consist of from twenty-two to twenty-five persons, and as soon as the winter station has been thoroughly established the greater number of these will proceed to the south to lay dépôts.

I hope that it will be possible to start this party off not later than the third week in January, when sixty or seventy days remain for travelling. At the same time the ship will leave McMurdo Sound and proceed to the eastward. The region of King Edward's Land should be reached before the end of January or very early in February. If open seas are to be found in this region, they are certainly most likely to occur about this date. I believe that the exploration of King Edward's Land can best be conducted by landing a wintering party in this region, and every provision is being made for this object. A second hut, provisions, complete outfit, and travelling equipment for six men have been set apart, and, if a suitable spot can be found, a party of six or seven men will be left there.

I realise that this part of my plan is beset with many difficulties. A suitable wintering-place may not be found, and, even if found, the difficulty of landing stores remains, in a region where heavy pack-ice is continually on the move. But the interest of the exploration of King Edward's Land justifies a great effort in the attempt.

Our present knowledge of King Edward's Land is of the roughest description. We have seen rising snow-slopes, and have here and there caught a fragmentary glimpse of exposed rock. Does this mean an archipelago of small islands, or some part of the main continental mass? It is not difficult to see how many interesting problems of land and ice distribution depend on the answer to this question.

The small Eastern party, if left, will be left with full supplies and some transport facilities. They will have to face the unknown severity of a winter in one of the most inhospitable parts of the Antarctic regions; they will have to face unknown difficulties and

dangers in the journeys they undertake. But I can imagine no direction in which the hardships and difficulties of sledge journeys will be more amply rewarded. Should this party be safely landed, I should endeavour to give them some connection with the Western party—400 miles to the westward—by landing additional stores at one or two places on the Barrier edge, if such places can be found.

After landing the Eastern party, the ship will return to McMurdo Sound, and then proceed to the northward. I am hopeful that at the latest this will be in about the third week of February, and that a considerable supply of coal will still remain. If that is so, she will be directed to investigate the pack in the region of the Balleny Islands, and to proceed to the westward through or to the south of these islands. My hope is, that by then again steering to the south, she may throw some further light upon the coast-line between Cape North and Adélie Land, and reconnoitre this coast with a view to landing parties upon it on a future occasion. It will be remembered that we found the sea shallow to the westward of the Balleny Islands, and therefore, even if the *Terra Nova* is unable to undertake further geographical discovery, I hope that some time may be spent in biological investigations in the shallow waters of this sea. These objects will occupy the ship during the month of March, after which she will be directed to return to New Zealand.

Returning to the Western party, I hope that the month of April will find all safely established in the hut, with suitable dépôts laid well south on the Barrier.

During the winter, preparations will be made for a great effort to reach the South Pole in the following season. By that time we shall know what reliance can be placed respectively on the ponies, the dogs, and the motor sledges. But in any case a large party of men will be detailed for the Southern party. Some of the scientific staff will remain at the wintering station throughout the summer. A small party will act independently in the western mountains for geological purposes; but at least sixteen, and possibly more, men will accompany the main transport agents on the road to the south.

I may pause here to give my opinion on the conditions and prospects of the southern journey. We know now that the first phase of that journey must be over the plateau of the Great Barrier, the second a climb through mountain passes, and the third a traverse of a lofty inland plain. It is only possible, certainly not probable, that any means of transport can be taken beyond the first phase. If it is impossible, then we shall have, as had Sir Ernest Shackleton, to make all further advance with the unaided efforts of men alone. Shackleton's party started on the second phase with full loads, and achieved what is probably the maximum that could be accomplished under such circumstances. The only manner in which such a record could be beaten is by taking a larger party of men and sending sections of them back at intervals. This is, of

course, a well-known expedient in polar work, but it has to be remembered that each multiple of the original number of men only adds a fraction, and a diminishing fraction, to the radius of action. In other words, a party with the aid of a supporting party of equal numbers can only hope to achieve a distance one-third greater than it would have done had it been without a supporting party. Taking this fact into consideration, together with the increased risk of individual breakdowns which the larger number of men must bring, it must be evident that the achievement of the South Pole, in view of the distance which has to be traversed in the second and third phases of the journey, is by no means a certainty. Of course, one is not without hope that either the ponies, the dogs, or the motor sledges may traverse the disturbed regions of the glacier, and if this is possible the difficulty of the journey should be greatly diminished. But, even so, it must be remembered that the last phase of the journey, owing to the height of the plateau, has to be accomplished under climatic conditions which for severity are unequalled either in the Arctic or Antarctic regions.

The excessive winter cold of the Great Ice Barrier does not seem to commence to pass away until the month of September, and conditions of travelling remain comparatively severe even in October. For this reason, perhaps more on account of the animals than the men, I do not propose to start upon the southern journey until the month of October. That month and the following will be spent traversing the Barrier and ascending the glacier. I should hope to reach the upper plateau fairly early in December. An ideal day for reaching the South Pole would be the 22nd of that month, when the sun achieves its maximum altitude.

Special 4-inch theodolites have been constructed for the expedition. With such instruments, and the sun at an altitude of 23° , there is no doubt that the position of the pole could be determined with an accuracy of 1 mile, and it is interesting to consider a situation where the sun could be followed by the telescope of the instrument for twenty-four hours without any perceptible difference in its altitude.

Such is the main outline of my plan for reaching the South Pole.

I will turn now to give you some idea of the men who will help forward these plans. Time does not permit me to more than briefly note their names and the work that will be entrusted to them.

Lieut. E. R. G. R. Evans, a distinguished navigating officer in the Navy, and the possessor of former Antarctic experience, will be second in command of the expedition. He will be in command of the *Terra Nova* on her outward voyage, and be landed in the Western party.

Lieut. Victor Campbell, an ex-naval officer, will be in charge of the Eastern party.

With the Western party, for surveying and general executive

duties, there will be associated Lieut. Rennick, R.N., and Engineer-Lieut. Riley, R.N. With this party also will be Mr. Meares, whom I have already mentioned as being in charge of the ponies and dogs; Mr. Ponting, the photographer of the expedition, whose work is so well known to the public; Mr. Day, in charge of the motor sledges; and Captain Oates will assist Mr. Meares.

Without counting the scientific gentlemen, other members of the Western party will be Mr. Cherry Garrard, Mr. Grant, and Mr. Feather, together with six or more members of the crew. The Eastern party will also contain at least four members of the crew. The crew has been informed that no selection will be made for the landing parties until the ship reaches the ice. It is obvious that I shall then be better able to judge which of them is best suited for the requirements of shore work. It is perhaps interesting to note in this connection that besides Mr. Evans, Mr. Day, Mr. Feather, and Mr. Cheetham, the following members of the crew will have had previous Antarctic experience: Evans, Lashley, Crean, Williamson, Smythe, and Heald (all of whom were members of the *Discovery* expedition), and Paton (who served in the relief ship *Morning*). The knowledge which these men possess of Antarctic conditions will, of course, be a great asset.

In dealing with the scientific work of the expedition I must confine myself to noting the names of the various members of the staff who will undertake the scientific work, the localities in which their services will be employed, and the bare outline of the subjects to which their attention will be turned. Dr. Wilson will be the chief of the scientific staff; his Antarctic services are too well-known to need comment. He will, as heretofore, study the birds and mammals in his scientific capacity, and as artist continue the charming series of sketches which so greatly enhanced the *Discovery* records.

I have regarded geology as one of the most important interests which can be served on our expedition, and I have therefore included three geologists in the staff. Subject to modification, my plan is that one should be with the Eastern party, the second with the Southern party, and the third should have a roving commission to explore Victoria Land within an easy distance of the Western station. The services of two distinguished geologists have already been obtained to fill these places: they are Mr. Griffith Taylor, an Australian who has completed his studies at Cambridge as a '51 scholar; and Mr. W. G. Thomson, a Rhodes scholar, of New Zealand. The third place is not yet filled, and in filling it we propose to take the advice of Professor David, of Sydney University, who is probably the best judge in the world of the work which remains to be done and the men who should be selected to do it.

In contemplating the continued study of the marine fauna of the Antarctic regions, I have considered it as without the province of an expedition such as ours to undertake research work in oceanic depths;

such work calls for an outlay of time and ship-space which could not be afforded by the expedition. I have therefore considered that 500 fathoms is the limit at which dredging operations can be conducted, and the equipment of the ship has been arranged accordingly.

Two gentlemen have consented to accompany the expedition for this work : Mr. Nelson, from the Plymouth Biological Laboratory, will be landed with the Western party, and will be given every facility possible to conduct researches in the waters of McMurdo Sound ; Mr. Lillie will remain in the ship. It is difficult to say when and where his dredges can be brought into use, but I am hopeful that at least some opportunities may be found on the southern voyage, and that after the wintering parties have been landed, on the northern voyage, and especially in the shallow waters of the Belleny Islands, Mr. Lillie may have a chance to glean a rich harvest of result.

The subject of meteorology makes an ever-increasing appeal to the domains of Polar research, in view of the fact, which is now very generally accepted, that Polar conditions have a preponderating effect upon the weather of the whole world. For this reason I am most happy to have secured the services of an eminent physicist, Dr. G. Simpson, a member of the Meteorological Department of India. Conversant with all the latest methods of exploring the upper air-currents and investigating the electrical conditions of the atmosphere, Dr. Simpson is prepared to take the fullest advantage of the opportunities which will be afforded for the exceptional, as well as for the more usual, routine observations of our meteorological station. Realising the importance of this work, I have allowed him a special hut and space for a very large outfit of scientific instruments. In addition to the meteorological instruments, self-recording magnetic instruments will be taken with the object of comparing the magnetograms with those obtained by the *Discovery* expedition, and ascertaining the secular magnetic changes as well as of connecting the more irregular changes with other physical phenomena. Arrangements have also been made for gravity observations, for auroral photography, and for the study of the other branches of physical science which Dr. Simpson will undertake.

Ever since my first introduction to the Antarctic I have felt that a new and most interesting field of research was open to any one who with trained abilities would undertake a close study of ice-structure in that region. Nowhere in the world can ice-formations be found with more varied characteristics. It seemed to me that these varied ice-formations could tell their history to the initiated as clearly as the rocks of our geological formations. Recent inquiry has shown me that more competent persons than I have pursued this line of thought, and already the structure of the glaciers of the temperate climates has been studied with a view to disclosing the nature of their origin. For this study, and for the further investigation of physical problems which lie outside the scope of Dr. Simpson's work, I have obtained the services of

Mr. Wright, a native of Toronto, and a scholar of Caius College, Cambridge, as chemist of the expedition. Mr. Wright's work is not so clearly defined as is that of others, but I cannot help thinking that it is by means of it that we shall best attack those great problems of the southern glaciation, and especially of the Great Ice Barrier, which yet remain unsolved.

Although perhaps the main scientific work of the expedition will be accomplished by the members of the shore parties, observations of the greatest importance will be made by those who remain in the ship. To Lieut. Pennell, an officer of the Royal Navy, assisted by Lieut. Bowers, will be entrusted the survey or resurvey of any lands that may be seen, the task of keeping a complete and careful meteorological record, and the conduct of continual magnetic observations for variation as well as continual observation of the other elements. These naval officers, as well as those of the shore party, have been especially selected from that navigating branch of our service whose training best fits them for the work which they will have to perform.

I have left until last the mention of those officers who serve in that especially important dual capacity as medical and scientific men, being at the same time in charge of the health and well-being of the community and of important branches of the scientific work. Dr. Levick will be landed with the Eastern party, and of that pioneer community will be the zoologist, botanist, photographer, and doctor. Dr. Atkinson, also a surgeon from the Royal Navy, will add to his medical duties the more delicate scientific work in which he has been especially trained, the study of bacteriology and parasitology. In the latter especially, a science in which great strides have been made in recent years, he looks for important results in an entirely new field.

Those individuals who will form the shore party and crew of the *Terra Nova* number fifty in all, of which twenty-four officers and men have been lent from the Royal Navy, one from the Army, and two from the public services of India; it is sufficient to add that all have been most carefully selected for the work, all have been medically examined and found fit for it, and all have already evinced that enthusiasm which is the stepping-stone to success. I am at least confident that if success does not attend our efforts, it will not be because the endeavour of my companions has failed to deserve it.

[R. F. S.]

WEEKLY EVENING MEETING,

Friday, June 3, 1910.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L.
LL.D. F.R.S., President, in the Chair.

THE RIGHT HON. SIR RENNELL RODD, G.C.V.O. K.C.M.G. P.C.,
His Majesty's Ambassador to the Court of Italy.

Renaissance Monuments in the Roman Churches, and their Authors.

RENAISSANCE sculpture in Rome has, naturally, been overshadowed by the Antique. And yet the monuments which fill her churches, or have been broken up and hidden in crypt and cloister, display a remarkable, and, in a sense, unique development of fifteenth century art and portraiture.

Here you may follow, sometimes in one single church, the whole evolution of monumental art, from the earliest times, when it began to be an object to those who were leaving an uncertain and unquiet world, to secure a last resting place in the immediate neighbourhood of the shrine, where the relics of the saint and the presence of the miraculous elements offered the surest guarantee against disturbance. But it was only for the great and influential that room was found within the sacred edifice. Their effigies were set recumbent, inlaid in the pavement, humbly near to mother earth, the bishop clasping his crozier, the warrior of the church his cross-hilted sword, and soon the feet or knees of thronging worshippers wore away their marble out-lines and even the record of their names. Then some of the more illustrious were elevated on a base, secure above the tramping feet, as on a bier set against the wall. As time went on, a niche was hollowed out to receive the tomb: the vault became a canopy, and the effigy, lay as it were on a bed, from which angels, rudely carved, drew back a curtain to reveal the face of the sleeper. So the wall-grave arose, and the living fought with tooth and claw to secure the option of the choicest spot wherein to lie through future centuries. The delicate art of the Cosmati redeemed by exquisite colour and design the incompleteness of the sculptor's craft, and, gradually, with the spirit of the classical revival, the Gothic and Romanesque character of the wall-graves gave place to the more accomplished and conscious manner of the Renaissance. Rome, which has always rather tended to assimilate than to produce, summoned the skilled craftsmen of Tuscany and Lombardy to assist in preparing for the princes of the church, memorials appropriate to the stateliness and

magnificence of their lives. In the latter half of the fifteenth century the revived art of sculpture was touching the plenitude of achievement, and for a brief moment in the finest examples of the Roman ecclesiastical monuments, we are dimly reminded of the restraint and dignity with which the Attic Stelæ treated the presentation of death. The great prelates of the Renaissance are portrayed to us, robed and mitred, lying in tranquil sleep on the flowered sarcophagus. The saints of their choice occupy shell-vaulted niches in the pilasters which support the architrave above them, and from the lunette the Madonna looks benignly down on one who should have been her faithful servant. If the modern spirit had already invaded their lives, if rationalism and materialism were in reality fast undermining the significance of forms and ceremonies from which they turned to Pagan intellectualism, fifteenth century art at any rate preserved for a little longer the tradition of a less complex psychology. But as the struggle for mastery over material execution ceased to be apparent, the vanities of the world invaded even the monopolies of death. Not willingly did the great and powerful resign the state that had given them so much, and even in death they were reluctant to be dissociated from a part in life. The recumbent figure on the sarcophagus draws up one knee, raises the head, and, propped on one elbow, the departed prelate watches the movement of life below, or enviously regards the more sumptuous sepulchre of his rival. So Sansovino interpreted the new spirit of the age. Before long the tenant of the costly monument is sitting bolt upright, and the suggestion of death is conveyed by a skeleton deftly carved in yellow marble relieving against the background of basalt. From the baroc piles in St. Peter's the enthroned pontiffs of the seventeenth and eighteenth centuries continue, as in life, to dispense benediction or menace heresy, and, following the descending curve of degeneration, we reach the nadir in the modern horrors of the Campo Santo.

I propose to-night to deal briefly in the short time at our disposal with the second half of the fifteenth century, and attempt to establish a few attributions of authorship, to clear away a few misconceptions, due in many cases to the confused statements of the indispensable but uncritical Vasari, and to introduce to your notice certain craftsmen little known to the general public, whose contribution to the achievements of Renaissance art has emerged from the researches of Müntz and Bertolotti, and the critical investigations of Guoli, Venturi, Tschudi, Schmarzow, Fabriczy, and others.

The great Donatello visited Rome in 1433, and executed a sepulchral slab for the tomb of the Archidiacono Crivelli in the Church of Araceli, as well as a marble tabernacle, once in Sta. Maria delle Febbre, and now preserved in the Sacristy of St. Peter's, which is historically important, as being the first work of a monumental character in which he finally discarded all trace of the Gothic

tradition. While here, he no doubt came across an old associate whom he had known at Pisa, who was now engaged on various works in the Vatican, one Pippo, or Filippo di Gante, a Pisan sculptor, whose father had exercised the same craft before him, and whose son, Isaiah of Pisa, brought up it would seem in the eternal city, may be regarded, in conjunction with his constant associate, Paolo Romano, as the founder of the Roman Renaissance school. Isaiah's son, Gian Cristoforo Romano, well known for his work in the Certosa of Pavia, became its most skilful exponent in the early years of the sixteenth century. We have thus four generations of craftsmen in the same family. The principal works of Isaiah were recorded in a contemporary Latin panegyric poem by one, Porcellio de' Pandini, preserved at Pisa, and others we are able to identify by his well-marked archaic style, formed it would seem by the study of late Roman classical models, or again by the entries of payments in the pontifical accounts during the reign of Pius II., where he is found constantly working with the favourite artist of that pope, Paolo di Mariani di Tacco, better known as Paolo Romano. Time will not allow me to-night to investigate in detail the work of these two masters, and the parts they took in the most considerable monument of the time, the triumphal arch of Alfonso of Aragon, at Naples, but one or two points I must ask you to hold in mind in view of their bearing on the subsequent evolution of sculpture in Rome.

In the first place it seems to have been Isaiah of Pisa who set and fixed the type in Rome, generally adopted for the sepulchral monuments of the Renaissance. At any rate the first complete example we have is from his hand, in the tomb of Pope Eugene IV., who died in 1447. It was executed during the reign of his successor, which extended to 1455. Removed from St. Peter's to San Salvatore in Lauro, probably in the reign of Paul II., it may have suffered in transport, as certain portions, the two figures in the niches on the left side and the kneeling angels in the recess, are the work of another and apparently somewhat later artist employed to reproduce the broken pieces. The monument was evidently similar in design to another tomb of Isaiah's, namely that of Saint Monica in the church of S. Agostino, which was broken up when the church was modernised. Only the recumbent figure and the plain sarcophagus remain in situ. The four saints from the pilaster niches, much mutilated, are now set up in the porch of the transept entrance, and they are identical in design with the figures on the tomb of Eugene IV. Another monument by Isaiah, slightly earlier in date perhaps than either of these, that of the Portuguese Cardinal Chiaves, might be reconstructed from the pieces scattered over the Church of St. John Lateran. The type of Isaiah's monuments is rather Tuscan in character, modified perhaps by a study of the Roman triumphal arch. The latest record we possess of his activity is an entry in the pontifical accounts, in the year 1464, when he received payment for work done in conjunction

with Paulo Romano on the tabernacle made to contain the head of Saint Andrew, a priceless relic brought to Italy by Thomas Palaeologos, Despot of Morea. The remains of this tabernacle are now in the crypt of Saint Peter's, and a comparison of the pieces from two lunettes is interesting as showing the difference between the work of Isaiah and Paulo.

I must content myself with but a brief reference to-night to that interesting personality in the history of Roman sculpture, Paolo di Mariani, known as Paulus de Urbe, or Paulo Romano, who is not to be confounded with another earlier Magister Paolo, the author of several monuments erected at the beginning of the fifteenth century. He will, however, serve to introduce to you another name, which presents a very curious problem.

Vasari knew but little of either Isaiah or Paulo Romano. He assigns to the Florentines, Niccola and Varrone, the tabernacle of St. Andrew, and to the latter the statue of St. Andrew, near the Ponte Molle, erected on the spot where the relic was received. But the evidence of the pontifical accounts is conclusive, and they show that Paulo Romano was paid 33 ducats for its execution. In his short biography, however, Vasari tells a curious story of a rivalry which sprang up between Paulo and a certain Mino del Reame, or del Regno, which means, of course, the kingdom of Naples. This Mino, whose name he says was perhaps Dino, challenged the Roman sculptor to compete with him for the execution of a colossal statue of St. Paul, and adds that Paulo was so harassed by the persecutions of this Mino that he renounced his intention of carrying out a colossal statue of St. Peter, which had been ordered from him by Pope Pius II. Now it is true that the Vatican accounts contain records of payments for pedestals for the statues of St. Peter and St. Paul, ordered of Paulo Romano, which were to be placed at the foot of the steps leading to the old Basilica, as well as for a statue of St. Paul, while there is no entry of payment for a statue of St. Peter. On the other hand, the two statues which originally decorated the approach to the church actually exist in the sacristy of the present St. Peter's, and both of them would seem to be from the same workshop, and resemble the other work of the Roman sculptor. Again, there is another statue of St. Paul, stated by Vasari to be by Paulo Romano, which now stands on the bridge of St. Angelo. The idea of two sculptors making statues 14 or 15 feet high, merely in a spirit of rivalry is rather extravagant, but there is probably, as usual, some truth behind Vasari's story, and curiously enough we find over the door of the church of St. Giacomo in Piazza Navona two angels by different hands supporting an obliterated shield. Under one of them is carved in large letters *Opus Paoli*, and under the other *Opus Mini*. The latter, which is much the better and freer of the two, is, in my opinion, not the work of Mino da Fiesole. There are other references in Vasari, too circumstantial to be ignored, to this second Mino, and I

think there is justification for the re-establishment of a personality new to general experience, and for a differentiation of his work from that of the better known Mino of Fiesole, who has hitherto been credited with it.

Mino da Fiesole no doubt, did important work in Rome, and exercised influence over other sculptors there. To appreciate what he really did, the chronology of his visits is important. Leaving untouched the question of whether or not he was the author of the Strozzi bust in the Berlin Museum, with an inscription recording that it was made by him at Rome in 1454, when he can only have been 21 or 22, we know from the Vatican registers that he was there in 1463, when he was employed together with Isaiah and Paolo on sculptures for a pulpit which Pius II. designed to place outside the Basilica of St. Peter's, from which to give the benediction. This pulpit was never finished, but the statues of the Apostles made for it may be seen in the crypt of St. Peter's. To this period no doubt belong some panels made for a casquet to contain relics of St. Jerome ordered by Cardinal d'Estouteville for Sta. Maria Maggiore, which church his eminence was restoring at his own cost. They may be seen to-day in the Industrial Museum at Rome, and are of extremely poor workmanship, though unmistakably revealing Mino's characteristic style. In the Vatican registers no mention of him is traceable during the reign of Paul II., 1467-1471, and we can account for his presence elsewhere from 1469-73. He was apparently recalled to Rome in or about 1474, in the reign of Sixtus IV., to carry out the monument of his predecessor. In 1480 he was back in Florence, and in 1484 he died.

The manner of Mino da Fiesole is well known to all who have studied Italian art; the fine and delicate folds of his draperies, his constant use of sharp angles, with a prevailing suggestion of the diamond shape, which even seems to pervade faces and individual features; the thick eyelid and half-closed eye; the sharp knee, protruding through the diaphanous dress fabric; the regular fall in rather conventional repeating angles of draperies between the open knees of his seated figures; the flat treatment of hair, consistent with his general shadowless scheme, free from all deep cutting; the weak and rather meaningless pose of the hands; the secular and aristocratic type of his Madonnas, as opposed to the devotional type; all these, together with a certain preciousness and an artist's love of his material, which make him give the marble an ivory-like polish, and in his later work an elaboration of delicate ornament and tracery.

During his second visit to Rome after 1474, we must rather regard him as having been the guiding spirit of a great workshop from which issued a number of the finest monuments of the period. No single one is entirely from his hand, unless it be the tomb of Francesco Tuornaboni, in the Minerva Church, which was probably carried out in Florence. These extensive commissions were probably divided

up among the members of an artistic partnership, each undertaking the portion for which he had a special reputation or aptitude. In some cases the only contribution of Mino seems to have been the Madonna of the central niche or lunette.

And first, as regards the more important work in Rome, which has been wrongly, I venture to think, attributed to him. In the Church of Sta. Maria Maggiore are the component parts of a magnificent ciborium, made for Cardinal d'Estouteville, whose portrait appears on one of the panels as donor. The four large panels are now set up in the apse of the church, and a large number of smaller fragments are in the sacristy, including a relief of the Madonna and Child, inscribed *Opus Mini*, one very similar to which, but unsigned, is in the collection of Count Strogonoff. As the restoration of the church, of which this ciborium was the principal ornament, was completed by 1474, it seems probable that the altar itself was also anterior to the return of Mino da Fiesole to Rome, and were it admittedly his, it could not be contemporary with the very inferior work on the panels illustrating the life of St. Jerome. While certain pieces have features which resemble the style of the Fiesolan, especially in the poise of the figures, which rest on one leg only, I have, after a careful study of the whole, come to the conclusion that it is not his work, but that of a contemporary, whose manner presents analogies and affinities but also marked differences. The Fiesolan was not a successful craftsman in relief: witness his feeble contributions to the pulpit at Prato. The panels of this ciborium are finely and boldly conceived. The scene of the Nativity is a charming and felicitous composition. But the type of the Madonna in the Adoration of the Magi, as well as in the Assumption, is quite unlike the type of the Tuscan. Still more may this be said of the Virgin on the panel in the sacristy and its fellow in Count Strogonoff's collection, which are essentially devotional in character, and I, at any rate, know of no example by Mino da Fiesole the least resembling them. The flying angels in the *mandorlo* round the Virgin of the Assumption appear to be by the same artist who competed with Paolo Romano for the angel over the door of the Church of St. Giacomo. A tabernacle for holy oil, in the Church of Sta. Maria in Trastevere, also inscribed *Opus Mini*, appears to be by the same sculptor as well as a Crucifixion in Sta. Balbina, which is strongly reminiscent in execution of the fragments in Sta. Maria Maggiore. On the other hand, the master of Fiesole does not seem to have blazoned his name on the marbles in Rome which are undoubtedly his. On one of these only have I found a signature, and there it is quite difficult to find, modestly hidden in a corner. Under these circumstances, I was not surprised to find that Professor Venturi, in the new volume of his monumental work on the history of Italian art, boldly assigns the sculptures of Sta. Maria Maggiore to Mino del Reame, an artist who had studied the Tuscan Mino, and was a rival of sufficient importance to be

styled the Mino of Naples, and to sign that name on his own work without any compunction as to infringement of copyright.

In considering Mino da Fiesole's own work in Rome, I will first ask you to accompany me to the crypt of St. Peter's, to inspect the fragments of the tomb of Paul II., of Pietro Barbo, the Venetian Mæcenas of art, the builder of the Palazzo Venezia, and the greatest collector of his time. It was erected by his nephew, Marco Barbo, immediately after his death, removed when the old Basilica was pulled down, re-erected in the new St. Peter's in the middle of the sixteenth century, and subsequently once more broken up and relegated in fragments to the crypt, to make room for other sepulchres more appropriate to the baroco splendour of the new edifice. I will first invite your attention to a slide made from an old print, which, I take it, represents the monument as reconstructed in the new church. Its original design was somewhat different, as we learn from a rough sketch in a manuscript now in Berlin. The panels at the side are an addition, as well as the corner pieces squaring the lunette. Over the centre of the lunette there was originally a relief representing the Father in a glory of cherubim, and on either side winged angels, set like crockets, on the arch. Even a superficial examination of the component parts shows that the sculptures are by two different hands. The lower base, in two pieces, is now in the Louvre. Vasari says Mino del Reame worked on some small figures in the base, and these putti between the floral festoons have analogies with some of the ornamentation of the ciborium in Sta. Maria Maggiore. By Mino da Fiesole are the allegorical figures of Faith and Charity in the base, the latter among the finest examples of his art, and signed at the top right-hand corner unostentatiously; the Temptation, now stripped of its two principal ornaments, which were originally mortised on, and were probably objected to by the same spirit which imposed the addition of breeches to the nude figures in Michel Angelo's Last Judgment; the evangelists Luke and John, of the pilaster niches, and the relief of the Last Judgment in the lunette, as well as half the angels on the rim, now scattered about the crypt with their fellows, designed by his associate in the execution of the monument. The rest of the sculptured pieces are evidently all designed by the same artist, and though his personality and style are very individual, and quite unmistakable, his name might have remained a mystery had he not recorded it in large letters under the feet of the figure of Hope; Giovanni Dalmata—John the Dalmatian. So far as I am aware, this inscription is the only record of his name in Rome, and no written document has yet been found which testifies to his presence there. But with the evidence of style afforded by the monument of Paul II., we are able to identify a considerable amount of work by this artist in Rome. The panel in the base displaying the creation of Eve is also his—a remarkable composition, minutely and scrupulously finished—as well as the evangelists Matthew and

Mark, the large panel above the recumbent figure, representing the Resurrection, and the relief of the Almighty in benediction, which once surmounted the lunette. Dalmata also executed the recumbent figure of the Pope, a remarkable piece of portraiture—and a very graceful design for the panel roofing the square niche in which it lies.

As this interesting sculptor, who, like Laurana, Giorgio da Sebenico and others, hailed from the further side of the Adriatic, is little known in this country, I am anxious so far as the lantern will permit, to illustrate his salient characteristics. For contrast's sake, look again at Mino's figure of Charity, typical of his well-known manner. Now compare with this Dalmata's figure of Hope. The most obvious differentiation lies in the mode of treating draperies. The stuff falls in heavy round folds and rock-like masses. There is a realism of execution suggesting direct study from the model, which contrasts with the artificial precision of Mino's method. His angels and female faces have a cast of countenance foreign to Italian art, and perhaps recall the types of his country of origin. For the masculine faces, look once more at the creation of Eve, and the Resurrection, of which I have only the central figure; and again at this figure from one of the niches. In all these faces you will notice a characteristic not very pleasing, a certain obtuse and inert expression. Once appreciated it is unmistakable, and gives a clue to nearly all Dalmata's work in Rome. Another monument, executed about 1479, that of Cardinal Eruli, the fragments of which are also in the crypt, seem to have been entirely Dalmata's.

We have another example of co-operation between Mino and Dalmata in the fragments of a ciborium altar, also executed for Marco Barbo, and set up in the church of St. Marco in 1476, which have now been put together, with barbarous disregard for its original design, in the sacristy of that church. The reliefs however belonged, there is little doubt, to the tabernacle which is inserted between them, as their subjects are typical of the holy elements to be reserved there. That on the left, representing Jacob bringing savoury meat to the blind Isaac, is Dalmata's; that on the right, by Mino, represents Melchisedek giving bread and wine to Abraham.

From St. Marco we pass to the ancient basilica St. Clemente, and, having in mind the characteristics exhibited by Dalmata in the crypt, we examine the tomb of Cardinal Roverella, in the choir, to the right of the high altar. The cardinal died in 1476, just about the time when the tomb of Paul II. was completed, if Vasari is right in assigning two years for its execution. Here again two artists have been at work, one of them undoubtedly Dalmata, but his collaborator is neither of the Minos. He is probably a sculptor who, elsewhere, worked with Mino da Fiesole, or called in Mino to assist him, one Andrea Bregno, to whom I shall come by and by. His share in this monument consists of the fine recumbent portrait figure, the Apostles,

and the little angels at the base. The rest of the monument—the Madonna and attendant angels, the dignified figure of the Father, and the angels at the ends of the sarcophagus holding back the curtains, a Renaissance adaptation of an old Gothic motive—can be assigned to no other hand but Dalmata's. A *Pieta*, evidently his also, a fragment from some monument now broken up, may be seen in the porch of the church of St. Agostino. Herr von Tschudi has pointed out his marked manner in certain portions of the Tebaldi monument in the Church of the Minerva. This prelate died in 1466. It does not follow that the tomb was constructed immediately after his death. We may however assume that Dalmata worked in Rome for about fifteen years. Of his subsequent career some records have been discovered by Fabriczy and others. He was summoned to the court of Matthæus Corvinus, King of Hungary, who ennobled him and gave him a castle in Slavonia. His work may be traced in Venice and Vienna, and finally in Ancona, where the tomb of the beatified Girolamo Gianelli, a work of his later and failing years, was executed as late as 1509.

And now to return for a moment to Mino da Fiesole. Another important work which must have been put in hand about the same time as the tomb of Paul II. is that of Cardinal Forteguerra in Sta. Cecilia. The fragments of this monument, which had been broken up, were only put together again in 1891, and it has suffered from injudicious regilding. The Madonna and Child is his work, as also is the recumbent figure of the fighting cardinal, which is treated in the same manner as his presentation of the Count Ugo in the Badia at Florence. It does not lie on the sarcophagus, but on a sort of shell or bier raised above it. The other figures were probably contributed by Andrea Bregno, the angels in the lunette being characteristic of his manner. In the sacristy of this church there is also a Madonna from Mino's workshop.

A year before Cardinal Forteguerra, died Pietro Riario, Cardinal of San Sisto, the favourite nephew who was called by Sixtus IV. from a Franciscan monastery to become a prince of the church, and who, after a meteoric career of wild excesses, died only two years after his elevation. His monument, one of the most interesting in Rome, is in the Church of the Holy Apostles. The relief of the Madonna and Child appears to be the work of Mino in one of his happiest moods. The saints in the upper niches of the pilasters are also his. One, at any rate, of the lower ones I should attribute to another hand, and the groups on either side of the Madonna have been, I think, with justice given to Andrea Bregno. So, also, the sarcophagus and the recumbent figure have hitherto been. With this attribution, however, I venture to differ. The portrait figure is treated in the same manner as in the Forteguerra monument. The face is strong, highly finished, and polished like ivory. The sarcophagus is the most beautiful example of such work in Rome. The

heads of the sphinxes are reminiscent of those on the Tuornabuoni monument in the Minerva. The little putti between the garlands are masterpieces of decorative workmanship. It was copied some twenty years later for the grave of Cardinal Savelli in the Araceli Church, which comes no doubt from Andrea Bregno's workshop. There was evidently here no question of casting and accurate reproduction. It is a mere copy, with all the essential differences of the two manners. The one is individual and full of charm, the other characterless and mechanical.

And now we have to consider the problem of an artist whose individuality has only recently been disengaged and re-established, though he worked for at least forty years in Rome and perhaps much longer. The name of Master Andrea appears first, chronologically speaking, in an inscription on the old high altar of Sta. Maria del Popolo, erected by Cardinal Roderigo Borgia (Alexander VI.) in 1473, and now preserved in the sacristy. The inscription states that while Andrea was at work on it, fate cut the thread of life of his beloved Marcantonio at the age of seven, owing to the carelessness of his attendants. The boy had probably climbed the scaffolding to see his father's work, and fell from the top, therefore the inscription on the altar. It is clear that the sculptor of this altar was also the author of two tabernacles in the first chapel to the left of the door, and it is also clear, though I cannot to-night explain all the chain of proof, that he is identical with the Andrea Mediolanensis, of Milan, who inscribed his name on the great Piccolomini altar in the cathedral at Siena. Now with regard to this altar we have a letter from the illustrious Platina to Lorenzo di Medici, recommending to him one Andrea, an eminent sculptor, his neighbour in Rome and intimate friend, who wished to bring marble through Tuscan territory from Liguria for an altar on account of Cardinal Piccolomini. In a contract which Piccolomini made in 1501 with the great Michel Angelo for the completion of this altar with certain additional figures, Andrea is again referred to. Milanese, who edited this document, obscured his individuality by adding the name Fusina. The Lombard sculptor, Andrea Fusinæ, is, however, a very different person, and quite rudimentary powers of critical observation suffice to show that our Andrea has nothing to do with him. Once more our Andrea is mentioned in the rhyming chronicle of Giovanni Santi, Raphael's father, who speaks of "the eminent Andrea Verrocchio, and that other Andrea who at Rome is such a master of composition."

Who, then, was this master who stood so high in the esteem of contemporaries? The clue is afforded by a monument, long lost sight of in a corridor leading from the church of the Minerva, which is now incorporated in the Ministry of Public Instruction. The inscription tells us that it was erected to the memory of Andrea Bregno, of Osteno, in the territory of Como, a very celebrated sculptor, known to contemporaries by the name of Polycleetus, who first restored to use

and application the lost art of chiselling (*artem celandi*) as practised by the ancients. He lived 85 years. It was erected in 1506 by Bartolomeo Bolli, controller of the Papal records, and Catherine, his wife, Bregno's wife, that is ; for another document discovered by Bertolotti shows that Andrea di Brignonibus, a sculptor of the diocese of Milan, who lived in the Rione Trevi, made a testamentary disposition on the 8th of July, 1503, of a part of his house to his wife, Catherine. This will thus refers to him as of Milan, though he was born at Como, where the Bregno family produced at least four members who became eminent in the story of art. Round the circular niche, containing the bust, and in the pilasters are shown all the tools and implements of his craft. The words "*artem celandi*," the art of chasing or chiselling, which he is said to have revived, have, I believe, a special meaning, and the phrase is not merely complimentary. A strict latinist would maintain that *celare* should signify the chasing of metals, but we find in the latinity of this period such phrases as *celatorius marmorum*. I have found no record in any of the Papal registers of Andrea Bregno as a goldsmith, and had he revived any lost art in working precious metals, he could hardly have failed to receive commissions from Paul II. or Sixtus IV. He is only referred to as *scultor marmorarius* or *statuarius celeberrimus*, and I believe the meaning to be that he was the first in Rome to reintroduce the art of decorative marble-cutting on incised surfaces, after the manner of the ancients. A study of his works shows that he was indeed the chief initiator in Rome of renaissance surface decoration, ornamenting pilaster and frame with delicate candelabra and foliage and armorial design—and I believe it can fairly be established that it was after his acquaintance with and association with the Roman Andrea that Mino da Fiesole applied such elaboration of ornament to his own monumental work.

Andrea was born about 1421, and we have no evidence as to how the first forty years of his life were spent. If I may hazard a conjecture, it would be that he worked with Paolo Romano and Isaiah. There is no time to-night to examine the evidence in support of this theory. It must suffice to say that his first tombs follow the model established by the latter, and I should attribute to him the restorations to the tomb of Eugene IV., or, shall we say, the portions which are evidently not by Isaiah himself. Andrea's angels have their prototype in those of Paolo Romano in the shrine of St. Andrew in the crypt, and a third lunette, differing in some respects from the other two, may be his work. In his native Osteno are two tabernacles closely resembling those of St. Maria del Popolo ; they are dated 1464, and were probably executed in Rome. For his earlier work here we must go to San Pietro in Vincoli, where the monument of Cardinal de Cusa is no doubt rightly attributed to him, and to the church of Aracœli, where the grave of Cardinal d'Albret (Lebretto) demands particular attention. Lebretto died in 1465, and if this

was one of the first independent commissions obtained by the Lombard artist, it might account for the exceptional care and finish bestowed on the figures, before great press of work compelled him to leave execution to pupils. The St. Peter and St. Paul, rendered on the lines laid rudely down by Paolo Romano, fixed the type ever afterwards followed in Rome. The reliefs in the niches are among the most beautiful works of the Renaissance in the city. The St. Michael is a little masterpiece, both of design and execution. The recumbent figure is evidently an excellent piece of portraiture. At present there is little ornamentation. Eight or ten years later Bregno had learned to cover base and pilaster and architrave with every elaboration of arabesque and floral design.

In the Salviati chapel of St. Gregorio there is an altar erected by an abbot of the church in 1469, the manner of which suggests Lombard traditions. It is now generally attributed to Bregno, and a comparison of its details with those of the great altar of the Madonna della Quercia at Viterbo, carried out by him some twenty years later, is, I think, convincing. Next in chronological order comes the altar of Sta. Maria del Popolo, executed for Roderigo Borgia in 1473, and about the same time are the tombs of Pietro Riario and of Cardinal Forteguerra, where I have little doubt Bregno was the artist who collaborated with Mino da Fiesole, as well as the Roverella grave in St. Clemente, where he worked in combination with Giovanni Dalmata. The scheme of the Lebreto grave was reproduced in that of Cardinal Alanus in S. Prassede in about 1475, but the two female saints in the pilaster niches seem to be by another sculptor, who is responsible for a good many of the figures in the Roman graves, but who remains for the present unidentified. Some of Bregno's best work in design and portraiture belongs to the years 1478-79. The fine tomb of Cardinal Cocca, of which I have not got a slide, in the Minerva, and the beautiful monument of Cardinal Cristoforo delle Rovere in Sta. Maria del Popolo, are of this period; the Madonna of the lunette in this last, however, once more by Mino da Fiesole. Some of the decorative work in this monument repeats motives found in the Piccolomini altar at Siena. The very dignified figure of the cardinal is a little soft for Mino, and is no doubt rightly attributed to Bregno, of whose manner the angels on either side of the Madonna are typical. This monument was so much admired that it was constantly reproduced. In one instance, in that of Cardinal Ferici, in the cloisters of the Minerva church, the Madonna also appears to be from a design by Mino. Other instances are the tombs of Cardinal Superanzi in the Minerva, of Cardinals Rocca, Pallavicini and Giustiniani in Sta. Maria del Popolo, and that of Cardinal Diego Valdez, who died in 1506, carried out in his lifetime, in Sta. Maria in Monserrato. All these copies are, with the exception of the first, very inferior in execution to the original. There are a great number of other monuments of this period of

active production when the nepotism of the Popes filled Rome with wealthy and ambitious prelates, which are properly traceable to the workshop of Andrea Bregno, but as I am lecturing in London and not in Rome, it is useless to weary you with lists. The latest, perhaps, in which his direct influence may be traced, is the Savelli grave in Aracœli, which cannot be dated before 1495, when Bregno was already a man of 75. In design it is practically a remodelling of the Lebreto grave in the same church, with a lunette superadded above the architrave. The saints below the pilasters have their prototypes in those of the tabernacles of Sta. Maria del Popolo. The sarcophagus is a copy, vastly inferior in workmanship and delicacy to that of the Riario grave in the SS. Apostoli, which I have given my reasons for attributing rather to Mino da Fiesoli's inventive genius than to Andrea. The Madonna and Child of the lunette belong to the type produced by Luigi Capponi of Milan, who, it would seem, succeeded to the direction of the great workshop.

From an inspection of the various monuments to which I have referred, if not from their photographic reproduction, it is possible to derive some idea of Bregno's manner. His recumbent figures are finely executed, suggesting able portraiture, without the strength and delicacy of Mino's. He created the types of saints, in the subordinate parts of these Roman monuments, which were reproduced by numbers of contemporaries and successors, whose work it is not easy to differentiate from his own. They display much skill and facility without strong individuality of style or the impression of a dominant personality. They remind us rather of the art of the ivory carver, complete within its limitations. Above all, Bregno was a master of ornament, and if he lacks great originality he taught many others the rules and methods of a charming art of surface decoration.

The great activity which the nepotism of Sixtus IV. provided for the sculptors bottega, which produced the monuments of the Riarios, the delle Roveres, and the Bassi, was continued through the pontificate of Innocent VIII., to which belong a whole series of kindred pieces. In this and the following reign, moreover, a French prelate, Guillaume de Pérrier, who became very wealthy as auditor of the Rota, erected a whole series of altar-pieces which bear the stamp of the same workshop. From the mass of craftsmen who must have been employed it is difficult to disengage distinct personalities. But I am inclined to think that students of the work of Cristoforo Romano, the son of Isaiah of Pisa, in the Certosa at Pavia, will see in details of not a few of the Roman monuments evidence of a similar hand, and it is legitimate to assume that before going north, in or about 1491, he was one of the numerous band of assistants of Bregno. The name of Jacopo di Andrea of Florence is also preserved to us in a record as the author of the beautiful tomb of Marc Antonio Albertoni in Sta. Maria del Popolo. This is one of the first instances of a somewhat different type of smaller monument which came into use towards the

end of the fifteenth century (1489), and it seems probable that this Jacopo of Florence was, as his appellation suggests, a pupil of Andrea Bregno.

There is, however, an artist of considerable merit and achievement whom we are able to identify, thanks to the preservation of two contracts, and with a brief notice of his work I will conclude. The restoration of Luigi Capponi to his place in the history of art was first due to the critical investigations of Conti Gnoli. But I do not think the full catalogue of his work in Rome has yet been compiled.

The first of these documents is a contract of the year 1485, made with Luigi Capponi of Milan and Giacomo di Domenico della Pietra, for a marble tomb to be made for Brusati, Archbishop of Nicosia, nephew of Cardinal Roverella, by the side of whose tomb it stands in San Clemente. It was to be completed in four months at a cost of 60 ducats. It resembles the monument of Christoforo delle Rovere on a smaller scale. I have only been able to obtain a slide made from an old print in Tosi's collection. I will ask you to notice certain characteristics of the ornamentation. The candelabra of the pilasters, the tazzas and the little flaming lamps, the strings of beads festooned or dependent. These are a sort of hall mark of Capponi. The drapery is academic, the folds are deeply and straightly chiselled, and there is a geometrical correspondence of angles in the sleeves. The second document which has come to light is a contract between a certain Michele Buttaroni and Master Luigi, this time alone, for a marble crucifixion to be made for the church of Sta. Maria delle Grazie. The relief to be enclosed in a frame for an altar. The crucifixion has now been moved to a ward in the hospital of the Consolazione behind the church, and is therefore difficult of access; the frame is still in the sacristy. The reliefs on the pilasters are almost identical with those of the Brusati tomb. Over the pediment are the arms of Innocent VIII. In this contract no mention is made of Giacomo di Domenico, who, we may assume, was only the marble mason. The figure on the cross in the relief is rather inanimate; on the other hand, the figures of the Virgin and St. John on either side are full of expression; the sense of grief in the faces is powerfully rendered, and all the workmanship is good down to the minutest details, the hands especially being rendered with delicate care in execution. I am sorry it has not been possible to procure a photograph of this remarkable piece of work. But the St. John is tolerably well represented, though much less fine in execution, in another relief of which I have a slide. It represents Pope Leo I. kneeling in adoration before the Evangelist, and may be seen in the baptistery of St. John Lateran. The figure of the St. John is here turned to the left instead of to the right, as in the crucifixion, but the whole manner of treating the drapery, and the characteristic corded long hair flowing down over the neck, leave no doubt as to the identity of authorship. You should also notice the flaming lamp suspended

from a nail, holding up festooned garlands, for this also we shall find elsewhere. Now observe the kneeling pope, whose name is inscribed near the feet. The drapery here is treated in quite a different fashion, is undulatory, and natural rather than academic. Characteristic is the facial angle, and the great length from the point of the nose to the chin. Having these points in mind, we go to the church of San Gregorio, and find in a chapel of the transept an altar with a marble front divided into three compartments, illustrating the miracles of St. Gregory. In the compartment on the left, Pope Leo of the Lateran reappears as St. Gregory, and we find throughout all the profile faces of the three reliefs the same curious facial angle and disproportion between the upper and lower portions of the face. The St. Sebastian of this compartment and the San Rocco of the corresponding are very near relations of the St. John of the Lateran relief. There are several other points of analogy, notably the strings of beads which appear on the pilasters attached to the flaming lamps like those of the Lateran relief and the Brusati tomb. They reappear in festoons on the architrave. Under one of the pilasters are the initials M.B. inserted in a kind of cabalistic emblem, under another a wheel with rays, and under a third the Florentine lily.

An explanation of these emblems is afforded by a monument now in the courtyard in front of the church, to which it was removed from the chapel when the church was redecorated. This monument reproduces nearly all the characteristic features of Capponi's work which have been noticed. In it we find again the Florentine lily, the eight-rayed wheel, the armorial bearings of the Bonsi family, and the inscription tells us how Michele Bonsi initiated the idea of erecting the monument in his lifetime, and his brother Antonio, though the elder of the two, afterwards joined in the scheme. From the archives of the Bonsi family in Florence we learn that they erected a chapel in San Gregorio in 1470. Antonio was sent as orator of the republic to Rome in 1498. He was apparently a merchant of brocades, as large purchases of materials from Antonio Bonsi and Co. figure in the coronation accounts of Pius III. Michele had been established already earlier as a banker and agent in Rome. In this monument the recumbent figure is replaced by two portrait busts, inserted in circular niches. It appears to be the first example in Rome of a manner frequently adopted afterwards, which Luigi Capponi no doubt introduced from Lombardy, where we are familiar with it in the *certosa* of Pavia and elsewhere. It is repeated in the tomb of the brothers Antonio and Pietro Pollaiuolo, in San Pietro in Vinculi, which I also attribute to Capponi, as, I think, we may also do the monument of Andrea Bregno, which we saw on the screen a few minutes ago.

Having fixed certain mannerisms of style, and certain specific characteristics of Capponi's treatment of decorative work peculiar to himself, wherever we find these repeated in other examples during the last 20 years or so of the 15th century we may safely assign them to

his workshop. Thus there are several ciboria, or tabernacles for holy oil, which may be catalogued under his name. One of these is in the hospital, close to his remarkable relief of the crucifixion. The most interesting and elaborate of them is in the church of the Quattro Coronati. All his own peculiar paraphernalia of ornament are found here: the hanging lamps, the coruncopia of the Bonsi tomb, the candelabra and the strings of beads. The angels resemble those of the tomb, and the chiselling of the wings shows the identity of workmanship in both. A Virgin and Child over the door of the hospital of the Consolazione, where there is so much of his work, resembles that of the Bonsi tomb so closely that it must also be assigned to him, and from these two Madonnas we are able to identify others in other monuments of Rome, of which, however, it would be unprofitable to trouble you with a list. There is, however, one important attribution which I have been enabled to make, and which is, I believe, entirely new. In the crypt of St. Peter's are many fragments of a tabernacle made in the reign of Innocent VIII. to contain the relic of the Holy Lance. The three sides represented doors guarded by angels. The figure of the Saviour over the central doors reveals the same hand as the crucified figure in the hospital relief, and the angels, though executed by different marble cutters, are all designed by the artist of the ciborium, which I have just shown you. I have only one slide representing a pair of these angels, put together with a number of fragments from some other monument, but the type of Capponi's angel is sufficiently marked to leave no doubt that he was the author of the reliquary of the Holy Lance.

In the brief time available it has only been possible to deal with a small proportion of the great quantity of sculpture produced in Rome during the second half of the fifteenth century. If it was not all of the highest quality, it has at any rate the interest of remarkable decorative workmanship, and includes a great number of first-rate portraits. With the close of the century the sense of restraint gave way to extravagance and over-elaboration, and the devotional and reverential quality disappeared. I should like, in conclusion, to show you one last example, which has a particular interest for Englishmen, because it is the monument of Cardinal Bainbridge, Archbishop of York: a most truculent and quarrelsome prelate, whose ungovernable temper brought him to an untimely end, being poisoned by a servant he had castigated. An unknown artist has, however, made his effigy immortal in the little church of the English college, where he lies an embodiment of saintly repose.

[R. R.]

GENERAL MONTHLY MEETING,

Monday, June 6, 1910.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. D.C.L. F.R.S.,
President, in the Chair.

Martin Onslow Forster, Esq., D.Sc. F.R.S.

Harry Grindell-Matthews, Esq.

Percival Lowell, Esq., A.B. LL.D.

Maurice Rüffer, Esq.

Lady Truscott,

were elected Members of the Royal Institution.

The Honorary Secretary announced the decease of Sir William Huggins, a Manager and Vice-President, on May 12 ; of Professor Stanislao Cannizzaro, on May 9, and Professor George F. Barker, on June 6, 1910, Honorary Members of the Royal Institution ; and the following Resolutions, passed by the Managers at their Meeting held this day, were read and unanimously adopted :—

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution and by Science in the decease of their most distinguished Member and Vice-President, Sir William Huggins, Order of Merit, K.C.B. D.C.L. LL.D. D.Sc. Founder of the Science of Astrophysics ; Past President of the Royal Society, the Royal Astronomical Society, and the British Association ; and a Member of all the most important Foreign Academies.

Sir William Huggins was a pioneer in the use of Spectroscopy and Photography to Astronomical Research, and made many most important investigations proving the constitution of comets, stars and nebulae. He initiated what is known as the determination of the motion of Stars in the Line of Sight by optical methods, which is universally recognised as one of the most brilliant and far-reaching discoveries of Modern Astronomy. In his studies he was aided by Lady Huggins, Hon.F.R.A.S., and they have conjointly published the results of the investigations in many celebrated works, namely, "Atlas of Representative Stellar Spectra" (1899) (for this work the Authors were awarded the Actonian Prize of the Royal Institution), "The Royal Society, or Science in the State and in the Schools" (1906), and "Scientific Papers" (1909).

Sir William Huggins delivered six Friday Evening Discourses, the titles being as follows :—"The Physical and Chemical Constitution of the Fixed Stars" (1865), "Further Results of Spectrum Analysis as applied to the Heavenly Bodies" (1869), "The Photographic Spectra of the Stars" (1880), "Comets" (1882), "The Solar Corona" (1885), "The New Star in Auriga" (1892), and also the Tyndall Lectures on "The Instruments and Methods of Spectroscopic Astronomy" (1895).

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with Lady Huggins in her bereavement.

Resolved, That the Managers of the Royal Institution desire to record their

sense of the loss sustained by the Institution and by Science in the decease of their Honorary Member, Professor Stanislao Cannizzaro, Hon.F.R.S., Hon.F.C.S., Senator of the Kingdom of Italy, Officier de la Legion d'Honneur, Corresponding Member of the Academy of Science, Paris, Professor of Chemistry at Rome.

Professor Cannizzaro delivered the Faraday Lecture so long ago as 1872, which he entitled "Considerations on some Points of the Theoretic Teaching of Chemistry"; and on the occasion of the Faraday Centenary Commemoration in 1891 he was elected an Honorary Member of the Royal Institution.

His monumental work on the classification of the Atomic Weights of the Elements by means of Specific Heats has been universally recognised as marking an epoch in the development of Chemical Science, and for which generalisation his name will always be associated with that of Avogadro.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with the family in their bereavement.

Resolved, That the Managers of the Royal Institution desire to record their sense of the loss sustained by the Institution and by Science in the decease of their distinguished Honorary Member, Professor George Frederick Barker, Sc.D. (Pennsylvania), LL.D. (Allegheny and McGill), Emeritus Professor of Physics at the University of Pennsylvania, Commander of the French Legion of Honour, Past President of the American Association for the Advancement of Science and of the American Chemical Society, Member of the National Academy of Science; celebrated for his various Scientific Papers and Reports, the Author of an original Text-Book on Physics, and long one of the Editors of the 'American Journal of Science.'

He was elected an Honorary Member on the occasion of the Commemoration of the Centenary of the Royal Institution.

The Managers desire to offer, on behalf of the Members of the Royal Institution, the expression of their most sincere sympathy with Mrs. Barker and the family in their bereavement.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

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WEEKLY EVENING MEETING,

Friday, June 10, 1910.

HIS GRACE THE DUKE OF NORTHUMBERLAND, K.G. P.C. D.C.L.
LL.D. F.R.S., President, in the Chair.

DR. H. DESLANDRES, D.Sc., Membre de l'Institut,
Directeur de l'Observatoire de Meudon.

The Progressive Disclosure of the Entire Atmosphere of the Sun.

[In French.]

LE soleil auquel est consacrée cette conférence, est un magnifique sujet d'études. Tous les hommes sentent plus ou moins clairement que les destinées terrestres sont liées étroitement à celles du soleil, et qu'il est nécessaire de reconnaître sa nature intime, son rayonnement total, ses variations, en un mot son action précise et complète sur notre globe. Notre dépendance vis à vis du soleil est absolue, et récemment, elle a été résumée d'une manière simple par un homme politique français, maintenant ministre des finances, auquel je demandais un crédit spécial pour l'observatoire de Meudon que je dirige, et pour les recherches solaires. Il refusait d'abord, en alléguant l'accroissement continu des dépenses publiques. Puis, comme j'insistais, il s'écria : "Vous avez raison, le soleil est *notre maître* à tous ; il est impossible que nous ne fassions pas quelque chose." C'est ainsi que l'observatoire de Meudon a pu joindre à son budget ordinaire une somme supplémentaire, certes peu élevée, mais qui est arrivée au moment opportun, et nous a beaucoup aidés dans les recherches que je vous présente aujourd'hui.

L'étude moderne du soleil exige en effet des installations coûteuses, des appareils compliqués et un personnel spécial apte aussi bien aux observations physiques qu'aux observations astronomiques. Or le soleil luit pour tout le monde, et mûrit toutes les moissons ; et, à priori, il semble naturel que tous les hommes de la planète apportent leur concours aux recherches solaires. Partant de cette idée, j'ai proposé, il y a quelques années, à la Société astronomique de France une taxe spéciale et générale pour le soleil—et d'ailleurs très-minime. Si chaque français, ai-je remarqué, donnait par an un sou, un simple sou pour le soleil, la somme totale serait encore élevée ; elle permettrait d'assurer l'enregistrement continu du soleil et de ses variations, non encore réalisé, et donc une connaissance plus approfondie de l'astre. Mais les taxes nouvelles sont toujours plus nombreuses, et celle-là, bien que très faible et très légitime, serait

probablement écartée. D'ailleurs, il faut bien le dire, l'homme civilisé actuel, le citoyen surtout, s'occupent peu du soleil ; ils le regardent moins que l'homme primitif et le sauvage qui n'ont ni montre ni almanach. La réalisation de cette idée est réservée pour la cité future, et pour un état social plus parfait que le nôtre.

Le recours au gouvernement, à la collectivité, est une habitude française. Il vaut mieux comme en Angleterre, faire appel à l'initiative privée, à l'initiative d'hommes éclairés et généreux. C'est ainsi qu'a été fondée la Royal Institution, qui a vu éclore tant de belles découvertes et tant de savants illustres. Ce bel exemple doit être proposé à tous, et on sait qu'il a été largement suivi en Amérique où les plus grands observatoires, et surtout ceux consacrés au soleil, sont dus à de simples particuliers.

En fait, dans les cinquante dernières années, grâce à de grandes découvertes, grâce à l'appui des gouvernements et des Mécènes, l'étude du soleil a pris un développement considérable. Les astronomes ont pu lui donner peu à peu une organisation sérieuse et permanente et même l'étendre à l'atmosphère entière de l'astre, jusqu'alors inaccessible.

La découverte principale sur le soleil est la variation périodique de ses taches noires, variations que subissent aussi les facules brillantes de la surface et l'atmosphère entière très étendue. Le soleil entier a une grande oscillation générale ; et, fait plus curieux encore, cette oscillation s'étend à la terre et, tout au moins, à ses éléments magnétiques.

L'extension du phénomène solaire à la terre a une importance capitale ; elle implique presque nécessairement une action spéciale, nouvelle, exercée par le soleil sur notre globe ; elle est la cause première de la grande faveur actuelle des recherches solaires. Après la découverte de Sabine et Lamont sur l'accord de nos variations magnétiques avec le soleil, la science anglaise a accordé la plus grande attention aux taches du soleil ; et *la première* elle a organisé l'enregistrement photographique des taches et des éléments magnétiques sur plusieurs points de globe, et la concentration de tous ces documents dans un même observatoire qui les relève avec précision. Les travaux d'Ellis et de Maunder sur la question sont bien connus et il convient aussi de rappeler ceux de Lockyer et de Shuster, qui ont reconnu récemment dans les variations des taches des périodes plus grandes et plus petites que la période principale de 11 années.

L'action exercée par le soleil sur la terre est attribuée généralement aux taches ; mais elle peut avoir son origine dans l'atmosphère solaire qui a les mêmes variations ; d'où la nécessité d'étudier et de relever avec soin cette atmosphère. Or, depuis près de 20 ans, je me suis attaché à la reconnaissance de l'atmosphère entière du soleil, et je vous présente aujourd'hui les résultats les plus récents, qui ont mis au jour les couches supérieures de cette atmosphère jusqu'ici inexplorées.

Atmosphère des éclipses—au bord solaire extérieur.

L'atmosphère du soleil s'est montrée à l'homme pour la première fois dans les éclipses totales, au bord solaire extérieur. Elle forme alors l'anneau lumineux qui se détache sur la fond du ciel devenu noir, en entourant le disque lunaire, également noir. Elle comprend deux parties distinctes, à partir de la lune et du bord solaire : la *chromosphère* mince et brillante, de couleur rose, de laquelle se détachent les proéminences également roses, et la *couronne*, plus pâle mais très étendue. Dans ce qui va suivre, il sera question surtout de la chromosphère et des proéminences.

En temps ordinaire, l'anneau lumineux des éclipses est caché par l'illumination beaucoup plus vive de notre ciel. L'écran qui le masque est lumineux : pour l'écarter, l'astronome anglais Sir Norman Lockyer, a en le premier, en 1866, l'idée de recourir au spectre, en admettant, ce qui était probable, que l'atmosphère solaire fût gazeuse. C'était *une idée de génie*, qui depuis a fait son chemin.

L'éclipse de 1868 montre en effet que les proéminences roses sont constituées en grande partie par l'hydrogène incandescent qui émet les radiations déjà reconnues dans le laboratoire sous l'influence de l'étincelle électrique, et en particulier une raie rouge intense appelée H_{α} . Et, après l'éclipse, Janssen aux Indes, Lockyer en Angleterre, avec le spectroscope et la raie rouge, retrouvent les proéminences et la chromosphère des éclipses. Ce résultat a excité un enthousiasme légitime ; car la méthode, à la fois simple et féconde, est employée depuis 40 ans à la reconnaissance journalière de la chromosphère, des positions et des formes des proéminences. Cette étude est même plus captivante que celle des taches ; car les proéminences ont les formes les plus variées et les changements les plus rapides. Elles apparaissent à toutes les latitudes, et suivent aussi la période undecennale des taches, la durée du maximum étant, il est vrai, plus longue.

L'étude spectrale du bord solaire, poursuivie en temps ordinaire, ou mieux pendant les éclipses, fait aussi connaître la composition chimique de la chromosphère, et aussi la hauteur minima de chaque vapeur, estimée par la longueur de la raie correspondante dans le spectre.

D'une manière générale, les vapeurs à faible poids atomique et les gaz légers s'élèvent le plus haut ; tel est le cas de l'hydrogène et de l'hélium. La raie la plus haute dans ces deux gaz est la raie rouge H_{α} de l'hydrogène, les autres raies de l'hydrogène ayant des hauteurs et des éclats qui diminuent du rouge à l'ultraviolet.

Mais les plus hautes de toutes sont les raies violettes H et K, très brillantes, qui sont émises par les composés du calcium. Comme le poids atomique et la densité de la vapeur de calcium sont relativement élevées, le fait paraît assez étrange ; il est expliqué simplement, d'après les idées de Lockyer, par une dissociation du calcium dans le

soleil et l'étincelle de nos laboratoires. Les raies H et K, à tous égards exceptionnelles, sont très brillantes au bord solaire, et assurent aisément la photographie des proéminences avec les plaques ordinaires.

D'autre part, les vapeurs lourdes, qui sont de beaucoup les plus nombreuses s'élèvent peu dans l'atmosphère, et ne sont aisément visibles que dans les éclipses. Elles forment la couche basse de la chromosphère, relativement fort brillante, appelée *couche renversante*.

Chromosphère projetée sur le disque, couche moyenne.

Tels sont les résultats principaux de la méthode Lockyer-Janssen. Ils sont assurément remarquables, mais, à certains égards, incomplets. Ils ne s'appliquent qu'à la partie de la chromosphère *extérieure* au bord solaire, et même aux vapeurs légères, et élevées de ce bord. La partie intérieure au bord, ou projetée sur le disque, en projection 50 fois plus étendue, lui échappe. Or cette lacune a été comblée de

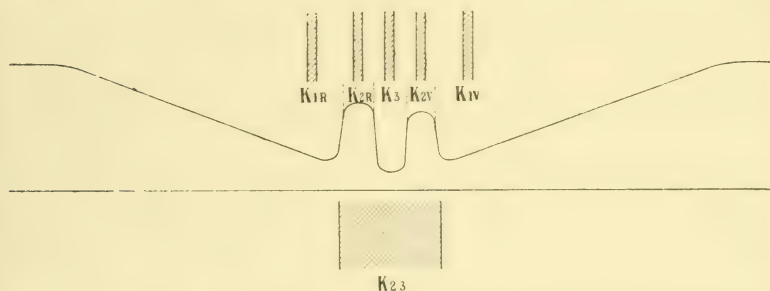


FIG. 1.—Courbe des intensités du spectre solaire à l'emplacement de la large raie noire K. On a représenté par des traits avec hachures les positions des fentes des spectrohéliographes.

1892 à 1894 par une méthode *absolument générale*, qui décèle toutes les vapeurs, lourdes ou légères, et leurs couches successives dans la demie-sphère entière tournée vers la terre.

Au bord solaire, les raies des vapeurs se détachent brillantes sur le spectre continu de notre ciel ; mais, sur le disque, ces raies sont noires, comme on sait, et le spectre continu qui leur sert de fond est celui du soleil lui-même et est beaucoup plus intense. A priori la difficulté paraît beaucoup plus grande.

Or les raies H et K du calcium présentent une exception à cette règle, et le fait a été annoncé simultanément en février 1892 par Hale et Deslandres. Ces raies noires sont très larges et même les plus larges du spectre solaire ; mais, aux points de la surface où *est une facule*, elles sont renversées, autrement dit elles offrent en leur

centre une raie brillante qui même est double et se détache sur la large raie noire aussi bien que la raie des proéminences au bord extérieur. (Voir la Fig. 1, qui montre la raie K et ses composantes K_{1V} , K_{2V} , K_3 , K_{2R} , K_{1R} .)

Le résultat à été obtenu par Hale avec un spectrohéliographe, appareil nouveau, assez complexe, qui isole une radiation avec une seconde fente, et, par le mouvement de cette fente lumineuse, fournit une image monochromatique de l'astre. De mon côté, j'ai employé le simple spectrographe ordinaire et des sections successives, mais en préconisant l'emploi du spectrohéliographe.

Cependant les deux observateurs étaient en désaccord sur un point capital. Hale plaçait les vapeurs ainsi décelées dans la facule même, sous la surface ; je les plaçais au contraire au-dessus dans

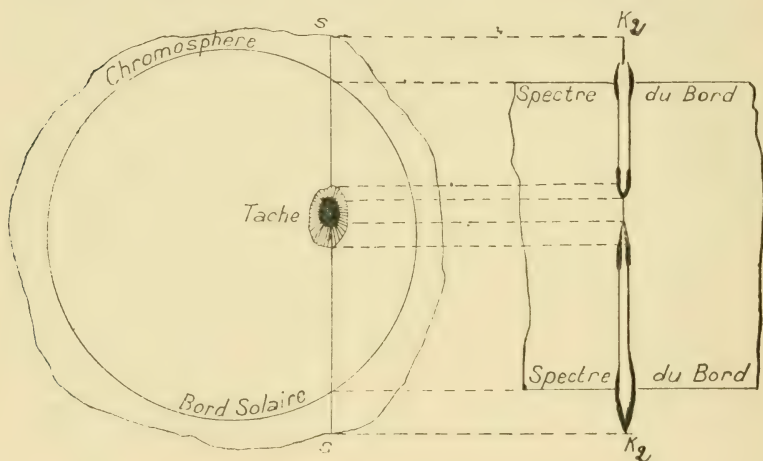


FIG. 2 (schématique).—ss, section faite par la fente du spectroscopie dans le soleil dont la chromosphère et la tache sont très agrandies ; K_2 raie brillante, attribuée aux vapeurs du calcium, qui apparaît au milieu de la large raie noire K du spectre normal ; elle est simple et fine au-dessus des taches et à la partie supérieure de la chromosphère, et double sur les autres points, étant alors divisée en deux par la raie noire centrale K_3 .

l'atmosphère même. Or le spectrographe ordinaire, qui réunit tous les éléments de la question, permet de la résoudre ; il est, à ce point de vue, supérieur au spectrohéliographe.

La raie double K_2 est brillante non seulement sur les facules, mais sur tous les autres points du disque où elle est, il est vrai, plus faible et plus difficile à distinguer. De plus, au bord, la raie brillante double K_2 , au bord intérieur, est toujours nette en ce point, et est prolongée à l'extérieur par une raie brillante double identique. (Voir

la Fig. 2 ci-contre, figure schématique, qui montre bien l'aspect de la raie double K_2 , au bord du soleil et aussi sur une tache.)

Comme la raie K_2 extérieure au bord représente par définition la chromosphère, la conclusion est la suivante : *L'image de la raie K_2 avec le spectrohéliographe représente la chromosphère entière de l'astre projetée sur le disque.*

D'ailleurs les images du calcium faites à Paris en 1894 et qui sont les premières images complètes, montrent des plages faculaires brillantes plus larges que celles de la surface, et aussi les parties brillantes plus petites appelées maintenant *floculi*, qui sont présentes aussi bien aux pôles qu'à l'équateur—j'ai vérifié la présence des *floculi* aux pôles dans les années de minimum et pendant la période undecennale tout entière.

La raie brillante K_2 reste double au bord extérieur jusqu'à 4' ou 5' d'arc, et, comme la chromosphère au bord est haute de 10'', on peut dire que cette image représente la chromosphère moyenne.

En résumé, si le premier spectrohéliographe ayant donné des résultats a été réalisé en Amérique, c'est en France qu'on a reconnu pour la première fois la chromosphère entière du soleil.

Chromosphère basse.

Mais on peut aller plus loin. En 1893, j'ai annoncé que l'isolement d'une raie noire ordinaire avec le spectrohéliographe donnerait l'image même de la vapeur correspondante ; et, en 1894, j'ai isolé avec le petit spectrohéliographe de faible dispersion, organisé à Paris, les bords dégradés de la raie K, appelées K_{1R} et K_{1V} , et les raies noires voisines le plus larges de l'aluminium, du fer et du carbone. L'image obtenue diffère de celle de K_2 ; les taches masquées par fois avec K_2 ont toujours leur ombre et pénombre bien nettes, et les plages faculaires sont brillantes au centre comme au bord, mais moins larges que dans l'image K_2 . En fait, cette image nouvelle est intermédiaire entre l'image de la surface et celle de la couche moyenne chromosphérique K_2 . Elle représente l'image de la couche renversante entière qui serait obtenue ainsi pour la première fois.

J'ai ajouté qu'une dispersion plus forte permettrait d'isoler les raies plus fines qui sont les plus nombreuses, et, en particulier, la petite raie noire centrale K_3 , entre les deux composantes K_2 . Cette raie K_3 correspond à la couche supérieure de la chromosphère. La méthode s'annonce ainsi comme absolument générale ; elle fournit l'image de toutes les vapeurs solaires, et aussi l'image de leurs couches successives superposées, au moins lorsque la raie est divisible en parties distinctes, ainsi que la large raie K.

Or le nombre des raies solaires s'élève à 20,000 ; et, d'après Jewell, toutes les raies solaires offrent plus ou moins la constitution spéciale de cette raie typique du calcium. Le champ nouveau offert à l'investigation s'annonce comme extrêmement étendu.

*Recherches ultérieures. Grand Spectrohéliographe
d'un type nouveau.*

Le programme de recherches, indiqué en 1894, est donc extrêmement vaste. Il a été appliqué en partie dans les années suivantes, et les progrès ont été réels si non très rapides.

En 1903, Hale et Ellermann, à l'observatoire Yerkes, reprennent l'étude des raies noires, avec un spectrohéliographe plus dispersif, et la poursuivent à partir de 1906 au Mont Wilson avec des appareils encore plus puissants. Ils ont obtenu de magnifiques images et toute une série de faits nouveaux. Avec les raies de la couche renversante, les résultats sont à peu près les mêmes que ceux de 1894 ; mais les raies de l'hydrogène, et tout récemment la raie H_{α} ont montré des phénomènes nouveaux, très curieux, dont il sera question avec détails un peu plus loin.

Cependant la dispersion employée par eux est seulement moyenne ; s'ils ont isolé un nombre de raies bien plus grand qu'en 1894, ils n'ont pas isolé les raies fines ; et même dans chaque cas, ils ont isolé la raie entière, ils n'ont pas séparé les parties distinctes de la raie et donc les couches successives de la vapeur. Leur image est un mélange de plusieurs images distinctes et de plusieurs couches.

Je me suis proposé de combler cette lacune, et de poursuivre jusqu'au bout le programme de 1894, en isolant nettement les couches supérieures non encore décelées. Devenu directeur de l'observatoire de Meudon en 1907, j'ai pu diriger de ce côté les ressources de l'observatoire, et, d'autre part le crédit extraordinaire signalé plus haut, nous a été fort utile. Bref, il a été possible de construire un grand spectrohéliographe, aussi dispersif que le grand spectrographe de Rowland et un grand bâtiment spécial capable de le contenir.

La bâtiment comprend une grande pièce de 22 m. sur 6 m. ; son toit est en pierre et terre, ce qui assure la constance de la température à l'intérieur. Il reçoit la lumière solaire d'un cœlostate placé au sud, constitué avec de vieux appareils du passage de Vénus, et d'un objectif ancien de 0.25 m. d'ouverture et 4 m. de distance focale. Ces pièces, qui sont médiocres, ont été utilisées par raison d'économie. Le spectrohéliographe, d'autre part, est d'un type nouveau, et offre plusieurs particularités intéressantes. Il est assez compliqué, au moins sur le dessin, car il comprend en réalité quatre spectrohéliographes différents réunis autour d'un même collimateur. Le premier est à trois prismes et à deux fentes, avec une chambre de 3 m., et une image du soleil de 85 mm. : le second est à réseau et à deux fentes avec une chambre de même longueur. Le troisième est une disposition différente des deux précédents. Enfin le quatrième, le plus puissant, est à trois fentes, à prismes ou à réseau. Il comprend un premier spectrographe avec chambre de 7 m., ainsi que dans l'appareil classique de Rowland, ce qui permet d'isoler des raies très fines. Mais

l'image solaire exigerait une pose trop longue ; on la reprend avec un second spectrographe qui le diminue au degré voulu, et élimine la lumière diffuse intérieure. Le soleil final a un diamètre qui peut être quelconque, et, grâce à certaines dispositions spéciales, *il est entier*, ce qui n'est pas réalisé dans les autres spectrohéliographes de grande dispersion. Les diamètres habituels sont 6 cm. et 4 cm.

L'appareil, avec ses deux spectrographes, a une longueur totale de 14 m., et, dans ces conditions, reste immobile. Il est même le premier spectrohéliographe dont toutes les parties soient fixes, la plaque étant mise à part. Les pièces mobiles sont la plaque photographique et l'objectif astronomique, qui sont mis en mouvement à la vitesse voulue par des moteurs électriques synchrones et des transformateurs de vitesse spéciaux.

La concordance des mouvements est assurée par des moyens électriques, indépendants de la distance, et le dispositif est présenté comme une solution générale du spectrohéliographe. Chacun des quatre spectrohéliographes a ses avantages particuliers, et le passage de l'un à l'autre se fait en quelques minutes. L'observateur a ainsi à sa disposition des moyens d'investigation variés. D'une manière générale, les spectrohéliographes à deux fentes de 3 m. donnent une image plus grande et plus riche en détails. Le grand appareil de 14 m. à trois fentes, donne, avec une pose plus longue, une image plus petite, mais beaucoup plus pure ; il permet d'isoler des raies plus fines.

Les recherches avec cet appareil ont été poursuivies avec un jeune astronome de l'observatoire, M. d'Azambuja, dont le nom est associé au mien.

Révélation de la couche supérieure K_3 du Calcium.

En 1908 nous avons pu isoler la petite raie noire centrale K_3 du calcium, et donc la couche supérieure de la vapeur. La Fig. 1 qui montre la raie K et ses composantes permet de bien juger le progrès réalisé.

Jusqu'alors les spectrohéliographes employés isolaient en même temps l'ensemble des deux composantes brillantes de K_2 , qui comprennent la raie K_3 , avec une fente de $\frac{9}{100}$ d'Angström. L'image, appelée par nous image K_{23} , était un mélange des couches K_2 et K_3 avec une prédominance de la couche K_2 , beaucoup plus brillante ; la couche supérieure K_3 était masquée. Or, avec le grand spectrohéliographe, nous avons pu isoler facilement avec des fentes de $\frac{3}{100}$ d'Angström et plus, isoler soit la raie K_2 , soit l'une des composantes de K_3 , et avoir ainsi des images de chaque couche bien pures et exemptes de toute lumière étrangère. Les fentes correspondantes sont indiquées sur la Fig. 1 par des traits avec hachures.

La vapeur de calcium qui au bord extérieur, s'élève plus que toutes les autres, présente ainsi dans l'atmosphère trois couches distinctes

superposées. Si on ajoute la surface, on a quatre couches, qu'il est intéressant de comparer.

Lorsqu'on s'élève à partir de la surface, les facules on plages

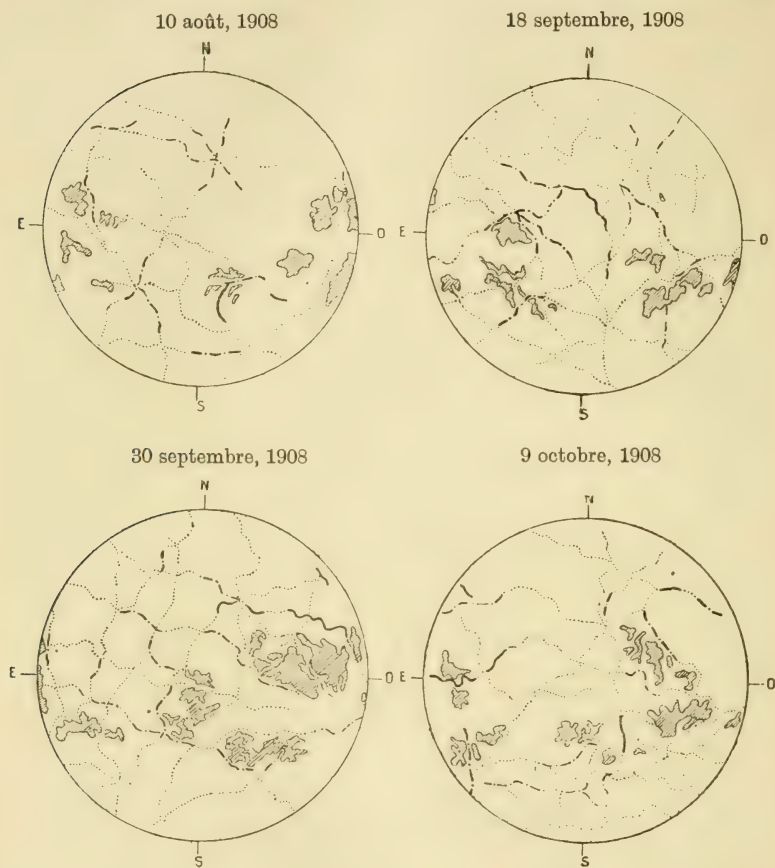


FIG. 3.—Réseau d'alignements relevé dans la couche supérieure de l'atmosphère solaire. Les traits noirs pleins correspondent aux alignements noirs, continus et très nets, appelés *filaments*; les traits discontinus aux alignements similaires moins nets, et les traits pointillés aux alignements encore moins visibles et parfois discontinus. Les parties hachées sont les plages brillantes faculaires les plus larges.

brillantes de cette surface augmentent progressivement en étendue et en éclat relatif. Les flocculi moyens augmentent aussi, lorsque les petits disparaissent ou sont à peine visibles. Il en résulte un aspect

particulier de la couche K_3 qui à première vue se distingue de la couche K_2 , photographiée depuis 1892. (Voir les deux images K_3 et K_2 , du 18 septembre 1908.) J'ajoute que le réseau spécial de flocculi, appelé par moi en 1894 *réseau chromosphérique*, et formé souvent, sur une étendue notable, de polygones juxtaposés par leurs côtés et leurs sommets, est en général plus net dans la couche supérieure.

D'autre part les taches noires, qui sont le caractère principal de la surface, diminuent progressivement, lorsqu'on s'élève et même disparaissent.

Par contre apparaissent des lignes noires, invisibles dans les couches basses, lignes souvent très longues et appelées par moi *filaments*. En général le filament est prolongé de chaque côté jusqu'au bord par d'autres lignes similaires, moins noires, moins nettes, appelées *alignements*. L'ensemble des filaments et alignements forme un véritable réseau sur le disque de soleil. Les filaments et les alignements sont un phénomène nouveau, caractéristique des couches supérieures.

Le filament a la même importance que la tache de la surface ; il persiste, comme elle, pendant plusieurs rotations et, comme elle aussi, il est le siège de perturbations spéciale, et est accompagné de proéminences.

Dans une première étude j'ai assimilé les taches aux dépressions ou cyclones de notre atmosphère, et les filaments aux anti-cyclones ; mais je reviendrai plus loin sur ce rapprochement, qui sera développé.

Révélation de la couche supérieure de l'hydrogène.

L'année suivante, en 1909, nous avons, d'Azambuja et moi, étudié avec les mêmes appareils les raies de l'hydrogène et surtout la raie rouge H_α . Ces raies ont été isolées déjà avec le spectrohéliographe par Hale et Ellermann, qui ont obtenu des résultats fort curieux. En 1903 ils ont reconnu que, avec H_β , H_γ , H_δ , les plages faculaires ne sont plus brillantes par rapport au fond, comme avec le calcium, mais sont souvent noires au contraire. Avec H_α , isolé en 1908, on a en plus tout autour des taches des séries de petites lignes, qui donnent parfois l'impression nette d'un tourbillon, et que Hale a décrites ici même dans une conférence spéciale. De plus ces images de H_α sont magnifiques et très riches en fins détails.

Cependant ces images américaines de H_α sont obtenues par l'isolement de la raie entière, et j'ai annoncé en 1908 qu'elles devaient être un mélange de deux ou trois images et couches distinctes. En effet, d'après Rowland, la raie H_α est doublement renversée, comme la raie K du calcium, mais plus faiblement. Sa largeur avec les parties dégradées est $1^A 24$, et $0^A 90$ sans ces mêmes parties. Il faut donc s'attendre à des images quelque peu différentes, lorsqu'on isole les différentes parties de la raie.

Or nous avons vérifié nettement ce fait, et même, contrairement

à notre attente, les différences entre les images de l'hydrogène sont relativement plus grandes qu'avec le calcium.

Les résultats exacts sont les suivants :—

Si on isole avec H_α la partie dégradée près des bords, qui correspond à K_1 du calcium à une distance du centre comprise entre $\frac{4.7}{100}$ et $\frac{6.2}{100}$ d'Angström, on a le résultat de 1903, c'est à dire les plages faculaires noires par rapport au fond.

Avec le milieu de chaque moitié, entre les distances $\frac{1.0}{100}$ et $\frac{4.2}{100}$ d'Angström, l'image est toute différente ; elle offre les principaux caractères des images américaines de 1908, et en particulier les groupements de petites lignes qui constituent ce que Hale a appelé les *Solar Vortices*.

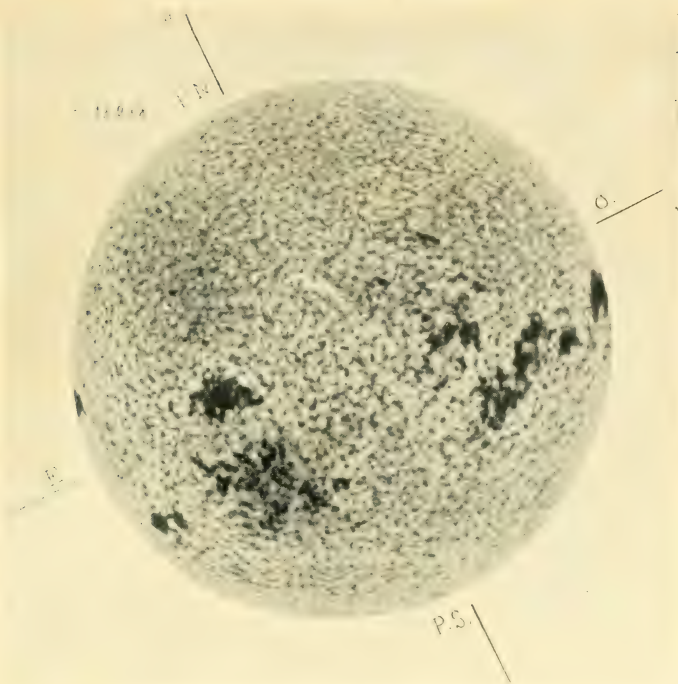
Enfin, avec le centre de la raie, on a une troisième image différente des deux autres, beaucoup plus pâle et simple, qui correspond à la couche supérieure de l'hydrogène.

Or, et ce point est important, cette nouvelle image offre les mêmes filaments noirs que la couche K_3 du calcium. Quant aux plages faculaires, sur cette image, elles ne sont jamais noires mais brillantes ; elles sont moins étendues qu'avec K_3 , et correspondent aux maxima de lumière de ces mêmes plages dans la couche K_3 , maxima qui diffèrent de ceux des couches K_2 et K_1 . Les parties les plus noires et les parties les plus brillantes sont les mêmes. (Voir les images conjuguées de K_3 et de H_α , obtenues le 11 septembre 1909, les 21 mars et 11 avril 1910.)

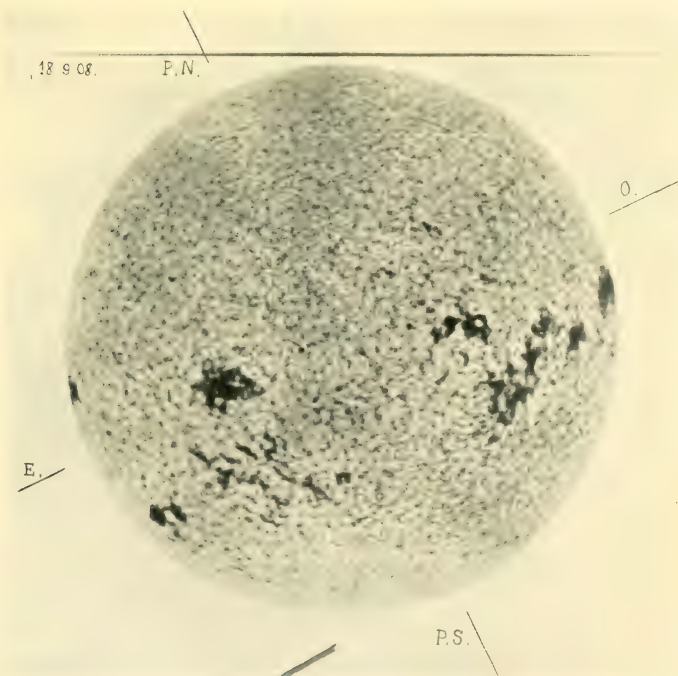
De plus nous avons isolé aussi les différentes parties de la raie bleue H_γ de l'hydrogène, moins élevée dans l'atmosphère que la raie H_α et nous avons obtenu des images qui montrent presque exclusivement les plages faculaires en noir, comme la partie dégradée de la raie rouge H_α , et qui correspondent donc à une couche basse.

Finalement, on est conduit à conclure que l'hydrogène offre, comme le calcium, au moins trois couches distinctes superposées qui sont pour la première fois clairement séparées.

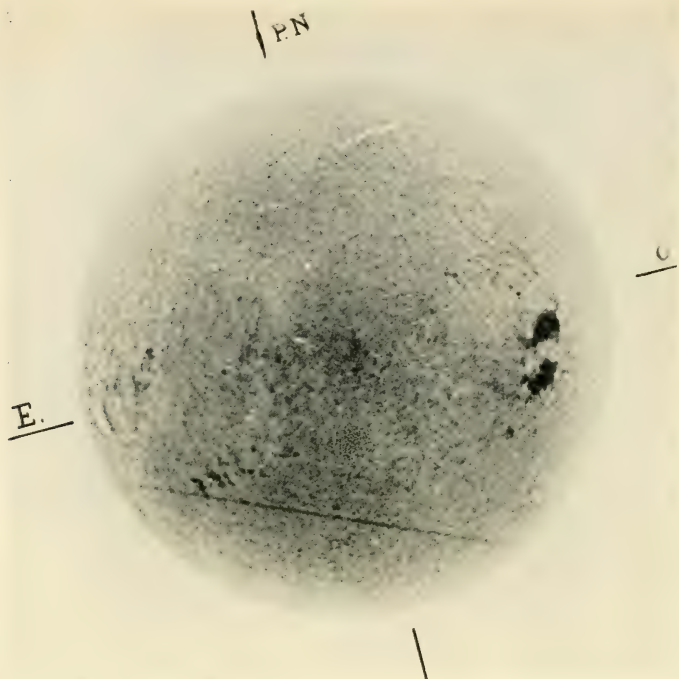
Cependant, dans ce qui précède, j'ai expliqué les différentes parties d'une même raie, et les différentes images par le jeu ordinaire de l'émission et de l'absorption dans les gaz, en admettant, comme il est naturel, que la densité du gaz et la largeur de la raie diminuent lorsqu'on s'élève dans l'atmosphère. Mais on a objecté que la dispersion anormale pouvait jouer aussi un rôle et expliquer au moins en partie les particularités des images. Or, à mon avis, la dispersion anormale, certes, doit intervenir, mais faiblement, et est négligeable dans une première étude. Les raisons sérieuses à l'appui de cette assertion, seraient ici trop longues à développer. D'ailleurs si on a reconnu dans le laboratoire la dispersion anormale avec la raie H_α de l'hydrogène, on ne l'a pas constatée avec les raies du calcium. De plus, comme le centre de la raie ne subit pas la dispersion anormale, l'objection ne s'applique pas aux images de la couche supérieure, qui nous occupent surtout ici.



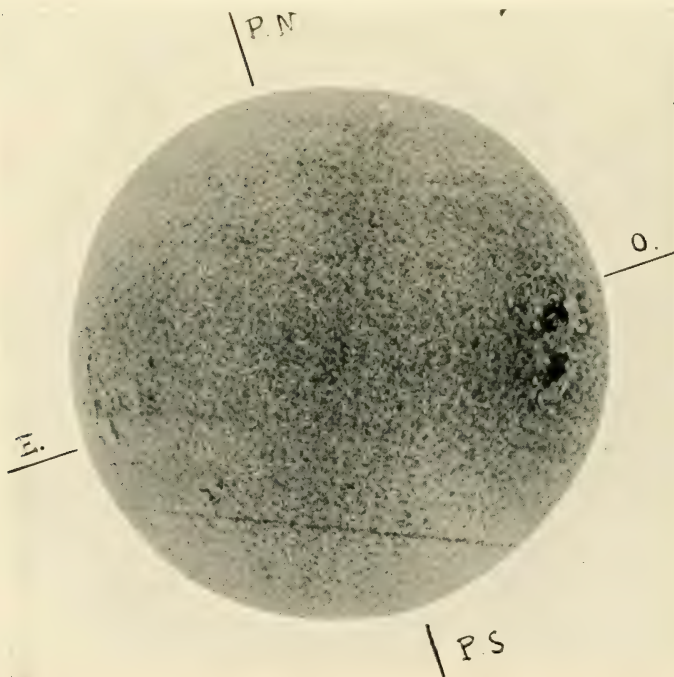
Couche supérieure K₂ du calcium.



Couche moyenne K₂ du calcium.



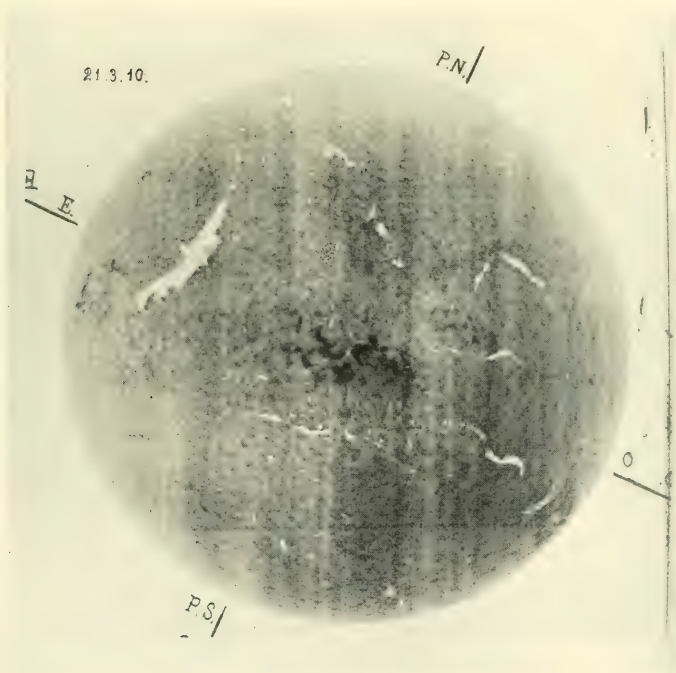
Couche supérieure de l'hydrogène.



Couche moyenne de l'hydrogène.



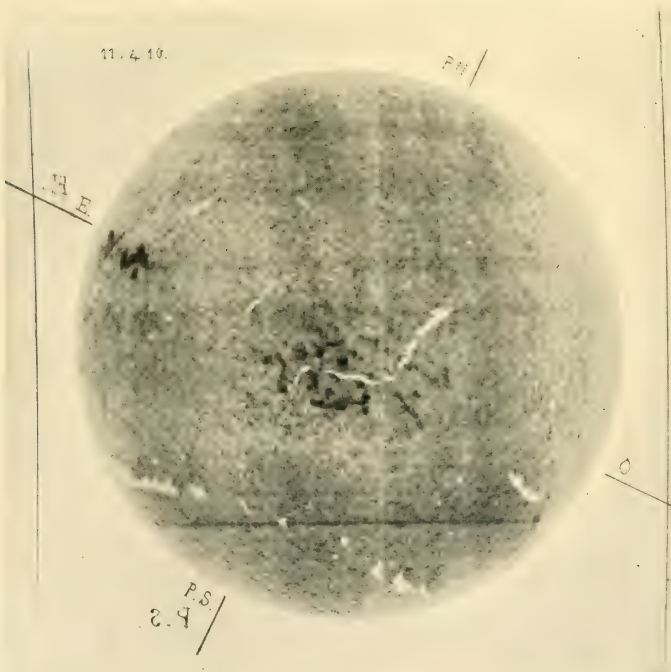
Couche supérieure du calcium.



Couche supérieure de l'hydrogène mélangée à une petite portion de la couche moyenne.



Couche supérieure du calcium.



Couche supérieure de l'hydrogène mélangée à une petite partie de la couche moyenne.

Les filaments noirs qui se retrouvent les mêmes avec le calcium et l'hydrogène, sont bien un élément caractéristique des couches supérieures. Quelques uns avaient été déjà entrevus ou signalés par Hale dans les premières images complexes de K et de H α , sous le nom de longs flocculi noirs, et présentés comme dûs très probablement aux couches élevées. On a en effet dans ces conditions les filaments les plus importants dont la raie noire est très large. En fait la reconnaissance complète des filaments et de leurs propriétés ne peut être abordée qu'avec les images mêmes des couches supérieures.

Un autre élément important de ces dernières couches est la plage faculaire brillante qui se retrouve au même point que sur la surface, mais avec des formes différentes.

En résumé, si on considère les quatre couches formées par la surface et l'atmosphère, les parties les plus brillantes restent au-dessus des facules. Mais les parties les plus noires ont des positions très différentes sur la surface et dans la couche supérieure. En bas ce sont les taches et en haut ce sont les filaments, qui ont une surface noire totale supérieure à celle des taches. Il convient de mesurer l'aire des filaments aussi exactement que celle des taches.

Recherches sur les mouvements de l'atmosphère. Spectro-enregistreurs des vitesses.

Le filament noir attire surtout l'attention, et a bien, comme il a été dit plus haut, une importance au moins égale à celle des taches. Quelle est donc l'origine, quelle est donc la nature de ces longues lignes noires ? Une réponse précise est bien difficile, et il suffit de rappeler notre incertitude à l'égard des taches qui sont étudiées depuis 300 ans. Cependant avec le filament, la recherche peut être plus facile. La surface, qui porte la tache, est comprise entre l'intérieur du soleil, qui nous échappe et les couches basses complexes de l'atmosphère ; mais la couche supérieure, à laquelle est lié le filament, est plus libre plus dégagée, et peut avoir une structure et des mouvements plus simples.

Et en effet, nous avons obtenu récemment à Meudon sur le filament quelques résultats dignes d'intérêt et grâce à l'emploi d'un appareil spécial, organisé jusqu'ici à Meudon seulement et appelé *Spectro-enregistreur des vitesses*. Cet appareil que j'emploie depuis 1892, a été en 1907 largement amélioré. Il décèle, comme son nom l'indique, les mouvements radiaux des vapeurs solaires, en juxtaposant les petits spectres de sections successives équidistantes sur le disque solaire, avec une seconde fente large et des mouvements discontinus automatiques. Cet enregistreur est un complément obligé du spectrohéliographe et est au moins aussi utile. Il décèle, outre les vitesses radiales, les formes générales de la vapeur, les détails de la raie entière et en particulier la largeur de la raie isolée, largeur très variable d'un point à l'autre de l'astre. Il révèle les

points où le spectrohéliographe est en défaut ; car ce dernier ne peut, avec une fente de largeur constante, isoler exactement une raie de largeur variable ; en un mot il enregistre tous les éléments qui échappent au spectrohéliographe et permet d'interpréter sûrement ses résultats.

Sur les épreuves obtenues avec la raie K, l'examen à l'œil nu montre aussitôt que les mouvements radiaux sont en général plus notables sur le filament que sur les points voisins ; parfois même toutes les raies K_3 du filament sont inclinées dans le même sens ; ce qui annonce un tourbillon à axe horizontal, qui peut-être opposé au tourbillon à axe vertical admis dans les taches. Mais, à cette agitation succède, comme avec la tache, un calme relatif. Si alors, on mesure avec soin les déplacements et la vitesse radiale de K_3 lorsque la vapeur est au centre du soleil, on trouve que la vapeur est ascendante et avec une vitesse souvent supérieure à la vitesse équatoriale de rotation (soit 2 km. par seconde). Le fait a été vérifié sur plusieurs filaments. Les taches et les filaments mis à part, les vitesses verticales dans la couche supérieure sont notables et souvent du même ordre que la vitesse équatoriales de rotation. La grandeur de ce mouvement vertical étonne moins si on remarque que la masse de gaz qui est l'atmosphère repose sur un foyer intense de chaleur.

Des mesures analogues ont été faites avec soin au centre du soleil sur les facules et les flocculi, et le résultat a été inverse. La vapeur, au contraire, a un mouvement descendant et les parties relativement noires autour sont ascendantes. D'une manière générale, aux points brillants de l'image K_3 de la couche supérieure, la vapeur descend ; elle monte là où l'image est relativement sombre ; ce qui est assez logique, car la vapeur qui descend se comprime et s'échauffe ; celle qui monte se détend et se refroidit.

Cette propriété reconnue déjà sur un grand nombre d'épreuves, est importante ; car elle explique la structure spéciale de ces couches atmosphériques, qui s'annoncent comme divisées en courants de convection juxtaposés, exactement comme les liquides de nos laboratoires chauffés uniformément par le bas.

Les flocculi brillants forment souvent sur une étendue notable et avec netteté des polygones juxtaposés par leurs sommets, et tout semblables aux polygones qui constituent les cellules tourbillons des liquides, si bien étudiées en France par Besnard.* Comme la vapeur descend sur les flocculi brillants et s'élève dans les intervalles, chaque polygone solaire est aussi une cellule tourbillon. Quant aux autres flocculi du même soleil ils offrent des polygones moins nets ou

* Cette disposition par polygones juxtaposés est parfois très nette sur le soleil presque entier. L'épreuve K_3 du 18 septembre 1908 présente dans l'hémisphère sud, près du centre, quelques-uns de ces polygones, réunis par leurs côtés et leurs sommets ; mais, une image plus nette et plus grande est nécessaire pour les bien voir.

incomplets, ou encore, mais plus rarement, ont des formes tout à fait irrégulières.

D'autre part, les filaments et alignements sont probablement la limite de tourbillons cellulaires plus grands, superposés aux précédents dans la couche supérieure, et dont les taches occuperaient le centre. Cette disposition est en accord avec les mouvements de cette couche près des taches reconnus par l'astronome anglais Evershed. On s'explique alors aisément pourquoi les taches sont des points et les filaments des lignes parfois très longues. La question, par ces recherches, est donc déjà un peu éclaircie; elle sera, semble-t-il, élucidée complètement par des mesures continues de vitesses radiales, mesures étendues au disque entier de l'astre, et malheureusement très longues.

Reconnaissance des filaments polaires.

Je terminerai par une nouvelle propriété des filaments récemment reconnue à Meudon et publiée. L'observatoire a déjà les images de la couche supérieure pour plus de 20 rotations entières de l'astre, et il est possible d'étudier la distribution des filaments. Ils apparaissent à toutes les latitudes; mais, aux pôles, en général, ils sont groupés sur une courbe plus ou moins circulaire, souvent non confondue avec un parallèle, et qui entoure le pôle. Cette courbe polaire de filaments, est parfois nettement dessinée au deux pôles; mais en général elle est nette seulement à un seul, et se déplace d'un pôle à l'autre. Cette courbe polaire était particulièrement nette et forte en avril dernier au pôle sud. (Voir les deux images du 11 avril et la Fig. 4, qui représente les filaments de quatre jours différents.)

Ces filaments polaires sont accompagnés de proéminences, et sont en accord avec les maxima secondaires de proéminences qui ont déjà été signalées aux pôles. Ils peuvent aussi être en relations avec la forme particulière de la couronne solaire au moment du minimum et avec l'inclinaison souvent constatée de l'axe coronal par rapport à l'axe ordinaire de rotation.

Parfois, la courbe polaire est accompagnée du côté de l'équateur d'une ligne de filaments parallèles, qui est réunie à la courbe par des filaments ou alignements inclinés; et on a ainsi une disposition analogue à celle des bandes de la planète Jupiter.

Enfin la zone polaire de filaments, où la vapeur, comme on l'a vu plus haut, est ascendante, peut-être rapprochée de la zone des taches et facules voisine de l'équateur, et où la vapeur est au contraire descendante. On est conduit à supposer dans la couche supérieure une grande circulation méridienne, un grand courant général de convection, analogue à celui qui existe sur la terre dans chaque hémisphère entre la latitude de 35° et le pôle.

Le temps manque malheureusement pour développer toutes les conséquences de ces premières observations. Mais les faits présentés

suffisent à montrer le grand intérêt des études sur l'atmosphère solaire supérieure et la nécessité de les continuer.

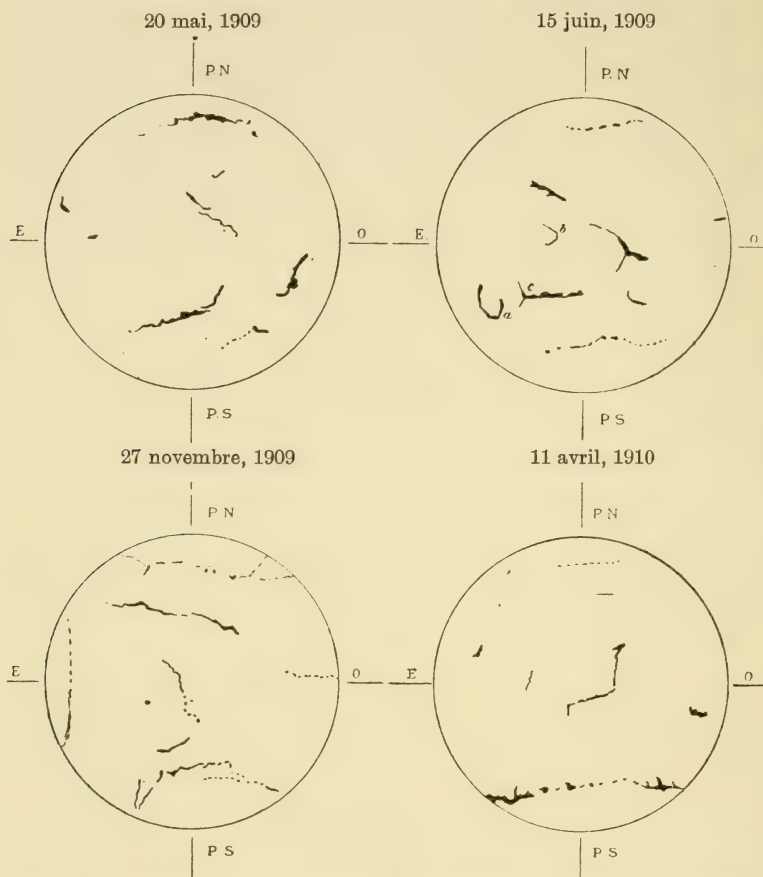


FIG. 4.—Images de la couche supérieure de l'atmosphère solaire qui montrent les filaments noirs caractéristiques et en particulier les filaments polaires. Ces images, obtenues avec l'aide de d'Azambuja, ont été relevées sur les épreuves monochromatiques du soleil obtenues avec la partie centrale des raies $H\alpha$ de l'hydrogène ou K du calcium. Elles montrent seulement les filaments noirs sans les alignements. Les plages brillantes des épreuves au-dessus des facules n'ont pas été représentées.

L'atmosphère solaire est la seule que nous puissions observer dans son ensemble et dans ses couches successives. Nos appareils enregistreurs donnent en quelques minutes son aspect général et ses mouve-

ments principaux ; à ce point de vue, elle nous est mieux connue que l'atmosphère terrestre que nous observons seulement dans ses parties basses et sur une étendue restreinte, même avec l'aide du télégraphe.

Le réseau de courants de convection et les filaments curieux reconnus dans les couches hautes du soleil, peuvent se retrouver aussi sur la terre, et c'est ainsi que l'étude du soleil peut nous apprendre à mieux connaître notre propre atmosphère.

[H. D.]

GENERAL MONTHLY MEETING,

Monday, July 4, 1910.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. D.Sc. F.R.S., Treasurer
and Vice-President, in the Chair.

Denys Hague, Esq.

was elected a Member of the Royal Institution.

The Chairman announced that His Majesty The King had graciously consented to become Patron of the Royal Institution.

The Special Thanks of the Members were returned to William Flockhart, Esq., F.R.I.B.A. *M.R.I.*, for his Gift of a Print of the Tempio di Antonino, the Ancient (converted) Temple, which, in its converted form, served Mr. Vulliamy as the model for the front of the Royal Institution in 1838.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Astronomer-Royal—Report to Board of Visitors of the Royal Observatory, 1910. 4to.

Secretary of State for India—G. T. Survey : Vol. XXV. Synoptical ; and Charts. 4to. 1909.

Report of Board of Scientific Advice, 1908–9. 8vo. 1910.

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta : Rendiconti. Vol. XIX. 1^o Semestre, Fasc. 9–11. 8vo. 1910.

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American Academy of Arts and Sciences—Proceedings, Vol. XLV. Nos. 8–15. 8vo. 1910.

American Geographical Society—Bulletin, Vol. XLII. No. 6. 8vo. 1910.

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British Association for the Advancement of Science—Report of the Seventy-ninth Meeting (Winnipeg), 1909. 8vo. 1910.

British Architects, Royal Institute of—Journal, Third Series, Vol. XVII. Nos. 15–16. 4to. 1910.

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Cambridge Philosophical Society—Proceedings, Vol. XV. Part 5. 8vo. 1910.

Cambridge University Library—Report for 1909. 8vo. 1910.

Canada, Department of Mines—Mineral Production of Canada, 1907–8. 8vo. 1910.

Canada, Office of the Archivist, Ottawa—Journal of Larocque, 1805. Edited by L. J. Burpee. 8vo. 1910.

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- Carnegie Foundation for the Advancement of Teaching*—Bulletin, No. 4: Medical Education in United States and Canada. 8vo. 1910.
- Chemical Industry, Society of*—Journal, Vol. XXIX. Nos. 11–12. 8vo. 1910.
- Chemical Society*—Proceedings, Vol. XXVI. Nos. 372–374. 8vo. 1910.
- Journal for June, 1910. 8vo.
- Cornwall Royal Polytechnic Society*—Seventy-seventh Annual Report. 8vo. 1910.
- Daily News, Ltd.*—Daily News Year Book, 1910. 8vo.
- Editors*—Agricultural Economist for June, 1910. 4to.
- Astrophysical Journal for June, 1910. 8vo.
- Athenæum for June, 1910. 4to.
- Author for July, 1910. 8vo.
- Chemical News for June, 1910. 4to.
- Chemist and Druggist for June, 1910. 8vo.
- Concrete for June, 1910. 8vo.
- Dyer and Calico Printer for June, 1910. 4to.
- Electrical Contractor for June, 1910. 8vo.
- Electrical Engineer for June, 1910. 4to.
- Electrical Engineering for June, 1910. 4to.
- Electrical Review for June, 1910. 4to.
- Electrical Times for June, 1910. 4to.
- Electricity for June, 1910. 8vo.
- Engineer for June, 1910. fol.
- Engineer-in-Charge for June, 1910. 8vo.
- Engineering for June, 1910. fol.
- Horological Journal for June, 1910. 8vo.
- Illuminating Engineer for July, 1910. 8vo.
- Journal of the British Dental Association for June, 1910. 8vo.
- Law Journal for June, 1910. 8vo.
- London University Gazette for June, 1910. 4to.
- Model Engineer for June, 1910. 8vo.
- Mois Scientifique for June, 1910. 8vo.
- Motor Car Journal for June, 1910. 4to.
- Musical Times for June, 1910. 8vo.
- Nature for June, 1910. 4to.
- New Church Magazine for July, 1910. 8vo.
- Nuovo Cimento for May, 1910. 8vo.
- Page's Weekly for June, 1910. 8vo.
- Revue d'Electrochimie for May, 1910. 8vo.
- Science Abstracts for May–June, 1910. 8vo.
- Zoophilist for June, 1910. 4to.
- Florence, Biblioteca Nazionale*—Monthly Bulletin for June, 1910. 8vo.
- Florence, Reale Accademia dei Georgofili*—Atti, Vol. VII. Disp. 2. 8vo. 1910.
- Geographical Society, Royal*—Journal, Vol. XXXVI. No. 1. 8vo. 1910.
- Geological Society*—Abstracts of Proceedings, No. 896. 8vo. 1910.
- Quarterly Journal, Vol. LXVI. Part 2. 8vo. 1910.
- Hele-Shaw, H. S., Esq., LL.D. F.R.S. (the Author)*—Aerial Automobilmism. 8vo. 1909.
- Aeronautical Engineering. 8vo. 1910.
- The Dirigible and the Aeroplane. 8vo. 1910.
- Jefferson Physical Laboratory*—Contributions, Vol. VII. 8vo. 1909.
- Johns Hopkins University*—American Journal of Philology, Vol. XXXI. No. 2. 8vo. 1910.
- University Circulars, 1909, Nos. 8–9; 1910, Nos. 1–4. 8vo.
- University Studies, Series XXVII. Nos. 8–12. 8vo. 1909.
- London County Council*—Gazette for June, 1910. 4to.
- Mechanical Engineers, Institution of*—Proceedings, 1909, Parts 3–4. 8vo. 1909.
- List of Members, 1910. 8vo.

- Metropolitan Asylums Board*—Annual Report, 1909. 8vo. 1910.
Microscopical Society, Royal—Journal, 1910, Part 3. 8vo.
Monaco, Musée Océanographique—Bulletin, Nos. 167–173. 8vo. 1910.
National Church League—Gazette for June, 1910. 8vo.
Navy League—The Navy for June, 1910. 8vo.
New York Academy of Sciences—Annals, Vol. XIX. Part 2. 8vo. 1909.
Paris, Société d'Encouragement pour l'Industrie Nationale—Bulletin for May, 1910. 4to.
Paris, Société Française de Physique—Bulletin, 1909. Fasc. 4–5. 8vo. 1909.
Peru, Society of Mining Engineers—Bulletin, No. 75. 8vo. 1909.
Pharmaceutical Society of Great Britain—Journal for June, 1910. 8vo.
Photographic Society, Royal—Journal, Vol. L. No. 6. 8vo. 1910.
Royal Colonial Institute—United Empire, Vol. I. No. 6. 8vo. 1910.
Royal Engineers' Institute—Journal, Vol. XII. No. 1. 8vo. 1910.
Royal Irish Academy—Proceedings, Vol. XXVIII. A, No. 2; C, Nos. 3–5. 8vo. 1910.
Royal Society of Arts—Journal for June, 1910. 8vo.
Royal Society of Edinburgh—Proceedings, Vol. XXX. Part 5. 8vo. 1910.
Royal Society of London—Proceedings, A, Vol. LXXXIV. No. 567. 8vo. 1910.
Philosophical Transactions, A, Vol. CCX. No. 468; *B*, Vol. CCI. No. 275. 4to. 1910.
St. Petersburg, Imperial Academy of Sciences—Bulletin, 1910, Nos. 10–11. 8vo.
Sanitary Institute, Royal—Journal, Vol. XXXI. No. 6. 8vo. 1910.
Saxon Academy of Sciences, Royal—Abhandlungen, Band XXVIII. Nos. 1–2. 8vo. 1910.
Berichte: Mat.-Phys. Klasse, 1909, Nos. 4–5; 1910, No. 1. *Phil.-Hist. Klasse* 1909, No. 3; 1910, Nos. 1–5. 8vo. 1909–10.
Smithsonian Institution—Miscellaneous Collections, Vol. LIV. No. 1923; Vol. LVI. Nos. 1929, 1931. 8vo. 1910.
Società degli Spettroscopisti Italiani—Memorie, Vol. XXXIX. Disp. 5. 4to. 1910.
Solvay, Institut de Sociologie—Bulletin Mensuel, No. 1, Jan. 1910. 8vo.
South African Association for the Advancement of Science—Journal, Vol. VI. No. 8. 8vo. 1910.
Statistical Society, Royal—Journal, Vol. LXXIII. Part 6. 8vo. 1910.
United Service Institution, Royal—Journal for June, 1910. 8vo.
United States Department of Agriculture—Experiment Station Record, Vol. XXII. No. 6. 8vo. 1910.
United States Department of Commerce and Labour—Magnetic Observations, Hawaii, 1905–6. 4to. 1910.
United States Patent Office—Gazette, Vol. CLIV. No. 5; Vol. CLV. Nos. 1–3. 8vo. 1910.
Vienna Imperial Geological Institute—Jahrbuch, Band LX. Heft 1. 8vo. 1910.
Warsaw, Society of Sciences—Comptes Rendus, 1910, No. 4. 8vo. 1910.
Western Australia, Agent-General—Monthly Statistical Abstract, Jan.–March, 1910. 4to.
Western Society of Engineers—Journal, Vol. XV. No. 2. 8vo. 1910.
Wilde, H., Esq., D.Sc. D.C.L. F.R.S. M.R.I. (the Author)—Celestial Ejectamenta. (First Halley Lecture.) 8vo. 1910.

GENERAL MONTHLY MEETING,

Monday, November 7, 1910.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. D.Sc. F.R.S., Treasurer
and Vice-President, in the Chair.

Tom Bousquet Browne, Esq., M.I.Mech.E.

H. S. Hele-Shaw, Esq., LL.D. F.R.S. M.Inst.C.E.

Arthur Cameron Hurtzig, Esq.

Arthur Latham, Esq., M.A. M.D. F.R.C.P. M.R.C.S.

William Newall, Esq.

Mrs. Henry Gordon Shee,

St. Clair Thomson, Esq., M.D. F.R.C.P. F.R.C.S.

were elected Members of the Royal Institution.

The Special Thanks of the Members were returned to Mrs. Tyndall for her valuable gift of two Nicol's Prisms, constructed for the Lectures on Light given by Dr. Tyndall in America in 1872, and used by him subsequently in his researches and lectures; also two pieces of Rocksalt, the remains of a large block given to Dr. Tyndall by the King of Würtemberg in 1867.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Admiralty, Lord Commissioners of—Nautical Almanac for 1913. 8vo. 1910.

The Secretary of State for India—Wheat in India. By A. Howard. 8vo. 1910.

Report on Public Instruction in Bengal, 1908-9, and Supplement. 4to. 1909-10.

Kodaikanal Observatory Bulletin, Nos. 20-22. 4to. 1910.

Geological Survey: Palæontologia Indica, Series XV. Vol. IV. Fasc. 2; New Series, Vol. III. Memoir I. Part 1. 4to. 1909-10.

Memoirs, Vol. XXXVII. Part 4; Vol. XXXVIII. 8vo. 1909-10.

Records, Vol. XXXVIII. Part 4; Vol. XXXIX. 8vo. 1910.

Memoirs of Department of Agriculture, Botanical Series, Vol. III. No. 5. 8vo. 1910.

Report on Government Museum and Connemara Public Library for 1909-10. 4to. 1910.

British Museum Trustees—Catalogue of Lepidoptera Phalænæ, Vol. IX. and Plates. 8vo. 1910.

Catalogue of British Chalcididæ. 8vo. 1910.

Guide to British Vertebrates. 8vo. 1910.

Guide to Crustacea, etc. 8vo. 1910.

VOL. XIX. (No. 104)

- Accademia dei Lincei, Reale, Roma*—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XIX. 1^o Semestre, Fasc. 12; 2^o Semestre, Fasc. 1-4. Classe di Scienze Morali, Serie Quinta, Vol. XIX. Fasc. 3-4. 8vo. 1910.
 Rendiconto, 1910, Vol. II. 4to.
- Allegheny Observatory*—Publications, Vol. I. No. 23; Vol. II. No. 1. 4to. 1910.
- American Academy of Arts and Sciences*—Proceedings, Vol. XLV. Nos. 16-20. 8vo. 1910.
- American Geographical Society*—Bulletin, Vol. XLII. No. 9. 8vo. 1910.
- American Philosophical Society*—Proceedings, Vol. XLIX. Nos. 194-196. 8vo. 1910.
- Antiquaries, Society of*—Archaeologia, Second Series, Vol. XI. Part 2. 4to. 1909.
- Aristotelian Society*—Proceedings, Vol. X. 1909-10. 8vo.
- Asiatic Society, Royal*—Journal for July-Oct. 1910. 8vo.
- Association of Accountants*—Journal, Vol. III. Nos. 10-11. 8vo. 1910.
- Astronomical Society, Royal*—Monthly Notices, Vol. LXX. No. 8. 8vo. 1910.
 List of Fellows, 1910. 8vo.
- Australasian Association for the Advancement of Science*—Report of Twelfth Meeting (Brisbane), 1909. 8vo. 1910.
- Bankers, Institute of*—Journal, Vol. XXXI. Parts 7-8. 8vo. 1910.
- Basel, Naturforschenden Gesellschaft*—Verhandlungen, Band XX. Heft 3; Band XXI. 8vo. 1910.
- Batavia, Royal Magnetical and Meteorological Observatory*—Observations, 1907. 4to. 1910.
- Belgium, Royal Academy of Sciences*—Bulletin, 1910, Nos. 5-8. 8vo.
- Berlin, Royal Academy of Sciences*—Sitzungsberichte, 1910, Nos. 24-39. 8vo.
- Birmingham Natural History Society*—Annual Report, 1909, and Proceedings, Vol. XII. No. 3. List of Members. 8vo. 1910.
- Boston Public Library*—Bulletin, Vol. III. Nos. 2-3. 8vo. 1910.
- British Architects, Royal Institute of*—Journal, Third Series, Vol. XVII. Nos. 17-20. 4to. 1910.
- British Astronomical Association*—Memoirs, Vol. XVII. Part 1. 8vo. 1910.
 Journal, Vol. XX. Nos. 9-10. 8vo. 1910.
 List of Members, 1910. 8vo.
- Broadbent, Cecil, Esq., M.R.I.*—Precious Stones and Gems. By E. W. Streeter 6th Edition. 8vo. 1898.
- Brooklyn Institute*—Science Bulletin, Vol. I. No. 17. 8vo. 1910.
- Buenos Aires*—Monthly Bulletin of Municipal Statistics for March-Aug., 1910. 4to.
- Cambridge Observatory*—Report, 1909-10. 8vo.
- Cambridge Philosophical Society*—Transactions, Vol. XXI. Nos. 12-14. 4to. 1910.
 Proceedings, Vol. XV. Part 6. 8vo. 1910.
- Canada, Geological Survey*—Memoirs, Nos. 6-7. 8vo. 1910.
 A Reconnaissance across the Mackenzie Mountains. 8vo. 1910.
 Summary Report for 1909. 8vo. 1910.
- Canada, Office of the Archivist*—Inventory of Military Documents. 8vo. 1910.
- Canadian Institute*—Transactions, Vol. VIII. Part 4. 8vo. 1910.
- Carnegie, Andrew, Esq. (the Author)*—William Chambers: An Address Delivered at the Celebration of the Jubilee of the Chambers Institution, 1909. 8vo.
- Carnegie Institution, Mount Wilson Solar Observatory*—Contributions, Nos. 46-48. 8vo. 1910.
- Chemical Industry, Society of*—Journal, Vol. XXIX. Nos. 13-20. 8vo. 1910.
- Chemical Society*—Journal for July-Oct. 1910. 8vo.
 Proceedings, Vol. XXVI. No. 375. 8vo. 1910.
 List of Fellows, 1910. 8vo.
- Chicago, Field Museum of Natural History*—Zoological Series, Vol. VII. No. 9; Vol. X. No. 3. 8vo. 1910.

- Civil Engineers, Institution of*—Proceedings, Vol. CLXXX. 8vo. 1910.
 List of Members, 1910. 8vo.
- Cracovic, Academy of Sciences*—Bulletin, 1910, Math.-Phys.-Klasse, A, Nos. 4-7; B, Nos. 4-6. 8vo.
- Crawford, The Rt. Hon. The Earl of, K.T. LL.D. F.R.S. M.R.I.*—Bibliotheca Lindesiana: Catalogue of Books Preserved at Haigh Hall. 4 vols. fol. 1910.
- Dax, Société de Borda*—Bulletin, 1909, Part 3; 1910, Part 1. 8vo.
- Durning-Lawrence, Sir Edwin, Bart., LL.B. M.R.I. (the Author)*—Bacon is Shakespeare. 8vo. 1910.
- Edinburgh, Royal Observatory*—Annals, Vol. III. 4to. 1910.
- Editors*—Agricultural Economist for July-Oct., 1910. 8vo.
 American Journal of Science for July-Oct. 1910. 8vo.
 Astrophysical Journal for July-Oct. 1910. 8vo.
 Athenæum for July-Oct. 1910. 4to.
 Author for Aug-Nov. 1910. 8vo.
 Chemical News for July-Oct. 1910. 4to.
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 Concrete for July-Oct. 1910. 8vo.
 Dyer and Calico Printer for July-Oct. 1910. 4to.
 Electrical Engineer for July-Oct. 1910. 4to.
 Electrical Engineering for July-Oct. 1910. 4to.
 Electrical Industries for July-Oct. 1910. 4to.
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 Ion for Oct. 1910. 8vo.
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 Law Journal for July-Oct. 1910. 4to.
 London University Gazette for July-Nov. 1910. 4to.
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 Page's Weekly for July-Oct. 1910. 8vo.
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 Science Abstracts for July-Oct. 1910.
 Science of Man for May-July 1910. 8vo.
 Surveying for July-Oct. 1910. 4to.
 Terrestrial Magnetism for June-Sept. 1910. 8vo.
 Zoophilist for July-Nov. 1910. 8vo.
- Electrical Engineers, Institution of*—Journal, Vol. XLV. Nos. 202-204. 8vo. 1910.
 List of Members, 1910. 8vo.
- Faraday Society*—Transactions, Vol. VI. Part 1. 8vo. 1910.
- Florence Biblioteca Nazionale*—Bulletin for July-Oct. 1910. 8vo.
- Florence, Reale Accademia dei Georgofili*—Atti, 5 S. Vol. VII. Disp. 3. 8vo. 1910.
- Franklin Institute*—Journal, Vol. CLXX. Nos. 1-4. 8vo. 1910.
- Geneva, Société de Physique*—Mémoires, Vol. XXXVI. Fasc. 3. 4to. 1910.

- Geographical Society, Royal*—Journal, Vol. XXXVI. Nos. 2-5. 8vo. 1910.
- Geological Society*—Quarterly Journal, Vol. LXVI. Part 3. 8vo.
- Geological Survey of the United Kingdom*—Summary of Progress, 1909. 8vo. 1910.
- Goppelsroeder, Professor Dr. F. (the Author)*—Kapillaranalyse, 1861-1909. 4to. 1910.
- Göttingen, Royal Society of Sciences*—Nachrichten, 1910, Mat. Phys. Klasse, Heft 2-4. Geschäftliche Mitteilungen, Heft 1. 8vo.
- Harlem, Musée Teyler*—Archives, Série II. Vol. XII. Part 1. 8vo. 1910.
- Harlem, Société Hollandaise des Sciences*—Archives Néerlandaises, Série II. Tome XV. Liv. 3-4. 8vo. 1910.
- Ouvres Complètes de Christiaan Huygens, Tome XII. 4to. 1910.
- Imperial College of Science*—Calendar, 1910-11. 8vo. 1910.
- Imperial Institute*—Bulletin, Vol. VIII. No. 2. 8vo. 1910.
- Iron and Steel Institute*—Journal, Vol. LXXXI. No. 1. 8vo. 1910.
- List of Members, 1910. 8vo.
- Japanese Imperial Government Commission*—Illustrated Catalogue of Japanese Old and Modern Fine Arts Displayed at the Japan-British Exhibition. 2 vols. 8vo. 1910.
- Johns Hopkins University*—American Journal of Philology, Vol. XXXI. No. 3. 8vo. 1910.
- Jordan, Wm. Leighton, Esq., M.R.I. (the Author)*—The Sling, Part V. 8vo. 1910.
- Kansas University*—Bulletin, Vol. XI. No. 7. 8vo. 1910.
- Geological Survey, Vol. IX. 8vo. 1908.
- Kyoto, Imperial University*—Memoirs of the College of Science, Vol. II. Nos. 1-8. 8vo. 1909-10.
- Life-Boat Institution, Royal National*—Journal for Aug.-Nov. 1910. 8vo.
- Linnean Society*—Journal, Zoology, Vol. XXXI. Nos. 201-207. 8vo. 1910.
- Transactions: Botany, Vol. VII. Parts 13-14; Zoology, Vol. X. Part 9, Vol. XIII. Parts 1-3. 4to. 1909-10.
- Literature, Royal Society of*—Transactions, Vol. XXX. Part 1. 8vo. 1910.
- List of Fellows, Charter, etc., 1910. 8vo.
- London County Council*—Gazette for July-Oct. 1910. 4to.
- Historical Houses in London, Part 29. 8vo. 1910.
- Madrid, Real Academia de Ciencias*—Revista, Tomo VIII. Num. 8-10. 8vo. 1910.
- Manchester Literary and Philosophical Society*—Memoirs, Vol. LIV. Part 3. 8vo. 1910.
- Mersey Conservancy*—Report on Present State of Navigation of River Mersey (1909). 8vo. 1910.
- Meteorological Office*—Fifth Annual Report of the Meteorological Committee. 8vo. 1910.
- Trade Winds of the Atlantic Ocean. 4to. 1910.
- Meteorological Society, Royal*—Journal, Vol. XXXVI. No. 155. 8vo. 1910.
- Record, Vol. XXIX. No. 117. 8vo. 1910.
- Metropolitan Water Board*—Fourth Annual Report. 8vo. 1910.
- Fifth Research Report. 4to. 1910.
- Mexico, Sociedad Científica "Antonio Alzate"*—Memorias, Tome XXVII. Nos. 4-10. 8vo. 1909.
- Microscopical Society, Royal*—Journal, 1910, Parts 4-5. 8vo.
- Mission Brésilienne d'Expansion Economique*—Brazil: Its Natural Riches and Industries, Vol. I. 8vo. 1910.
- Monaco, Institut Oceanographique*—Bulletin, Nos. 174-181. 8vo. 1910.
- Montagu-Pollock, Sir M. F., Bart., M.R.I. (the Author)*—A Theory of Drawing. 8vo. 1910.
- Montpellier Académie des Sciences*—Mémoires, 2^e Serie, Tome IV. Nos. 1-2. 8vo.

- Mott, Professor F. W., M.D. F.R.S. (the Author)*—The Brain and the Voice in Speech and Song. 8vo. 1910.
- Musical Association*—Proceedings, Thirty-sixth Session, 1909-10. 8vo.
- National Church League*—Church Gazette for July-Nov. 1910. 8vo.
- Navy League*—Navy League Annual, 1910-11. 8vo. 1910.
- The Navy for July-Nov., 1910. 8vo.
- New Jersey, Geological Survey*—Annual Report, 1909. 8vo. 1910.
- New South Wales, Deputy Controller of Prisons*—Report on Prisons, 1909. 4to. 1910.
- New York Academy of Sciences*—Annals, Vol. XIX. Part 3. 8vo. 1910.
- New York, Society for Experimental Biology*—Proceedings, Vol. VII. Nos. 4-5. 8vo. 1910.
- New Zealand, High Commissioner*—Statistics of the Dominion for 1908. 4to. 1909.
- Crown Lands Guide, 1910. 4to.
- Official Year Book, 1909. 8vo. 1910.
- Report of Department of Agriculture, 1909. 8vo. 1909.
- Norfolk and Norwich Naturalists Society*—Transactions, Vol. IX. Part 1. 8vo. 1910.
- North of England Institute of Mining Engineers*—Transactions, Vol. LX. Parts 4-9. 8vo. 1910.
- Strata of Northumberland and Durham, Supplementary Vol. 8vo. 1910.
- Nova Scotian Institute of Science*—Proceedings and Transactions, Vol. XII. Part 2. 8vo. 1910.
- Onnes, Dr. H. Kamerlingh*—Communications from the Physical Laboratory of the University of Leiden, Nos. 115-116. 8vo. 1910.
- Paris, Société d'Encouragement pour l'Industrie Nationale*—Bulletin for June-July, 1910. 4to.
- Paris, Société Française de Physique*—Bulletin, 1910, Fasc. 1-2. 8vo.
- Patent Office*—Catalogue of Library, Supplement, 1898-1909. 4to. 1910.
- Paton, Messrs. J. and J. (the Publishers)*—List of Schools. 8vo. 1910.
- Guide to Continental Schools. 8vo. 1910.
- Peru, Cuerpo de Ingenieros de Minas*—Boletín, No. 76. 8vo. 1910.
- Pharmaceutical Society of Great Britain*—Journal for July-Oct. 1910. 8vo.
- Philadelphia Academy of Natural Sciences*—Proceedings, Vol. LXII. Part 1. 8vo. 1910.
- Photographic Society, Royal*—Journal, Vol. L. No. 7-10. 8vo. 1910.
- Physical Society of London*—Proceedings, Vol. XXII. Parts 2-3. 8vo. 1910.
- Post Office Electrical Engineers, Institution of*—Journal, Vol. III. Parts 2-3. 8vo. 1910.
- Rockefeller Institute for Medical Research*—Reprints, Vol. X. 8vo. 1910.
- Rome, Ministry of Public Works*—Giornale del Genio Civile for March-Aug. 1910. 8vo.
- Röntgen Society*—Journal, Vol. VI. Nos. 24-25, July-Oct. 1910. 8vo.
- Royal College of Surgeons*—Calendar, 1910. 8vo.
- Royal Colonial Institute*—United Empire, Vol. I. Nos. 7-11. 8vo. 1910.
- Royal Dublin Society*—Proceedings: Scientific, Vol. XII. Nos. 30-36, and General Index, 1898-1909; Economic, Vol. II. No. 2. 8vo. 1910.
- Royal Engineers' Institute*—Journal, Vol. XII. Nos. 2-5. 8vo. 1910.
- Royal Horticultural Society*—Journal, Vol. XXXVI. Part 1. 8vo. 1910.
- Royal Irish Academy*—Proceedings, Vol. XXVIII. A, No. 3; B, Nos. 4-8; C, Nos. 6-12. 8vo. 1910.
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- Royal Society of Edinburgh*—Proceedings, Vol. XXX. Part 6. 8vo. 1910.
- Royal Society of London*—Philosophical Transactions, A, Vol. CCX. No. 469; B, Vol. CCI. Nos. 276-278. 4to. 1910.
- Proceedings, A, Vol. LXXXIV. Nos. 568-570; B, Vol. LXXXII. 557-560. 8vo. 1910.

- St. Petersburg, Imperial Academy of Sciences*—Bulletin, 1910, Nos. 10-14. 4to. Mémoires, Classe Physico-Mathématique, Vol. XVIII. Nos. 14-16; Vol. XXIII. Nos. 7-8; Vol. XXIV. Nos. 1-9. 4to. 1909.
- Sanitary Institute, Royal*—Journal, Vol. XXXI. Nos. 7-10. 8vo. 1910.
- Sawyer, Sir James, M.D. F.R.S. (the Author)*—Points of Practice in Maladies of the Heart. 8vo. 1908.
- Selborne Society*—Selborne Magazine for July-Nov. 1910. 8vo.
- Smith, B. Leigh, Esq., M.R.I.*—Scottish Geographical Magazine, Vol. XXVI. Nos. 7-11. 8vo. 1910.
- Smithsonian Institution*—Miscellaneous Collections, Vol. LIII. Nos. 6-7; Vol. LV.; Vol. LVI. Nos. 4-10; Vol. LVII. No. 1. 8vo. 1910.
- Società degli Spettroscopisti Italiani*—Memorie, Vol. XXXIX. June-Oct. 4to. 1910.
- South African Association for the Advancement of Science*—Journal, Vol. VI. Nos. 9-12. 8vo. 1910.
- Steeves, G. Walter, Esq., M.D. M.R.I. (the Author)*—Francis Bacon: A Sketch of his Life, Works, etc. 8vo. 1910.
- Sun Insurance Office*—The Early Days of the Sun Fire Office. By Edward Baumer. 8vo. 1910.
- Sweden, Royal Academy of Sciences*—Handlingar, Band XLV. Nos. 5-7, 11 4to. 1910.
Arkiv-Botantik, Band IX. Hft. 3-4; Kemi, Band III. Hft. 4-5; Zoologie, Band VI. Hft. 2-4. 8vo. 1910.
Årsbok, 1910, No. 1. 8vo.
- Toronto University*—Studies: Biological, No. 8; Chemical, Nos. 86-99; Geological, Nos. 6-7; Physical, Nos. 32-35. 8vo. 1909-10.
- Transvaal Department of Agriculture*—Journal for July, 1910. 8vo.
- United Service Institution, Royal*—Journal for July-Oct. 1910. 8vo.
- United States Coast and Geodetic Survey*—Observations at Magnetic Observatories, 1905-6. 4to. 1910.
- United States Department of Agriculture*—Experiment Station Record, Vol. XXII. Nos. 7-8; Vol. XXIII. Nos. 1-4. 8vo. 1910.
Farmers Bulletin, No. 405. 8vo. 1910.
- United States Department of the Interior*—Geological Survey: Bulletins, Nos. 398, 406, 407, 415, 417, 419, 420, 422, 428. 8vo. 1910.
Water Supply Papers, 241, 243, 244, 245, 248, 249, 252. 8vo. 1910.
Reports, 1909: Administrative, 2 vols.; Education, 2 vols. 8vo. 1910.
- United States Patent Office*—Official Gazette, Vols. CLV.-CLIX. 8vo. 1910.
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- Vienna, Imperial Geological Institute*—Verhandlungen, 1910, Nos. 2-8. 8vo. Jahrbuch, Band LX. Heft 2. 8vo. 1910.
- Warsaw, Society of Sciences*—Comptes Rendus, Vol. III. Nos. 5-6. 8vo. 1910.
- Wellcome Chemical Research Laboratories*—Publications, Nos. 101-107. 8vo. 1910.
- Western Australia*—Statistical Abstract for April-July, 1910. 4to.
- Western Australia, Geological Survey*—Bulletin, No. 33. 8vo. 1909.
- Western Society of Engineers*—Journal, Vol. XV. Nos. 3-4. 8vo. 1910.
- Wrightson, Sir Thomas, Bart., J.P. D.L. M.R.I. (the Author)*—On the Impulses of Compound Sound Waves and their Mechanical Transmission through the Ear. 4to. 1907.
- Yale University, Astronomical Observatory*—Transactions, Vol. II. Part 2. 4to. 1910.
- Yorkshire Philosophical Society*—Annual Report for 1909. 8vo. 1910.
- Zoological Society of London*—Proceedings, 1910, Parts 1-3. 8vo.
List of Fellows, 1910. 8vo.

GENERAL MONTHLY MEETING,

Monday, December 5, 1910.

SIR JAMES CRICHTON-BROWNE, M.D. LL.D. D.Sc. F.R.S., Treasurer
and Vice-President, in the Chair.

Harry Rudston-Read, Esq.
William Allan Tanner, Esq.

were elected Members of the Royal Institution.

Professor Alexander Graham Bell, M.D. LL.D. Ph.D. Officer
of the Legion of Honour, Washington, U.S.A.,
Professor Pëtr Nikolajewitsch Lebedew, Ph.D. Moscow,
Professor Jules Henri Poincaré, D.Sc. Membre de l'Institut,
Hon. F.R.S., Paris,
Professor Emanuele Paterno, Marchesi di Sessa, Rome,
Professor Emil Gabriel Warburg, Ph.D., Berlin,

were elected Honorary Members of the Royal Institution.

The Special Thanks of the Members were returned to Hugo
W. Müller, Esq., Ph.D. LL.D. D.Sc. F.R.S. *M.R.I.*, for his Dona-
tion of £1000 to the Fund for the Promotion of Experimental
Research at Low Temperatures.

The following Lecture Arrangements were announced :—

PROFESSOR SILVANUS P. THOMPSON, B.A. LL.D. D.Sc. F.R.S. *M.R.I.*,
Principal of the City and Guilds Technical College, Finsbury. Six Lectures
(adapted to a Juvenile Auditory) on SOUND: MUSICAL AND NON-MUSICAL :
a Course of Experimental Acoustics. On *Thursday*, Dec. 29, Dec. 31, 1910;
Jan. 3, 5, 7, 10, 1911.

PROFESSOR FREDERICK W. MOTT, M.D. F.R.S. F.R.C.P., Fullerian Pro-
fessor of Physiology, Royal Institution, etc. Six Lectures on HEREDITY. On
Tuesdays, Jan. 17, 24, 31, Feb. 7, 14, 21.

A. E. H. TUTTON, Esq., M.A. D.Sc. F.R.S. Three Lectures on CRYSTAL-
LINE STRUCTURE: MINERAL, CHEMICAL, AND LIQUID. (*With Illustrations.*)
On *Tuesdays*, Feb. 28, March 7, 14.

M. AUREL STEIN, Esq., C.I.E. Ph.D. D.Lit. D.Sc. Three Lectures on
EXPLORATIONS OF ANCIENT DESERT SITES IN CENTRAL ASIA. On *Tuesdays*,
March 21, 28, April 4.

THE ASTRONOMER ROYAL, FRANK WATSON DYSON, Esq., M.A. F.R.S.
F.R.A.S. Three Lectures on RECENT PROGRESS IN ASTRONOMY. On *Thurs-*
days, Jan. 19, 26, Feb. 2.

P. CHALMERS MITCHELL, Esq., M.A. D.Sc. LL.D. F.R.S., Secretary of the
Zoological Society of London. Three Lectures on PROBLEMS OF ANIMALS IN
CAPTIVITY. On *Thursdays*, Feb. 9, 16, 23.

ARTHUR C. BENSON, Esq. Two Lectures on *RUSKIN*. On *Thursdays*, March 2, 9.

PROFESSOR ARTHUR KEITH, M.D. F.R.C.S., Conservator of Museum, and Hunterian Professor, Royal College of Surgeons, England. Two Lectures on *GIANTS AND PYGMIES*. On *Thursdays*, March 16, 23.

PROFESSOR W. A. BONE, D.Sc. Ph.D. F.R.S., Livesey Professor of Applied Chemistry, University of Leeds. Two Lectures on *SURFACE COMBUSTION AND ITS INDUSTRIAL APPLICATIONS*. On *Thursdays*, March 30, April 6.

ARTHUR HASSALL, Esq., M.A., Student of Christ Church, Oxford. Three Lectures on *PROBLEMS IN THE CAREER OF THE GREAT NAPOLEON*. On *Saturdays*, Jan. 21, 28, Feb. 4.

THOMAS G. JACKSON, Esq., R.A. M.A. LL.D. F.S.A. Three Lectures on *ARCHITECTURE: THE BYZANTINE AND ROMANESQUE PERIOD*. On *Saturdays*, Feb. 11, 18, 25.

PROFESSOR SIR J. J. THOMSON, M.A. LL.D. D.Sc. F.R.S. M.R.I., Professor of Natural Philosophy, Royal Institution, etc. Six Lectures on *RADIANT ENERGY AND MATTER*. * On *Saturdays*, March 4, 11, 18, 25, April 1, 8.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz. :—

FROM

Secretary of State for India—Geological Survey: Records, Vol. XL. Parts 1-2. 8vo. 1910.

Agricultural Journal of India, Vol. V. Part 4. 8vo. 1910.

Accademia dei Lincei, Reale, Roma—Classe di Scienze Fisiche, Matematiche e Naturali. Atti, Serie Quinta: Rendiconti. Vol. XIX. 2^a Semestre, Fasc. 5-9. 8vo. 1910.

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Allegheny Observatory—Publications, Vol. II. Nos. 2-8. 4to. 1910.

Miscellaneous Papers, N.S. No. 4. 8vo. 1910.

American Academy of Arts and Sciences—Proceedings, Vol. XLV. No. 21; Vol. XLVI. Nos. 1-9. 8vo. 1910.

American Geographical Society—Bulletin, Vol. XLII. No. 10. 8vo. 1910.

Amsterdam, Royal Academy of Sciences—Proceedings, Vol. XII. 8vo. 1909-10. Verslagen, Vol. XVIII. 8vo. 1909-10.

Verhandelingen, 2^e Sectie, Dl. XV. No. 2; Dl. XVI. Nos. 1-3. 8vo. 1910.

Jaarboek, 1909. 8vo. 1910.

Antiquaries, Society of—Archæologia, Vol. LXII. Part 1. 4to. 1910.

Proceedings, Vol. XXIII. No. 1. 8vo. 1910.

Astronomical Society, Royal—Monthly Notices, Vol. LXX. No. 9. 8vo. 1910.

Bankers, Institute of—Journal, Vol. XXXI. No. 9. 8vo. 1910.

Belgium, Royal Academy—Mémoires: Collection in 4to, 2^e Série, Tome II. Fasc. 4-5, Tome III. Fasc. 1; Collection in 8vo, 2^e Série, Tome II. Fasc. 7. 1910.

Botanic Society of London, Royal—Botanic Journal, Vol. I. No. 1, Oct. 1910. 8vo.

British Architects, Royal Institute of—Journal, Third Series, Vol. XVIII. No. 1. 4to. 1910.

The Kalendar, 1910-11. 8vo.

British Astronomical Association—Journal, Vol. XXI. No. 1. 8vo. 1910.

Canada, Department of Mines—Geology of Hedley District. 8vo. 1910.

Contributions to Canadian Palæontology, Vol. III. 8vo. 1910.

Publications, Nos. 59, 68. 8vo. 1910.

Chemical Industry, Society of—Journal, Vol. XXIX. Nos. 21-22. 8vo. 1910.

Chemical Society—Proceedings, Vol. XXVI. Nos. 376-377. 8vo. 1910.

Journal for Nov. 1910. 8vo.

- Civil Engineers, Institution of*—Proceedings, Vol. CLXXXI. 8vo. 1910.
Crawford, The Rt. Hon. The Earl of, K.T. LL.D. F.R.S. M.R.I.—*Bibliotheca Lindesiana*: Catalogue of Tudor and Stuart Proclamations, 1485-1714. 2 vols. fol. 1910.
- Devonshire Association*—Transactions, Vol. XLII. 8vo. 1910.
Devonshire Wills, Part X. 8vo. 1910.
- East India Association*—Journal, N.S. Vol. I. No. 4. 8vo. 1910.
- Editors*—*Aeronautical Journal* for Oct. 1910. 8vo.
Agricultural Economist for Nov. 1910. 4to.
American Journal of Science for Nov. 1910. 8vo.
Athenæum for Nov. 1910. 4to.
Author for Dec. 1910. 8vo.
Chemical News for Nov. 1910. 4to.
Chemist and Druggist for Nov. 1910. 8vo.
Concrete for Nov. 1910. 8vo.
Dyer and Calico Printer for Nov. 1910. 4to.
Electrical Engineer for Nov. 1910. 4to.
Electrical Engineering for Nov. 1910. 4to.
Electrical Review for Nov. 1910. 4to.
Electrical Times for Nov. 1910. 4to.
Electricity for Nov. 1910. 8vo.
Engineer for Nov. 1910. fol.
Engineer-in-Charge for Nov. 1910. 8vo.
Engineering for Nov. 1910. fol.
Horological Journal for Nov. 1910. 8vo.
Illuminating Engineer for Dec. 1910. 8vo.
Journal of the British Dental Association for Nov. 1910. 8vo.
Journal of Physical Chemistry for Nov. 1910. 8vo.
Law Journal for Nov. 1910. 8vo.
Model Engineer for Nov. 1910. 8vo.
Motor Car Journal for Nov. 1910. 4to.
Musical Times for Nov. 1910. 8vo.
Nature for Nov. 1910. 4to.
New Church Magazine for Dec. 1910. 8vo.
Page's Weekly for Nov. 1910. 8vo.
Science Abstracts for Nov. 1910. 8vo.
Science of Man for Aug.-Sept. 1910. 8vo.
- Florence, Biblioteca Nazionale*—Monthly Bulletin for Nov. 1910. 8vo.
- Franklin Institute*—Journal, Vol. CLXX. No. 5. 8vo. 1910.
- Geographical Society, Royal*—Journal, Vol. XXXVI. No. 6. 8vo. 1910.
- Geological Society*—Abstracts of Proceedings, Nos. 897-898. 8vo. 1910.
- Imperial Institute*—Bulletin, Vol. VIII. No. 3. 8vo. 1910.
- Iron and Steel Institute*—Carnegie Scholarship Memoirs, Vol. II. 8vo. 1910.
- Linnean Society*—Proceedings, 122nd Session. 8vo. 1910.
 List of Fellows, 1910-11. 8vo. 1910.
 Journal: Botany, Vol. XXXIX. No. 272; Zoology, Vol. XXX. No. 202. 8vo. 1910.
- London County Council*—Gazette for Nov. 1910. 4to.
- Meteorological Society, Royal*—Journal, Vol. XXXVI. No. 156. 8vo. 1910.
 Record, Vol. XXX. No. 118. 8vo. 1910.
- Monaco, Musée Océanographique*—Bulletin, Nos. 182-184. 8vo. 1910.
- Munich, Royal Bavarian Academy of Sciences*—Abhandlungen, Band XXIV. Ab. 3; XXV. Ab. 4. Sup. Band I. Ab. 9-10; II. Ab. 2.; IV. Ab. 1-2 4to. 1910.
 Sitzungsberichte, 1910, Ab. 5-9. 8vo.
- Natal, Agent-General*—Report on Mining Industry of Natal for 1909. fol. 1910.
- National Church League*—Gazette for Dec. 1910. 8vo.
- Paris, Société d'Encouragement pour l'Industrie Nationale*—Bulletin for Aug.-Oct. 1910. 4to.

- Pharmaceutical Society of Great Britain*—Journal for Nov. 1910. 8vo.
- Philadelphia, Academy of Natural Sciences*—Proceedings, Vol. LXII. Part 2. 8vo. 1910.
- Photographic Society, Royal*—Journal, Vol. L. No. 11. 8vo. 1910.
- Quekett Microscopical Club*—Journal, Ser. 2, Vol. XI. No. 67, Nov. 1910. 8vo.
- Royal Engineers' Institute*—Journal, Vol. XII. No. 6. 8vo. 1910.
- Royal Society of Arts*—Journal for Nov. 1910. 8vo.
- Royal Society of London*—Proceedings, B, Vol. LXXXIII. No. 561. 8vo. 1910.
- St. Petersburg, Imperial Academy of Sciences*—Bulletin, 1910, Nos. 15–16. 8vo.
- Sanitary Institute, Royal*—Journal, Vol. XXXI. No. 11. 8vo. 1910.
- Scottish Meteorological Society*—Journal, Vol. XV. Third Series, No. 27. 8vo. 1910.
- Selborne Society*—Selborne Magazine for Dec. 1910. 8vo.
- Siemens, Alexander, Esq., M.Inst.C.E. M.R.I. (the Author)*—Presidential Address to Institution of Civil Engineers, 1910. 8vo.
- Smithsonian Institution*—Miscellaneous Collections, Vol. LVI. Nos. 11, 13. 8vo. 1910.
- United Service Institution, Royal*—Journal for Nov. 1910. 8vo.
- United States Department of Agriculture*—Experiment Station Record, Vol. XXIII. No. 5. 8vo. 1910.
- Report of the Chief of the Weather Bureau, 1908–9. 4to. 1910.
- United States Department of the Interior*—Geological Survey: Bulletins 381, 402, 425, 426, 427, 432. 8vo. 1910.
- Professional Paper 68. 4to. 1910.
- Water Supply Papers, 237, 239, 246, 247, 250, 251. 8vo. 1910.
- Geological Atlas of the United States, Nos. 160–173. fol. 1908–10.
- United States Patent Office*—Gazette, Vol. CLX. 8vo. 1910.
- Vereins zur Beförderung des Gewerbflusses*—Verhandlungen, 1910, Heft 9. 4to.
- Vienna Imperial Geological Institute*—Verhandlungen, 1910, Heft 9–12. 8vo.
- Jahrbuch, Band LX. Heft 3. 8vo. 1910.
- Warsaw, Society of Sciences*—Comptes Rendus, Vol. III. No. 7. 8vo. 1910.
- Western Society of Engineers*—Journal, Vol. XV. No. 5. 8vo. 1910.
- Wilde, H., Esq., D.Sc. D.C.L. F.R.S. M.R.I. (the Author)*—On the Origin of Cometary Bodies and Saturn's Rings. 8vo. 1910.

WEEKLY EVENING MEETING,

Friday, January 21, 1910.

SIR WILLIAM CROOKES, LL.D. D.Sc. For. Sec. Roy. Soc.

Honorary Secretary and Vice-President, in the Chair.

PROFESSOR SIR JAMES DEWAR, LL.D. D.Sc. F.R.S.,

Fullerian Professor of Chemistry.

Light Reactions at Low Temperatures.

[ABSTRACT.]

THE physiological action of light was the subject of my first Friday Evening Lecture. At that time it almost seemed that we should not materially add to our knowledge of physiological problems by the application of very low temperatures. Later researches conducted in conjunction with Professors McKendrick and Macfadyen established that mere cooling did not destroy putrefying organisms, and also that other bacilli were not destroyed by continued alternations of warmth and extreme low temperatures. Seeds which had been kept for six hours in liquid hydrogen suffered no loss of vitality, neither did bacteria. It has been thought that chemical action is impossible at the low temperatures now obtainable, but there are reactions which are still possible. As an example your attention will be drawn, among other things, to the results of some recent experiments on phosphorescent bacteria, these organisms being peculiarly adapted for investigations of this kind from their special property of producing luminosity during life.

In dealing with light reactions, care must be taken to exclude the action of the radiant heat which always accompanies it. This involves the need of some arrangement like a water cell built up with quartz windows, which absorbs heat but is transparent to invisible light of short wave-length.

Experiments showing some effects of light radiation.—Here is a tall glass jar containing some chlorine peroxide, a yellow gas mixed with the air. When the radiation from the arc lamp is made to pass through it, decomposition is at once evident from the clouds which are formed.

The change of colour by cooling which is shown by some substances is of a different order. A strip of paper coated with red iodide of mercury can be cooled by simply dipping half-way into liquid air. The effect is to change the red to yellow in the cooled region.

The fluorescent appearance of some bodies when exposed to light is well known. This tall cylinder contains simply water, illuminated by

a vertical beam of violet light reflected in from an arc lamp. A few minute fragments of solid eosin thrown in, gradually dissolve, producing a red solution. The specks of eosin as they fall through the water leave behind a brilliant yellowish fluorescent track which soon permeates the whole solution. The violet rays are absorbed by the eosin, and subsequently emitted in the form of this beautiful greenish yellow colour.

A somewhat similar fluorescent or phosphorescent effect is produced by some bodies at low temperatures. On the outside of this long test tube is spread some ordinary egg albumen. This can be cooled down and frozen by simply pouring a little liquid air into the tube. When exposed to the light of the arc the frozen albumen shows a beautiful blue phosphorescence. A second tube covered with glycerin and similarly frozen shows a like effect. Zymase or yeast juice, obtained by breaking up yeast-cells and extracting the liquid contents, shows marked phosphorescence. All culture media used in bacteriological research behave in the same way. An ivory paper-knife cooled by dipping into liquid air phosphoresces brightly. A strip of gelatin behaves in the same manner, so also an ordinary paraffin candle.

These substances all show phosphorescence at the low temperature. Calcium sulphide on the other hand is a body which will phosphoresce at ordinary temperatures, and it becomes interesting to see what will be the effect of cooling on this luminous paint. This star-shaped piece of cardboard, which has been coated with calcium sulphide, shows strong phosphorescence at the ordinary temperature. Now float it on some liquid air in a dish, and you observe the light becomes dim and disappears. Warm it up by merely waving it in the air, and very soon it again gives out its characteristic light. This vacuum vessel contains a similar star which has been kept in liquid air for 24 hours. On taking it out the dark star becomes brightly luminous. Its phosphorescing properties are stored up or rendered latent so long as the low temperature is maintained. In order to become phosphorescent the body must first be exposed to light. It is interesting to see whether cooling to the temperature of liquid air before and during such an exposure has the effect of preventing subsequent phosphorescence. A similar sulphide of calcium cardboard star which has not been exposed to light and is consequently non-luminous at the ordinary temperature, is floated in an aluminium dish on the surface of liquid air, and when thoroughly cooled to -185° , exposed to violet light. All remains dark, but on allowing it to warm up we see that light-energy must have been absorbed at the low temperature, because the star now phosphoresces in a marked degree. If this experiment is repeated, using liquid hydrogen instead of liquid air, the same effects are observed.

All these substances absorb ultra-violet light and transform it into visible phosphorescence. By a similar absorption other bodies, however, possess the power of yielding other forms of energy. The

case of oxygen is interesting. This is transparent to heat, but has many absorption bands for light in the visible spectrum, and also has the power of absorbing the ultra-violet. Nitrogen, on the other hand, is relatively transparent. An ordinary spectrum is shown on the screen. A flask containing liquid oxygen is introduced into the beam, and several dark bands are produced, showing the absorptive power of the liquid. A similar vessel of liquid nitrogen, however, shows no such bands. These two spherical vacuum flasks of liquid oxygen and liquid nitrogen respectively can be placed in the parallel beam of the arc, and it will be seen that they both act as an ordinary lens and converge the light to a focus. On holding a piece of black paper in the focus, very soon a hole is burnt through and the paper ignites, the heat rays being evidently transmitted. We will now project a few photographs of absorption spectra of both liquid nitrogen and liquid oxygen admixed with it in various proportions, also similar slides of absorption spectra of yeast juice, gelatin, glycerin, etc., etc. These photographs were taken with a quartz spectrograph, using for the most part a cadmium-magnesium spark. All the photographs show marked absorption in the region of the ultra-violet.

Photo-electric Cells.—Cells can be constructed whereby light energy can be transformed into electrical energy. This cell has plates of silver coated with chloride of silver, placed in dilute sulphuric acid in a quartz tube, and is connected to a reflecting galvanometer showing a spot of light on the scale. When light is allowed to fall on one of the plates a deflection takes place. The chemical decomposition produced by light is transformed into electrical energy, causing an electric current. Other forms of light cells filled with liquid mixtures sensitive to light have been used in recent experiments. Their general construction is shown in Plate I. figs. I. and II. A is a fine platinum wire (secured in the paraffined cork in the base of the tube), drawn tight against the inner surface, and fixed over the top edge by soldering on to stouter copper wire wound round the outside of the top of the tube. A thick platinum wire B is placed at the back to act as the second electrode. The cell is conveniently mounted in a paraffin block in which are two depressions for mercury cups to connect the two electrodes. A is thus made to enclose a thin film of liquid between itself and the wall of the tube. Any change in the composition of this external film will not diffuse rapidly into the cell liquid, and thus differences of electrical potential so produced will be detected by means of the galvanometer. One of the two cells on the table contains a saturated aqueous solution of chlorine peroxide, the other a 20 per cent. solution of uranium nitrate in methyl alcohol.

When a beam of light from the lantern is directed on to the chlorine peroxide cell a good deflection of the galvanometer is observed. On shutting off the light the disturbance soon passes away, as a uniform diffusion in the cell is effected. The uranium nitrate

shows an even greater deflection, which does not die away so quickly unless we short circuit the cell. Such electrical effects depend upon the temperature of the cell. On cooling the uranium nitrate cell with a pad of cotton wool saturated with liquid air the alcohol will be cooled down to its freezing point, and now no electrical action takes place. In fact a temperature of only -80° C. is sufficient to arrest any current, the solution being then congealed to a jelly.

The ordinary photographic action of light is similarly prevented by cooling to a low temperature. A piece of photographic paper kept cooled locally in a patch of about 3 in. diameter by a cotton wool pad soaked in liquid air while exposed to the electric beam is blackened, except in the cooled patch which remains unacted upon.

The photographic action of ordinary light is largely restricted to the ordinary temperature. When we come to ultra-violet light we have a different state of things. This radiation is capable of producing effects at the temperature of liquid air and even liquid hydrogen. A convenient and powerful source of ultra-violet light is the electric arc in mercury vapour contained in silica tubes. Glass tubes would of course absorb the ultra-violet light besides being liable to fracture from the high temperature of the lamp. An arc between copper and carbon poles in air is also available as a source of ultra-violet light, but is not so convenient to work as a modern mercury vapour lamp. In all my later experiments on phosphorescent bacteria the mercury lamp has been used. The general arrangement of the apparatus is shown in Plate II. figs. I. and II. Liquid oxygen is highly absorptive of the ultra-violet light. If a few cubic centimetres of liquid oxygen are poured into a shallow silver dish floating on liquid air, and a quartz cell containing water to the depth of a centimetre is arranged to absorb the bulk of the heat radiation, the ultra-violet rays can continue to act on the liquid oxygen for a considerable time. Pouring the remaining liquid oxygen into an ordinary glass beaker, in which are suspended some strips of paper treated with potassium iodide and starch solution, the presence of ozone in the evaporating liquid is evident from the dark blue colour produced. To prove that this is the effect of the ultra-violet radiation the experiment will be repeated, with a very thin lamina of mica, which is opaque to the ultra-violet, interposed between the mercury lamp and the liquid oxygen. In this case no ozone is produced, the strips of paper remaining quite white after the exposed oxygen is poured into the beaker. The smell of ozone during the evaporation of the liquid air is the most delicate and characteristic property of the body. If the ozone has to be estimated quantitatively, then the liquid oxygen after exposure to the ultra-violet radiation of the arc or mercury lamp is transferred to a small vacuum vessel B (Plate I. fig. iv.) and allowed to slowly evaporate, the gas being bubbled through iodide of potassium solution in the vessel A. In order to prove that the formation of ozone can take place in liquid oxygen, avoiding any action on the gas, a quartz vacuum vessel (Plate I. fig. III.) had a

concentrated beam from the arc condensed in the liquid by means of a quartz lens D. After an hour's exposure ozone could easily be recognized.

The exposure at liquid hydrogen temperatures is somewhat too dangerous to carry out on the lecture table, but solid oxygen in liquid hydrogen contained in a quartz vacuum tube has also been subjected to the ultra-violet radiation. When the liquid hydrogen evaporated away, ozone came off from the liquefied oxygen during its evaporation, so that the production of ozone was still possible even at 20° A. from the impact of ultra-violet rays on solid oxygen.

Experiments on the action of ultra-violet light on phosphorescent bacteria at low temperatures.—The *Photobacterium phosphorescens* is also known as *Photobacterium phosphorescens gelidus*. It is the most widely distributed and best known of all photogenic bacteria. It occurs on the bodies of nearly all dead fish and is most easily obtained from dead herring or mackerel. The light emitted by this Bacillus is of a brilliant green colour which provides an easy means of identification. The culture-medium must contain certain inorganic salts characteristic of sea-water. A good artificial culture-medium has the following composition :—

1 litre of beef broth peptone gelatine.	
Sodic chloride	26.5 grammes.
Potassic chloride	0.75 "
Magnesian chloride	3.25 "

The microscopic appearance of these bacteria at different ages, and also when grown in different culture media, are shown in Plate III. Several cultures of these organisms, kindly prepared by Mr. Henry Crookes, are on the table. At the temperature of the room they are very bright. This large shallow tin vessel contains a good bright culture, and on to it some liquid air can be poured, which rapidly destroys the luminosity of the bacteria. The organisms are not killed, for on waving the tin in the air, thus allowing it to warm up, the brightness again appears. While the organisms are at liquid air temperature they can be exposed to the action of ultra-violet light. For this purpose a smaller culture in a tin dish is floated on liquid air by the arrangement shown in Plate II. fig. 1. A is a tin or aluminium dish about 10 cm. wide and 2 cm. high, in which is placed the smaller dish containing the culture. This arrangement is supported by a cork float on the liquid air contained in a glass silvered hemispherical vacuum vessel B. On B, a shield C rests, consisting simply of two polished sheets of tinned iron, fixed at a small distance apart, which protects the liquid air in B from the heat radiation. C is pierced by a central hole rather wider than A, in which a quartz dish D, about 1 cm. high, is placed, filled with water to act as a heat filter. This arrangement can be placed either under the arc or the mercury lamp, whichever is being employed.

Five minutes' exposure at a distance of a few inches from the mercury lamp is usually sufficient to kill the bacteria when they are

located on the surface layer of the culture medium. The exposed culture is taken out and allowed to warm up in the air. No brightness supervenes even after the culture is kept for several hours. A few colonies will occasionally develop on keeping for a day, but this is because the growth of the bacteria has extended downwards into the medium, and are thus protected from the radiations, since a thin layer of the culture medium is sufficient to absorb the ultra-violet. If a metal plate, out of which a cross has been cut, or perforated metal is placed on the surface of the frozen bacteria before exposure to light, the appearance of the surface of the bacteria when allowed to heat up to the ordinary temperature is shown in Plate IV. fig. II.

There are sources of deception possible in this work, unless proper precautions are taken. If the dish containing the culture is floated direct on the liquid air, and especially if it be heavy, causing the level of the culture to be below that of the exterior liquid air, it frequently happens that a thin layer of liquid oxygen makes its appearance on the surface of the culture, either by condensation at the lower level, or by capillary action, or even spraying. In this case we have the complications produced by the absorption of the ultra-violet light by the thin oxygen layer, and the possible action of the ozone produced. In many cases surprising results were obtained, in which the organisms seemed to remain alive even after long exposures. In addition to this there was the possibility of condensation on the cold culture of both moisture and oxides of nitrogen, the latter always present in the air round such a mercury lamp, although while the culture is in the liquid air, the atmosphere over it is for the most part kept clear of contamination by the evaporating oxygen and nitrogen. Another arrangement was used in order to allow the ultra-violet radiation to act on the bacteria in an atmosphere of hydrogen or pure nitrogen, thus eliminating any of the secondary reactions mentioned above. Plate II. fig. II. shows such an arrangement. A simple thin sheet metal box M is fitted with a quartz cover N, which slides tightly into it. The dish A, containing the culture, is placed in M, into which also a narrow lead pipe P opens. The whole arrangement can be supported by P in any position in the vessel of liquid air. Through P hydrogen or nitrogen can be passed, the excess escaping round the joint of M and N. Adopting such precautions made no difference in the efficiency of the ultra-violet in killing off the bacteria at low temperatures.

The luminosity of these bacteria may remain latent for considerable periods. Oxygen is necessary for their activity, but they will remain alive, although dark, if air be excluded for several weeks, although they then need a little time to develop their activity. The curves of Plate IV. fig. I. are interesting, and show the rate of growth of two identical colonies in air and a vacuous flask respectively. The ordinates represent the diameter of the colonies in millimetres. The abscissæ show the age in days. Curve No. (II)

shows the growth in a flask kept vacuous for fifteen days, and then opened to the air. Curve (I) is the growth of a colony in an ordinary open flask, which, however, was exhausted and sealed on the fifteenth day. Curve (II) is practically a horizontal line during the fifteen days of exhaustion, showing no development of the colony, whereas during this time curve (I) shows a steady increase. After the fifteen days, however, curve (II) begins to develop, and shows an increase corresponding to the opening of the exhausted flask. The effect of oxygen can be illustrated by a few experiments. This flask contains some broth culture of these bacilli, and is only feebly luminescent, but on bubbling oxygen through the liquid the froth so formed is exceedingly bright, the bacteria being excited by the oxygen to great activity. Here are three flasks over the interior surface of which some phosphorescent culture medium has been spread, followed by exhaustion of the flasks. They have remained so for one, two, and three weeks respectively. Upon opening these flasks the luminescence will once more re-establish itself by the action of the oxygen so admitted, although (especially in the case of the three weeks' old culture) its full brightness will not develop for 24 hours.

The phosphorescent bacteria behave differently towards various metals. Various cultures have been prepared, and plates of various substances placed thereon, and remarkable effects obtained. Some reproductions of these are shown (Plate V. figs. 1 to 6). Thus, a disk of zinc caused death to the organisms in a considerable zone surrounding it. Similarly, also, copper and silver, and especially mercury, which is particularly fatal. Tin, on the other hand, is inactive.

A piece of the metal whose effect it is desired to examine is placed in the centre of the tin dish containing the medium infected with the phosphorescent bacteria. The growth of the phosphorescence is then observed. Several metals have no apparent effect. Among these are gold and the platinum group, tantalum, cadmium, magnesium and tin. Similarly coconut charcoal, graphite, and selenium have no effect. Sulphur seems actually to stimulate the bacteria which cluster brilliantly round it on the culture, leaving the rest of the plate relatively dark. The metals which have been found to slightly check the growth in their neighbourhood are bismuth, thallium, lead and nickel. The alloys German silver, hard brass, and aluminium bronze have also only a slight effect. A stronger action is shown by iron, aluminium, zinc, copper and soft brass. The more deadly metals are cobalt, silver, mercury, antimony, arsenic, and phosphor bronze. Chloride and bromide of silver, cyanide of mercury, and arsenous acid also kill the bacteria. The destructive effect of the metal clearly depends on its slow solution at the ordinary temperature in the saline organic culture medium, aided no doubt by the presence of atmospheric oxygen in some cases.

The following table gives a list of the metals used and their effects on the bacterium.

ACTION OF METALS ON *B. PHOSPHORESCENS*.

No Action	Partial Action	Strong Action
Gold	Bismuth	Cobalt
Platinum	Thallium	Silver
Palladium	Lead	Mercury
Rhodium	Nickel	Antimony
Iridium	German silver	Arsenic
Tantalum	Hard brass	Phosphor-bronze
Cadmium	Aluminium bronze	
Magnesium		Chloride of silver
Tin		Bromide of silver
Graphite	Strong Action	Cyanide of mercury
Cocoanut charcoal		Chloride of mercury
Selenium		(HgCl ₂)
Sulphur (stimulates)	Iron	
	Aluminium	Arsenious acid
	Zinc	
	Copper	
	Soft brass	

Other bacteria have been exposed to the action of ultra-violet light. *Bacillus prodigiosus*, *B. coli communis*, *B. subtilis*, as well as *B. phosphorescens* are all killed by the action of this radiation at the temperature of -185°C .

It is remarkable that any radiation effect should take place in these organisms at liquid air temperature, when of course they are hard solids, and no question of ordinary chemical interaction between liquids or gases is possible, as in the case of such powerfully reacting agents as fluorine and liquid hydrogen, which explode violently when brought together under such conditions. Ozone from solid oxygen is, of course, another example of a chemical change taking place at the boiling point of hydrogen, which in this case is induced by the ultra-violet radiation. Any action that takes place must be in the solid contents of the bacteria, which seem to be actually broken up, when examined microscopically after exposure to the ultra-violet light. Perhaps the production of electrically charged ions through the action of the short wave length radiation on the surface of the solid organisms, is a potent factor in their destruction at low temperatures. In any case, it seems the protoplasmic molecule cannot stand the internal strain produced by the vibrations set up in it by the action of the rays of short wave-length, and is thereby forced to re-arrange its atomic structure, causing the death of the organism.

These are only a few indications that there is still much low temperature chemistry to be worked out.

[J. D.]

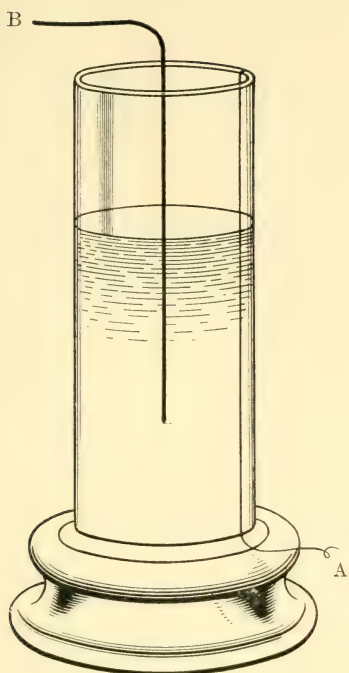


FIG. I.

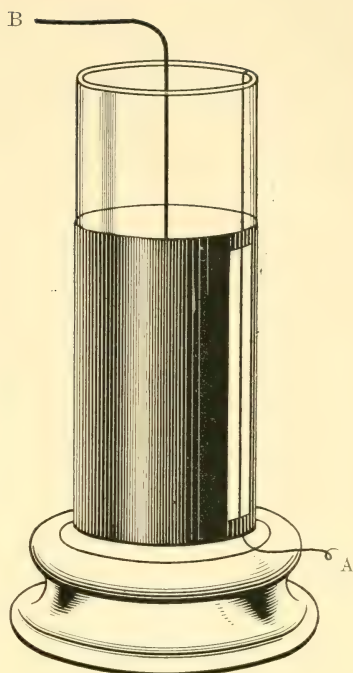


FIG. II.

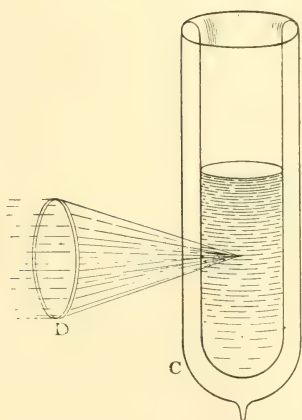


FIG. III.

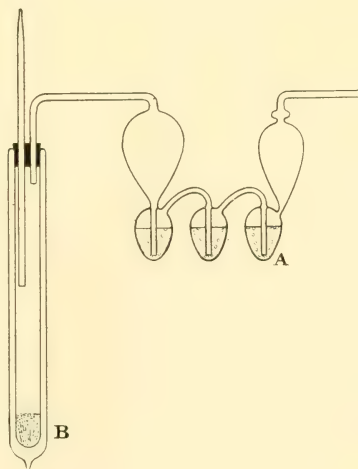


FIG. IV.

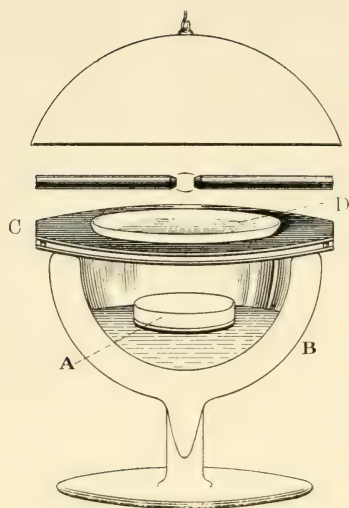


FIG. I.

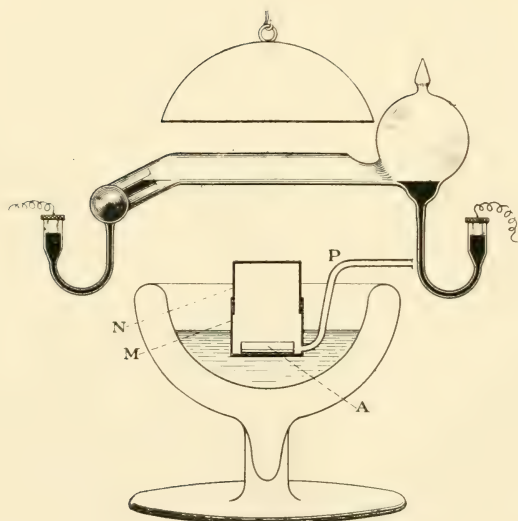
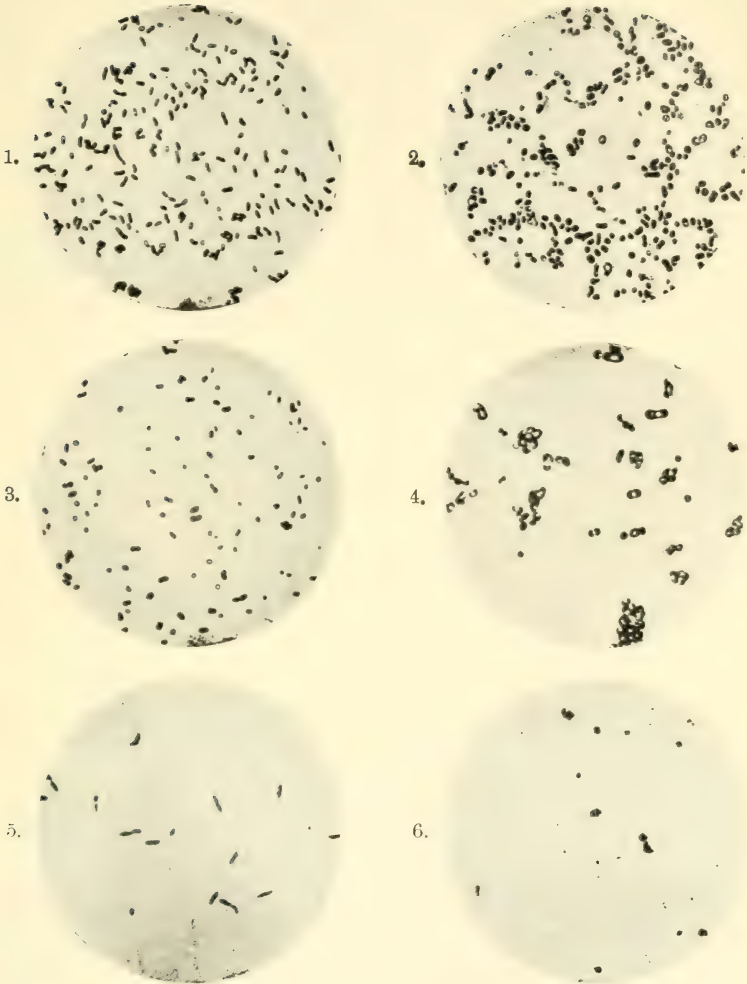


FIG. II.



PHOTOGENIC BACTERIA.
(*Photobacterium Phosphorescens.*)

GROWTH ON SOLID MEDIA.

- Fig. 1. Fresh culture, ordinary gelatine.
 „ 2. Three days' growth.
 „ 3. One day's growth, salted gelatine.
 „ 4. Old culture, salted gelatine.

GROWTH IN FLUID MEDIA.

- Fig. 5. Peptone sea-water.
 „ 6. Peptone sea-water, showing Flagellum.

FIG. I.—GROWTH OF TWO COLONIES IN DIAMETERS OF *B. PHOSPHORESCENS* REPRESENTED GRAPHICALLY.

- (I) In exhausted sealed tube for 13 days, then opened to the air.
 (II) In free air for 15 days, then exhausted.

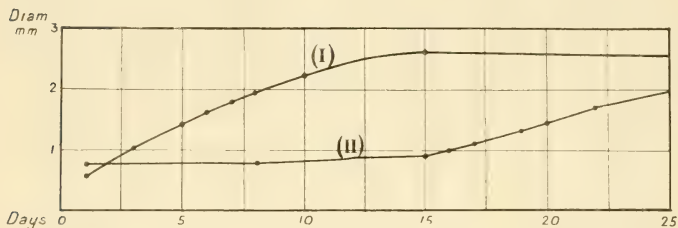
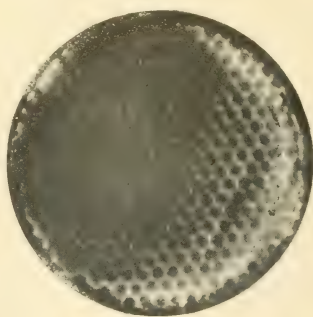


FIG. II.—REPRODUCTION OF PHOTOGRAPHS taken by light emitted by Bacteria after the action of ultra violet light on them at the temperature of liquid air and subsequent heating to the ordinary temperature.

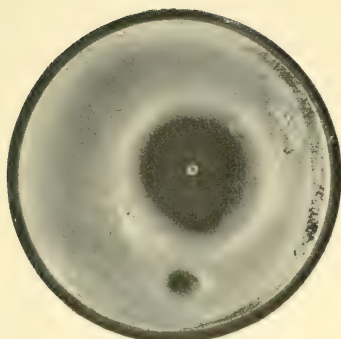


1.



2.

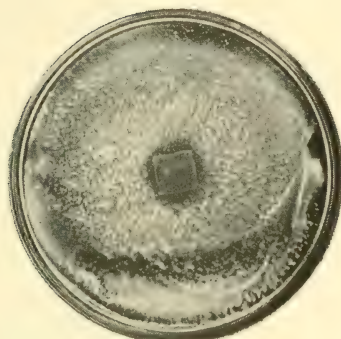
1. Round tin plate with cut out cross, bacteria killed where unscreened.
 2. Perforated zinc, showing same effect as above.



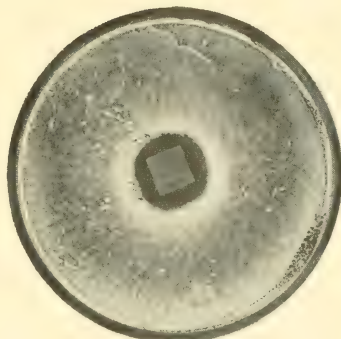
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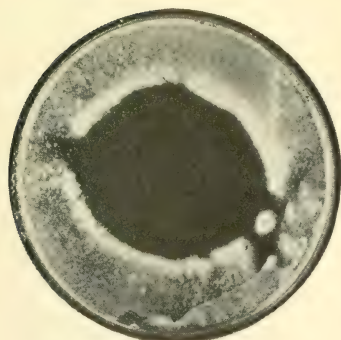
2.



3.



4.



5.



6.

1. Mercury,
2. Antimony,
3. Zinc.

4. Silver,
5. Chloride of Silver.
6. Sulphur.

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OF THE

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